Universidade Federal de Pernambuco
Centro de Ciências Sociais Aplicadas
Departamento de Ciências Econômicas

Pós Graduação em Economia

TRÊS ENSAIOS SOBRE OS EFEITOS ECONÔMICOS DO HORÁRIO DE VERÃO UTILIZANDO REGRESSÃO DESCONTÍNUA

WEILY TORO MACHADO

Tese de Doutorado

Recife
Abril 2016

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Trabalho apresentado ao Programa de Pós Graduação em Economia do Departamento de Ciências Econômicas da Universidade Federal de Pernambuco como requisito parcial para obtenção do grau de Doutor em Economia.

Orientador: *PROF. Dr. BRENO RAMOS SAMPAIO*

Recife
Abril 2016

Catalogação na Fonte
Bibliotecária Ângela de Fátima Correia Simões, CRB4-773

M82t Toro, Weily Machado
Três ensaios sobre os efeitos econômicos do horário de verão utilizando regressão descontínua / Weily Toro Machado. – 2016.
82 folhas: il. 30 cm.

Orientador: Prof. Dr. Breno Ramos Sampaio
Tese (Doutorado em Economia)– Universidade Federal de Pernambuco, CCSA, 2016.
Inclui referências.

1. Economia da saúde. 2. Crime e condições climáticas. 3. Horário de verão. 4. Política de saúde. 5. Segurança pública. I. Sampaio, Breno Ramos (Orientador). II. Título.

336 CDD(22.ed.) UFPE(CSA 2016-060)

UNIVERSIDADE FEDERAL DE PERNAMBUCO
CENTRO DE CIÊNCIAS SOCIAIS APLICADAS
DEPARTAMENTO DE ECONOMIA
PIMES / PROGRAMA DE PÓS GRADUAÇÃO EM ECONOMIA

PARECER DA COMISSÃO EXAMINADORA DE DEFESA DE TESE DO DOUTORADO EM ECONOMIA DE:

WEILY TORO MACHADO

A Comissão Examinadora composta pelos professores abaixo, sob a presidência do primeiro, considera o Candidato Weily Toro Machado **APROVADO**

Recife 26/04/2016.

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A DEUS e a minha família. Esposa: Lucélia. Filhos Eduardo e Gabriel. Pais: Sergio e Neusa. Irmãos: Wender e Wenderley. Cunhadas: Ozélia, Girlane, Laudiana, Laudicéia e Leiliane. Sobrinhos: Wesley, Heminy, Gabriely e Wender Filho. A vocês dedico este trabalho.

Acknowledgements

Ao Supremo Arquiteto do Universo (nosso DEUS) que nos abençoa em tudo que fazemos, a ti todos os louvores SENHOR.

A minha amada princesa, esposa (Lucélia), companheira, amiga, que sempre acreditou que tudo isso era possível, sua compreensão e apoio foi e é fundamental para que eu conseguisse meus objetivos.

Ao meu amado filho Eduardo (de 7 anos) que sempre cobrou o final desse trabalho para que eu possa brincar mais com ele depois do término, papai TE AMA.

Ao Gabriel (de 11 meses) que chegou ao longo dessa caminhada para alegrar nossos corações e nos dar mais forças para chegarmos ao final, com ele pude aprender como é a multiplicação do AMOR de pai para filho.

A minha Mãe minha professora de primário que me ensinou a ler, escrever e sempre esteve ao meu lado com suas orações.

Ao meu Pai um homem de coragem, lutador e destemido, sempre me encorajou a enfrentar as coisas mais difíceis da vida.

Ao meu irmão Wenderley que sempre vibrou com minhas conquistas e sempre, sempre esteve preocupado e torcendo para que tudo desse certo, receba minhas reverências.

Ao meu irmão Wender que ao seu modo contribuiu para que eu possa conquistar mais esta vitória, nunca esquecerei o que você fez por mim.

As minha cunhada Ozélia, amiga de todas as horas que sempre me apoio. A minha cunhada Girlane que sempre acreditou em meus trabalhos.

Aos Sobrinhos Wesley, Heminy, Gabriely e Wender Filho, um grande abraço desse velho TIO.

A minha tia Luzia (in memorian) e a meu tio Waldemar que me ajudaram nos tempos de faculdade, obrigado por tudo.

A Aninha que acompanhou de perto toda essa jornada, e sempre foi uma grande tia para meus filhos, obrigado pela ajuda. A Laudicéia e Leiliane que sempre vibraram com minhas conquistas, valeu cunhadas. Ao senhor José Cicero e Dona Eliza, obrigado pelas orações e pela torcida, valeu mesmo.

Ao professor e meu eterno orientador Breno Sampaio que sempre me desafiou para que pudesse crescer enquanto pesquisador, suas cobranças valeram a pena, muito obrigado por tudo, de agora em diante tudo será diferente, vejo as pesquisas com um olhar bem mais crítico.

Aos amigos do doutorado, que me ajudaram muito, muito e muito. Em especial, Anderson, Cleiton, Karine, André (valeu pelos trabalhos que fizemos juntos, obrigado pela força que sempre me deram), Wylmor, Carlo Eduardo, Feliciano, Ademir, Paulo e Fábio meu eterno coorientador e o primeiro DOUTOR da turma. A Cleiton por juntos termos começado a estudar sobre o Horário de Verão nos acidentes de trânsito, tudo começou aqui, valeu mesmo.

Um agradecimento especialíssimo ao professor coordenador do DINTER Dr. Arturo Alejandro Zavala Zavala, seu trabalho e dedicação foi fundamental para que pudéssemos chegar a realizar este grande sonho.

As professores do PIMES Álvaro Barrantes Hidalgo, Yony de Sá Barreto Sampaio, Nelson Leitão Paes, Alexandre Stamford da Silva, Tatiane Almeida de Menezes, Roberta de Moraes Rocha, Andrea Sales Soares de Azevedo Melo, Sarmento, Raul da Mota Silveira Neto, André Matos Magalhães, Francisco da Silva, Gustavo Sampaio.

Aos amigos do curso de Ciências Contábeis da Unemat/Cáceres, Aldo, Almir, Juliana e Ricarte, valeu pela torcida.

As instituições UNEMAT, UFMT, UFPE, CAPES e a seus dirigentes e colaboradores que nos acolheram, trabalharam e não mediram esforços para que tudo acontecesse dentre da mais perfeita ordem.

Aos amigos Weudes e Patricia, Renato e Tais, Bruno e Libna e aos pequenos David e Pedro do grupo de oração "Amor em Cristo", nossas orações valeram a pena.

A minha amiga e Reitora da Unemat Ana Di Renzo que sempre apoio e torceu para a chegada deste momento.

Aos mestres desde a minha primeira professora (minha mãe) que me ensinou a ler e escrever na escola Monteiro Lobato no sítio do meu pai. Também não poderia deixar de agradecer a todos os professores do ensino fundamental e médio da escola Estadual Padre José de Anchieta de Lambari D' Oeste-MT. Aos professores de graduação da UNEMAT/Cáceres-MT onde fiz a graduação em Contabilidade. Ao programa de Mestrado da Universidade Autônoma de Assunção no Paraguai, onde fiz o Mestrado em Administração. Chego ao topo da titulação acadêmica, e essa trajetória não poderia ser esquecida nesse momento. Agradeço os amigos e parceiros que fiz ao longo desse caminho.

Aos coautores Robson Tigre, Guilherme Amorim e Lucas Silva, pude aprender muito com vocês.

Em fim, a DEUS e a todos.

“DEUS NÃO DEMORA ELE CAPRICHA.”
Caio Fernando Abreu
“A MENTE QUE SE ABRE A UMA NOVA IDEIA JAMAIS VOLTA AO SEU TAMANHO ORIGINAL.”
Albert Einstein
—

Resumo

Este estudo é dividido em três capítulos. Usando dados diários para os estados Brasileiros e técnicas de regressão descontínua, verificamos a influência de um experimento natural, induzido pelo Horário de Verão (HV), no cotidiano das pessoas através de dois canais: (a) distúrbios do sono e (b) luminosidade. As seguintes variáveis de interesse são analisadas: 1) Internações hospitalares decorrentes de complicações relacionadas a diabetes mellitus; 2) Homicídios provocados por arma de fogo; 3) Mortes por infarto agudo do miocárdio. No primeiro capítulo, utilizando dados do Sistema de Informações Hospitalares (SIH/DATASUS), do Ministério da Saúde do Brasil, exploramos o impacto da privação do sono, provocada pelo HV, sobre as internações de indivíduos que tem Diabetes Mellitus (DM). O estudo traz fortes indícios de que a entrada do HV aumenta internações por DM em 6-8% nos estados adotantes da política, enquanto nenhuma variação ocorre nos estados não tratados. No segundo capítulo, usando dados do Sistema de Informação de Mortalidade (SIM/DATASUS), analisamos o impacto dessa política sobre o número homicídios. Encontramos evidências robustas em favor de uma redução em torno de 14% no número de homicídios nos estados tratados. Este efeito se concentra principalmente em horas que antes do HV eram escuras e após a transição passaram a ser claras. Novamente, as estimativas para os estados não tratados não apresentam nenhuma significância. No terceiro capítulo, também com dados do SIM/DATASUS, analisamos o efeito do HV sobre mortes decorrentes de infarto agudo do miocárdio. Nos Estados que adotam a política, há um aumento de 7-8,5% no número dessas mortes, e nenhuma relação estatística para os estados que não adotam a política.

Palavras-chave: Economia da Saúde; Avaliação de Políticas Públicas; Criminalidade; Desenho de Regressão Descontínua.

Abstract

This thesis consists of three chapters. Using daily data for Brazilian states and regression discontinuity techniques, we assess the impact of a natural experiment, induced by Daylight Saving Time (DST), on people's daily life through two channels: (a) sleep disturbances, and (b) ambient light. The following outcomes are studied: 1) Hospital admissions due to complications related to diabetes mellitus; 2) Homicides caused by fire arms; 3) Deaths due to myocardial infarction. In the first chapter, using data from Sistema de Informações Hospitalares (SIH/DATASUS), provided by the Brazilian Health Ministry, we assess the impact of sleep deprivation, caused by DST transition, on the number of hospital admissions related to Diabetes Mellitus (DM). The study provides credible findings that the DST entrance transition increases this type of hospital admission in 6-8% in the states that adopt DST while no significant effect is found in the states that do not adopt the policy. In the second chapter, using data from Sistema de Informação de Mortalidade (SIM/DATASUS), we analyze the impact of this policy on the number of homicides caused by fire arms. Robust evidence of a reduction in the number of homicides, of around 14%, is presented for the treated states. This effect is concentrated especially in hours that were dark before DST and turned to be illuminated after the transition. Again, estimated effects for non-treated states are not statistically different from zero. In the third chapter, which also exploited data from SIM/DATASUS, we analyze the effect of DST transition on deaths due to acute myocardial infarction. There is a 7-8.5% increase in the number of deaths due to this cause in the treated state and no statistically significant change for the untreated states.

Keywords: Health Economics; Public Policy Evaluation; Crime; Regression Discontinuity Design.

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CHAPTER 1

SLEEP DEPRIVATION AND DIABETES: REGRESSION-DISCONTINUITY APPROACH[1]

1.1 Introduction

In this paper we investigate the unidirectional effect of short-term sleep deprivation on patients with Diabetes Mellitus, using daily data from the public health care system in Brazil over a period of five years. We use the natural experiment induced by Daylight Saving Time as an exogenous shock to sleeping patterns and analyse its impact on figures for hospitalization, total hospital expenses and mortality of patients affected by this disease in Brazilian public hospitals.

In modern economic theory, individuals are motivated by incentives when making choices and allocating personal resources. As the amount of time dedicated to sleeping is one of people's scarcest personal resources, it is natural to think of it as subject to choice and affected by the same economic variables that affect other uses of time - an idea first highlighted in Biddle and Hamermesh (1990). In this line, there are many evidences pointing to a strong negative correlation between sleep duration and income e (e.g. Ásgeirsdóttir and Ólafsson, 2015), which is an evident proxy for such economic incentives.

On the other hand, however, innumerable studies already established important associations between less sleeping and negative health outcomes such as higher incidence of chronic diseases, cancer, depression and early mortality.[2] This suggests that a trade-off between health and personal gains might be in place when individuals program their sleeping routines, which recently have been turning this into an important subject matter in the economics literature (e.g. Giuntella et

[1]This work has as coauthors Guilherme Amorim, Lucas Silva and Breno Sampaio.
[2]See e Cappuccio et al. (2010) for a systematic review.

al., 2015). Not without reason, in a larger scope, widespread incidence of sleep deprivation may ultimately impose high risks to human capital[3] and to productivity of an economy as a whole, pushing increasingly higher spendings to its health care system as a direct consequence.

For it is still widely unclear whether poor sleep prevalently causes or is caused by poor health, we propose to shed light on this relationship by studying its effects over patients with Diabetes Mellitus (DM, hereafter). DM is a condition that encompasses a group of similar metabolic disorders, each caused by a complex iteration of genetic and environmental factors, and is characterized by the phenotype of hyperglycaemia - that is, the presence of high levels of sugar in an patient's bloodstream (Fauci et al., 2008, p. 2275). In the course of the last two decades, its prevalence around the globe rose from approximately 30 million cases in 1985 to 177 million in 2000, and if the current trend persist, more than 360 million people are estimated to have DM in the year 2030 (Wild et al., 2004). Indicators for Brazil do not lag behind the global trend: it is estimated that the country will sustain its current 4th position in the list of countries most affected by the disease for the next twenty years, going from the current estimated prevalence of 9% of its population in 2013 to 11.7% in 2035 (Guariguata et al., 2014). These figures also reflect increasing costs to the Brazilian health care system since, according to the International Diabetes Federation (IDF), the average cost of a patient with DM in Brazilian hospitals in 2014 was $1,527.60.[4] In 2015, total expenditures for DM treatments in Brazil summed up to roughly $22 billion.[5]

An important part of the follow-up care for DM consists in introducing improvements to lifestyle habits such as reformulating diet composition, increasing physical activity and losing weight - especially in the case of Type 2 DM, which comprises 90% of registered cases of DM today and is largely an outcome of an individual's excess body weight and physical inactivity.[6] Its high prevalence and close link with patients' personal habits have also made DM a subject of interest in recent economics literature (e.g. Oster, 2015) and, in order to explore new treatment routines and additional prevention strategies, much effort have been recently made in the medical literature to investigate other external risk factors for DM. Understanding the influence that sleep

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[3] For a recent empirical evaluation on this topic, see Jin et al. (2015).

[4] Source: `https://www.idf.org/membership/saca/brazil`. Retrieved 26 December, 2015.

[5] IDF Diabetes Atlas, 7th edition, 2015. Available at `http://www.diabetesatlas.org/`, retrieved 26 December 2015

[6] Diabetes Fact sheet No. 312. WHO. January 2015. Retrieved 12 November 2015.

impairment may exert on preexisting DM condition has been an important part of that effort.

Evidence that restrictions on sleep quality may adversely influence the risk of DM have been documented in various empirical studies for broad classes of DM (e.g. Gottlieb et al., 2005) and most notably for Type 2 DM M (e.g. Ayas et al., 2003; Spiegel et al., 2005; Reichmuth et al., 2005; Yaggi et al., 2006; Gangwisch et al., 2007). More recent studies have also narrowed the scope of the investigation of its effect on patients with Type 1 DM (e.g. Donga et al., 2010; Borel et al., 2013) and gestational DM (GDM) (e.g. Luque-Fernandez et al., 2013; Reutrakul et al.,2013). Seeking to describe the prospective effects of poor sleeping on DM, most of this research has focused on the long-term relationship between the two, by following patients with different assessments of sleep quality over extended periods of time and comparing changes in their levels of fasting glucose and/or glycated hemoglobin.[7] Some of the unveiled mechanisms behind this relationship include alterations in glucose metabolism, unregulation of appetite and decreased energy expenditure (Knutson et al., 2007; Tasali et al., 2009). The unidirectional effect of short-term sleep deprivation on DM, however, is a relationship that remains little explored so far.[8]

Patients affected with DM require frequent contact with the health care system for effective management and prevention of complications (Chaput et al., 2009). We interpret that any shock that may affect stability of patients with DM should affect hospital admissions for that same condition in that specific day (and possibly some of the following days owing to residual effects), which in turn should affect figures in health care costs and mortality due to DM. However, since the design of a proper randomized experiment is unfeasible in this context, mostly owing to legal and/or ethical constraints, our approach will be to use an identification strategy in order to emulate such a shock and vouch for causal interpretations: we explore the natural experiment induced by Daylight Saving

[7] One important drawback, however, is that most of this empirical work have so far relied on methods that strongly depend on almost ideal unconfoundedness conditions. One very frequent approach, for example, is the calculation of odds-ratio for changes in DM indicators given a measurement of average sleep quality, using probit/logit estimators. In addition, assessment of quality and/or number of hours of sleep have been widely conducted through self-reporting in field questionnaires, which can lead to problems such as mismeasurement and selection bias on the variables of interest - a weakness generally acknowledged by the authors themselves. These considerations, when taken to the worst scenario, may severely compromise the estimates' accuracy and are strong caveats against validity of those results.

[8] Donga et al. (2010), for example, show that partial sleep deprivation during only a single night induces insulin resistance in multiple metabolic pathways in healthy subjects, which may be of relevance for variations in glucoregulation in patients with Type 1 and Type 2 DM.

Time policy (hereafter, DST) as an alternative potentially as good as randomization to identify the effect of interest (Imbens and Lemieux 2008; Angrist and Pischke 2014). Our aim is to provide a well-founded and accurate estimate of the impact of one hour less of sleep in one single night on the risk of developing or aggravating DM in a population, using a regression discontinuity design to assess the effect that the transition to DST have on the increase/decrease of hospital admissions, health care costs and mortality for this specific condition in Brazil.

Our results show that, in states that adopt DST policy, transition to DST increases hospital admissions for DM in around 6% to 8% between specifications, while no effect is observed in states that do not adopt DST policy. We also observe no effect of leaving DST in DM hospitalization in any state. These results are shown to remain consistent when our sample in decomposed in macroregions. Age and gender decomposition show that this increase in hospitalization is mostly evident in the male population above sixty years of age. Placebo tests further confirms that there is no effect of entering DST on hospital admissions for diseases unrelated with DM, and that figures for hospitalization, costs and mortality for DM on every other day of the year other than on DST transition are generally not susceptible to short-term spikes. We find that transition to DST also increases health care expenses for DM in around 18.9% and, more importantly, increases mortality of patients with DM in around 8.5%. These estimates imply a rise in health care expenses of around $3 million and reflect a total of 155 deaths at a social cost of $.62-1.55 billion over the 5 year sample period we consider.

Our work mainly contributes to the literature that explores different factor risks for the different types of DM, particularly to the branch that explore how different forms of sleeping disorders can affect patients' conditions with DM. Our results extend the scope of investigation by evaluating a mechanism scarcely discussed with empirical evidence so far: the short-term, unidirectional effect of sleep deprivation on DM. In addition, this paper is also novel for its methodological approach since, to our knowledge, it is the first in this literature to make use of robust techniques in causal inference, allowing us to tackle important confounding issues such as endogeneity, reverse causality and omitted variables bias. Our results are so the most robust and reliable so far in terms of assessment of the effect of interest.

This paper is also important for more general discussions in policy design, contributing to the collection of empirical assessments on the several costs and benefits associated with an institutional change such as the implementation of DST. This policy is applied in many countries with the purpose to benefit activities that exploit sunlight after regular working hours and to have a direct impact on energy consumption by reducing the need for lighting during the day (the actual effect on overall energy use is heavily disputed). However, unintended consequences have been drawn in several other spheres such as student achievement (Wong, 2012), vehicle crashes (Smith, 2016), criminal activity (Doleac and Sanders, 2015) and individual well-being (Kountouris and Remoundou, 2014). Regarding its impact on health indicators, both positive and negative effects have been documented, such as the increase in outdoor recreational activities (Wolff and Makino, 2012) and the increase in incidence of acute myocardial infarction (Toro, Tigre and Sampaio, 2015). In our case, indicators are worsened with the rise in DM hospitalization, health care expenses and mortality.

The remainder of the paper is organized as follows. In section 1.2 describes structure and institutional framework of DST in Brazil, while sections 1.3 present the data set. In section 1.4 we present our methodological approach. Section 1.5 discusses the results. Finally, conclusions are presented in section 1.6.

1.2 Daylight Saving Time in Brazil

DST concerns the practice, adopted in certain countries and regions, of advancing clocks on standard time by one hour during the summer months in order to reap the potential benefits of a longer-lasting sunlight period during the day. It was first implemented as a nationwide policy in Germany and former Austria-Hungary in 1916, having since then being adopted by various other countries over several different times, particularly after the energy crisis of the 1970s.[9] In the present year of 2016, this policy will be observed in 76 countries around the world, affecting the lives of more than 1.5 billion people.

In Brazil, DST has been adopted every year since 1986, with adopting periods (and regions) administered by means of Federal enactments based on information of technical reports provided by The Electric System National Operator (ONS). The National Operator indicates which states

[9] A review of the origins, early adoptions and further discussion on DST is presented in Aries and Newsham (2008).

should adopt DST as well as the duration of the regime, which usually starts on the third Sunday of each October, when clocks skip forward from 12am to 1am, and extends to midnight of the third Sunday of each February.

Since this policy is grounded in the variation of daily sunlight during summer solstice, and given the country's geographical feature of having a wide longitudinal extension, DST implementation does not provide nominal benefits for states close to the Equator line, which leads to variation in the treatment status across the country (see Figure 1.1). This favors our identification strategy since it provides variation in DST adoption both between (i.e., adopters vs. non-adopters) and within states (i.e., among those that adopt; standard time vs. DST). Having non-adopter states helps us in designing robust placebo tests, given that other factors affecting DM, besides DST, must evolve smoothly around the transition date.[10]

Figure 1.1: DST policy in Brazil

Note: States in black (RS, SC, PR, SP, RJ, ES, MG, GO, MS, MT, DF) adopted DST from 2008 to 2012 and together constitute the Midwest, Southeast and South macroregions. States in grey adopted DST in only one of the years between 2008 and 2012 (BA in 2011 and TO in 2012). States in light grey did not adopt DST between 2008 and 2012.

Between 2008 and 2012, all states within Midwestern, Southern and Southeastern region, where light incidence vary the most during the year, adopted DST. Bahia (Northeastern region) and

[10]Doleac and Sanders (2015); Smith (2016), for example, consider law changes to DST policy in the US to account for endogeneity, since DST occurs simultaneously across 48 states (Arizona and Hawai do not observe DST) and at approximately the same time each year.

Tocantins (Northern region) adopted DST only in 2011 and 2012, respectively[11] (see Table 1 for a detailed list of adopters by each year). Therefore, we have 10-12 states adopting DST every year, the treated states, and 14-15 remaining untreated during our sample period.

Table 1: Brazilian states that adopted DST from 2008 to 2012

Entry Date	Exit Date	Adopting states
19/10/2008	15/02/2009	RS, SC, PR, SP, RJ, ES, MG, GO, MS, MT, DF.
18/10/2009	20/02/2010	RS, SC, PR, SP, RJ, ES, MG, GO, MS, MT, DF.
17/10/2010	19/02/2011	RS, SC, PR, SP, RJ, ES, MG, GO, MS, MT, DF.
16/10/2011	25/02/2012	RS, SC, PR, SP, RJ, ES, MG, GO, MS, MT, DF, BA.
21/10/2012	16/02/2013	RS, SC, PR, SP, RJ, ES, MG, GO, MS, MT, DF, TO.

Note: data from Observatório Nacional do Ministério de Ciências e Tecnologias. http://pcdsh01.on.br/DecHV.html

1.3 Data

We use individual-level data on hospitalizations from the SUS Hospital Admissions System (*Sistema de Internações Hospitalares* - SIH-SUS). This system, managed by the Brazilian Ministry of Health, is the government's official registry to every patient admission in Brazilian public hospitals, covering virtually all of the country's territory. It contains daily information on the causes of admissions following the International Classification of Diseases (ICD-10), along with several other important variables such as the incurred cost of every hospitalization to the health care system and whether the patient died following its hospitalization.

To ensure the best reliability of our data, we consider the years from 2008 to 2012, the last available year. During this period, there were 713,149 registered hospitalizations due to DM as identified by codes E100 to E149 in the ICD-10. As of 2012, each case should fall in one out of four available categories: insulin-dependent diabetes mellitus (E10), non-insulin-dependent diabetes mellitus (E11), malnutrition-related diabetes mellitus (E12), other specified diabetes mellitus (E13) and unspecified diabetes mellitus (E14).[12] the final aggregation accounts all forms of DM (brittle

[11] Results are unchanged if we exclude these two states.

[12] The expressions "insulin-dependent diabetes mellitus" and "non-insulin-dependent diabetes mellitus" have eventually worn out and became obsolete, being traded, respectively, for "type 1 diabetes mellitus" and "type 2 diabetes

and stable; ketosis-prone and nonketotic; juvenile-, adult- and maturity-onset; type 1 and type 2; and malnutrition-related DM) and excludes similar symptoms and disorders associated with DM but not formally classified as such (for example, glycosuria, impaired glucose tolerance, and DM in pregnancy, childbirth and/or the puerperium).[13]

The dependent variables in our study are based on the number of hospitalizations, the total amount of hospital expenses and the number of deaths caused by DM on a particular day and in a particular state and year. We aggregate data to the state level bearing two reasons in mind. First, the daily frequency of these hospitalizations is very small in disaggregated levels (for example, at county level). Second, aggregation allow us to gain statistical power and to smooth out other factors of potential bias, such as climatic conditions that could have affected hospital admissions at the municipal level but were unlikely to have affected them statewide. Also, following a procedure by Janszky and Ljung (2008) and also carried out in Smith (2016), we multiply the number of hospital admissions on the first and last days of DST by 24/23 and 24/25, respectively, to account for a possible distortion coming from the fact that the first day after the transition to DST ends up being one hour shorter than the rest of the days in a year (23 hours) and the first day after transition from DST to ST ends up being one hour longer (25 hours).[14]

In table 3 we present average number of hospital admissions, average health care expenses and average mortality, per day, unadjusted for day-of-week and time trend, for one week prior and one week after DST transition. We note that for states that adopted the policy, there are on average 21.797 hospital admissions per day per state on the week prior to transition to DST. On the week following the transition, this number increases to 23.079, an increase of almost 6%. This pattern is not observed when looking at states that did not adopt the policy, which present an increase of only 1.3%. This behavior is also observed when looking at average health care

mellitus" in the 2014 version of ICD-10. Fauci et al. (2008, p. 2276) note that, since many individuals with Type 2 DM will eventually need insulin treatment in order to control blood glucose levels, the previous classification have led to many confusion among practitioners. It is at least a curious fact that 57.16% of our database are within the categories of "other specified" and "unspecified diabetes mellitus". We choose, therefore, not to draw evaluations over different types of DM by disaggregating our data in the different DM categories.

[13] A more detailed list with the ICD-10 codes for DM is provided in the appendix.

[14] In table A1, presented in the appendix of this paper, we provide evidence that our results are not driven by these hour adjustments. In comumns 1 and 2, we run estimates of the impact of DST on DM hospitalization without using the procedure proposed by Janszky and Ljung (2008). In columns 2 and 4, we exclude the transition date (day one after transition) altogether. In both cases, results are qualitatively identical to our main specification, presented in table 3.1.

expenses and mortality. States that adopt the policy present an increase in health care expenses and mortality of, respectively, 12.6% and 11.8%. Theses numbers for untreated states are 8.8% and -1.3%, respectively.

Table 2: Average number of hospital admissions, health care costs, and mortality, per state for one week before and one week after Daylight Saving Time

State	Hospitalization		Health care costs		Mortality	
	Week Pre-DST (1)	Week Post-DST (2)	Week Pre-DST (3)	Week Post-DST (4)	Week Pre-DST (5)	Week Post-DST (6)
Treated	21.797	23.079	12,500.61	14,080.44	0.925	1.034
	(19.435)	(19.971)	(13,146.15)	(16,702.33)	(1.387)	(1.365)
Untreated	9.418	9.547	4,477,20	4,873.74	0.471	0.465
	(9.733)	(9.773)	(4,540.67)	(5,806.15)	(0.819)	(0.883)

Note: Standard deviations are in parentheses.

1.4 Empirical Strategy

In this section we present the empirical strategy we adopt to identify the causal effect of short-term sleep deprivation on the risk of DM, measured by shifts in hospitalization, mortality and health care costs with DM. In particular, we use transitions from standard time to DST as an exogenous shock to sleep and compare the number of diabetes outcomes on the day before entering DST with the number of diabetes outcomes on the first day after its initiation for states that adopted DST using a regression discontinuity (RD) design. For that, consider the following reduced-form model

$$\ln Diabetes_{isy} = \tau I(Transition_{isy} \geq 0) + g(Transition_{isy}) + \varepsilon_{isy} \tag{1.1}$$

where $\ln Diabetes_{isy}$ is the natural logarithm of the number of diabetes hospitalizations, health care costs or mortality in day i, state s and year y, $Transition_{isy}$ is defined as the number of days to transition to DST, which is equal to zero on the first day after transition and is positive (negative) after (before) then, g is a non-parametric function and ε is a random term. To eliminate persistent day-of-week effects (it might be the case that the number of diabetes hospitalizations is higher on weekends than weekdays, for example), state differences and long-term time trends, we follow Smith (2016) and Toro, Tigre and Sampaio (2015) and demean the log of number of diabetes hospitalizations, mortality and costs by day-of-week, state and year.

We utilize local-polynomial regression-discontinuity point estimators with robust bias-corrected non-parametric confidence intervals, provided by Calonico, Cattaneo and Titiunik (2014). Instead of selecting ad-hoc bandwidths, we rely on two optimal data-driven bandwidth selectors outlined in Imbens and Kalyanaraman (2012), hereafter IK, and Calonico, Cattaneo and Titiunik (2014), hereafter, CCT.

In the context described above, consistently estimating our parameter of interest requires that conditional on day-of-week, state and year fixed effects, the outcomes must evolve smoothly around the transition date in the absence of treatment, i.e., observed and unobserved covariates should vary continuously around the cutoff. Given our institutional setup, we have a well defined control group, namely untreated states, to test if other unobserved factors, correlated with DST transition, are in some way responsible for shifts in the number of diabetes hospitalizations, mortality or costs other then DST itself.

In addition to this falsification test done to all our three dependent variables, we consider two additional placebo tests when analysing hospitalization. First, we use other diseases that in principle should not be affected by the transition to DST and hence should not respond to treatment. Secondly, we check for causality in the spirit of Granger (1969) and estimate the coefficients of pre-treatment and post-treatment effects, a common test in the Differences-in-Differences framework to provide robustness to the results (see Autor, 2003). We assign, therefore, $I(Transition_{isy} \geq 0)$ for days preceding and following the actual DST transition. If our identifying hypothesis holds, we expect leads and lags to have no statistical relevance in explaining shifts to DM outcomes.

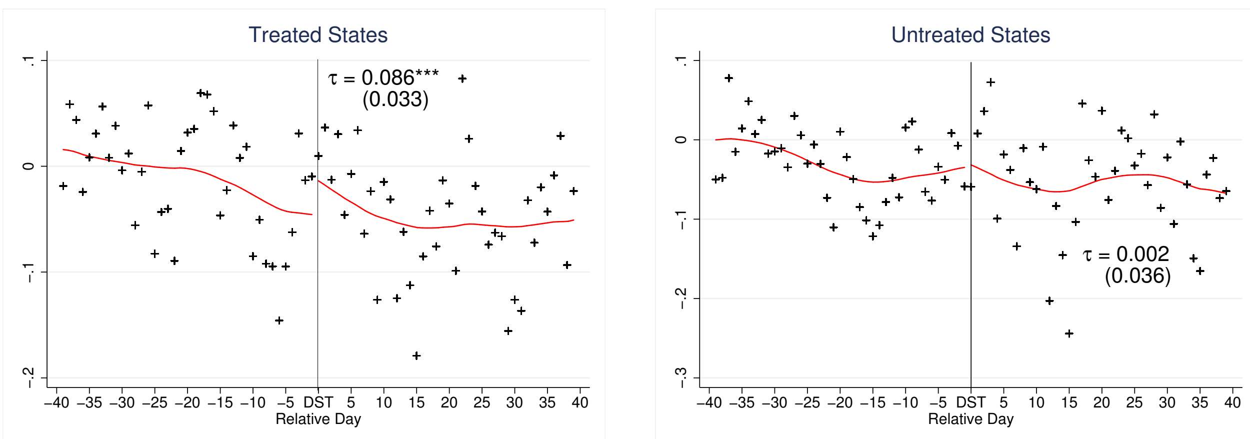
1.5 Results

Our results are divided into three parts. First we investigate if sleep deprivation induced by DST causes DM hospitalizations to increase, conducting more thorough evaluations in this part. Second we check whether health care costs increase after transition and if this sudden disturbance to sleeping patterns is sufficient to change mortality figures on patients that were hospitalized for reasons related to DM. At last we conduce placebo tests and other robustness checks to each of our three main outcomes.

1.5.1 Results on hospitalization

Figure 3.1 presents our main results graphically. Demeaned values of $\ln Diabetes$ by day-of-week, state and year are plotted, centered on the DST transition date. The graph on the left, which consider states that adopted DST, shows that points to the right of the cutoff are slightly shifted above, implying higher incidence of DM hospitalization after transition. This is not observed when looking at untreated states, in which we see no discontinuity around the cutoff point. Note that for untreated states, we consider as if DST was adopted in the same time period as treated states.[15]

Figure 1.2: DST entrance transition - residuals plot



Note: Residuals are generated from a regression of ln(diabetes) on day-of-week, State and year dummies. Fitted lines represent locally weighted regression.

In table 3 we present formal results considering both bandwidth selection procedures for treated and untreated states (CCT and IK). In columns 1 through 4, we present estimates for the effect of entering DST on DM hospitalizations for treated and untreated states. Results presented in columns 1 and 2 imply that, after transition, DM hospitalizations increased by about 6.2 to 8.6% in treated states. In untreated states, results are precisely zero (columns 3 and 4). This reinforces

[15] In this case, we estimate equation 3.1 using data on $\ln Diabetes_{isy}$ for untreated states.

our claim that we uncover the causal effect of sleep deprivation on DM hospitalization since, as mentioned in the previous section, unobservables are likely to be balanced near the threshold, given that outcomes for treated states are likely to be influenced by the same unobservables that determine outcomes for states not affected by the policy.

Still in table 3, results in columns 5 through 8 show that leaving DST exerts no significant effect on DM hospitalizations, in neither treated nor untreated states. Our results therefore support the hypothesis that sleeping more than in regular routine does not increase nor reduce the risk of developing or aggravating DM condition, as we assume that the addition of one extra hour during nighttime would reflect an increase on sleeping time for citizens in treated states.

Table 3: RD estimates of the impact of DST on DM hospitalizations for both treated and untreated states

	Entering				Leaving			
	Treated		Untreated		Treated		Untreated	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DST_{LATE}	0.086***	0.062**	0.002	-0.017	0.054	-0.004	0.017	-0.037
	(0.033)	(0.030)	(0.036)	(0.031)	(0.036)	(0.044)	(0.044)	(0.032)
Bandwidth selector	CCT	IK	CCT	IK	CCT	IK	CCT	IK
Bandwidth	12	26	18	31	14	49	11	33
Obs. to the left	684	1,482	1,326	2,418	798	2,727	858	2,574
Obs. to the right	741	1.539	1,404	2,496	855	2,850	936	2,652
Total	1,425	3,021	2,720	4,914	1,653	5,577	1,794	5,226

Note: CCT refers to the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012); IK is Imbens and Kalyanaraman (2012). All specifications use a first order polynomial and a uniform kernel. Robust Standard errors in parentheses. ***, ** and * represent p<1%, p<5% and p<10% respectively.

Our next steps will be to investigate the uncovered effect over different forms of decompositions in our data. Also, from here on we will be presenting our results considering only the CCT bandwidth selector, for it is to this date the most robust theory-based mechanism for calculating confidence interval estimators of local average treatment effects in RD designs.[16] In table 4, we separately estimate the impact of DST on DM hospitalizations on each of the five geographical regions in Brazil. The estimated treatment effects are consistent for the Midwest, Southeast and South regions (columns 1 through 3), which comprise together all of the adopting states of DST

[16] See Calonico, Cattaneo and Titiunik (2014). All of the following results remains qualitatively unchanged when using IK as optimal bandwidth selector and are available upon request.

policy. For the North and Northeast regions (columns 4 and 5), the effects are null, as expected.

Table 4: RD estimates of the impact of entering DST on incidence of diabetes mellitus decomposed per geographic regions

	Treated			Untreated	
	Midwest	Southeast	South	Northeast	North
	(1)	(2)	(3)	(4)	(5)
DST_{LATE}	0.112*	0.144***	0.167***	-0.013	0.018
	(0.059)	(0.045)	(0.056)	(0.041)	(0.051)
Bandwidth selector	CCT	CCT	CCT	CCT	CCT
Bandwidth	19	16	12	19	24
Obs. to the left	380	320	180	836	816
Obs. to the right	400	340	195	880	850
Total	780	660	375	1,716	1,666

Note: CCT refers to the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012). All specifications use a first order polynomial and a uniform kernel. Robust Standard errors in parentheses. ***, ** and * represent p<1%, p<5% and p<10% respectively.

In table 5, estimates for the impact of entering DST are discriminated by gender and age groups. Results show that DST impact on DM hospitalizations are mainly driven by the male population above 60 years of age, with an estimated increase of more than 20%, statistically significant at the 1% level (panel A, column 4), although positive coefficients are also observed for females between 21 and 40 years of age.

Table 5: RD estimates of the impact of entering DST on DM hospitalizations discriminated by age groups.

Age in years	0-20	21-40	41-60	60+
	(1)	(2)	(3)	(4)
	Panel A: Impact on male subjects			
DST_{LATE}	-0.037	-0.037	0.072	0.203***
	(0.035)	(0.038)	(0.045)	(0.050)
Bandwidth selector	CCT	CCT	CCT	CCT
Bandwidth	25	26	18	12
Obs. to the left	1,425	1,482	1,026	684
Obs. to the right	1,482	1,539	1,083	741
Total	2,907	3,021	2,109	1,425
	Panel B: Impact on female subjects			
DST_{LATE}	0.000	0.091**	0.048	0.063
	(0.042)	(0.042)	(0.045)	(0.048)
Bandwidth selector	CCT	CCT	CCT	CCT
Bandwidth	18	20	20	15
Obs. to the left	1,026	1,140	1,140	855
Obs. to the right	1,083	1,197	1,197	912
Total	2,109	2,337	2,337	1,767

Note: CCT refers to the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012). All specifications use a first order polynomial and a uniform kernel. Robust Standard errors in parentheses. ***, ** and * represent p<1%, p<5% and p<10% respectively.

This goes along with previous evidences that DM increase with age and is particularly prevalent in the male population with more than 60 years old (Fauci et al., 2008, p. 2277), and with evidence suggesting that older patients with DM are more often hospitalized than those without DM (Rosenthal et al., 1998). It serves also as an additional indicator that our sample is mostly formed of hospitalizations due to Type 2 DM, since "(it is) overwhelmingly the most common incident and prevalent type (of DM) in older age-groups," according to o Kirkman et al.(2012).

1.5.2 Results on health care costs and mortality

Alternatively to our previous specifications, another way to investigate whether short-term sleep deprivation poses as a risk factor for DM is to analyse the impact of transition to DST on total costs with DM hospitalizations. As discussed in the introduction, several statistical surveys have pointed to the connection between the growing prevalence of DM and its reflection in rising costs for the health care system in the long term. It is natural to suppose that such relationship would also hold in the short term, which might contribute as an additional evidence to the ones brought up by the results in hospitalization. We maintain our hypothesis that such impact, if shown to exist, is driven by the exogenous effect of DST transition on the available sleeping time of individuals in treated states, with every other relevant covariate varying smoothly under treatment assignment.

We measure the impact of transition to DST in daily costs with hospitalizations for DM in table 6, which reports RD treatment effects on treated and untreated states. Results show that the financial burden on DM treatment for the health care system increases by 18.9% in treated states, as shown in column 1. Given the average cost per day for treated states with DM treatment in our data is around R$754,509, back of the envelope calculations imply an increase of about R$143,357 per day. If the effect we estimate persists for the first 7 days after transition, costs increase in R$5,017,484 (around $2,918,562) within the first week of DST over the 5 year sample period. On states not affected by the policy, we observe no changes in hospital expenses, presented in column 2. Proceeding in a similar way as before, we show that there is no significant effect of leaving DST on hospital expenses in treated or untreated states.

Table 6: RD estimates of the impact of entering DST on total costs with hospitalizations for DM

	Entering		Leaving	
	Treated	Untreated	Treated	Untreated
	(1)	(2)	(3)	(4)
DST_{LATE}	0.189***	0.049	-0.040	0.072
	(0.069)	(0.137)	(0.068)	(0.181)
Bandwidth	18	23	17	13
Obs. to the left	1,026	1,794	912	1,014
Obs. to the right	1,083	1,872	969	1,092
Total	2,109	3,666	1,881	2,106

Note: We use the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012). All specifications use a first order polynomial and a uniform kernel. Robust Standard errors in parentheses. ***, ** and * represent $p<1\%$, $p<5\%$ and $p<10\%$ respectively.

Now, in order to hold the claim that the effect of DST transition over DM hospitalization is an indicator of the significant risk behind short-term sleep deprivation, we must assume that the spike in DST transition is not overestimated by patients being overly cautious about their own condition, hasting to the hospitals following one night sleep curtailment but carrying no real implications to their overall health. The following results in this section explores this possibility by measuring the impact of transition to DST on mortality figures related to DM. Since conditions that lead to death are obviously less controllable than a simple individual decision to attend a hospital, we expect our outcomes on mortality to carry a more clear evidence on the effect of DST on the risk of DM.

In tables 7 and 8 we present two separate, yet closely related set of results. Table 7 contains RD estimates for the effect of entering and leaving DST on the number of deaths due to DM in treated and untreated states, along the lines of our previous expositions. We argue that this alone is a good enough indicator for the risk of diabetes.[17] The effect of DST transition on DM mortality, by number of patients, is positive and significant. After transition, the number of victims increase by 8.5% (column 1), and no effect is observed in states not adopting DST (column 2). The effect of leaving DST is negative and significant in treated states, which means that the number of deaths decreases after transition to regular time and no risk of a short-term widening in sleeping time can therefore be inferred.

Table 7: RD estimates of the impact of entering DST on DM mortality

	Entering		Leaving	
States	Treated	Untreated	Treated	Untreated
	(1)	(2)	(3)	(4)
DST_{LATE}	0.085*	0.034	-0.129**	0.010
	(0.045)	(0.032)	(0.056)	(0.037)
Bandwidth	16	15	11	14
Obs. to the left	912	1,170	570	1,092
Obs. to the right	969	1,248	627	1,270
Total	1,881	2,418	1,197	2,362

Note: We use the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012). All specifications use a first order polynomial and a uniform kernel. Robust Standard errors in parentheses. ***, ** and * represent p<1%, p<5% and p<10% respectively.

[17] The number of deaths due to DM in our database also accounts for individuals that have been hospitalized in days prior to DST transition. That is, this number is the sum of the number of deaths by entrance date.

In columns 1 and 2 of table 8 we present estimates of the impact of DST on the proportion of deaths by hospitalization due to DM in a day. We interpret that, if this proportion remains fairly constant, the profile of medical treatments being carried over in hospitals in DST transition day will correspond to that of any other regular day. This would allow us to more securely relate any unveiled effect in mortality to our previous results regarding hospitalization. In columns 3 and 4, we present estimates of the impact of DST on the number of counties having at least one death in treated and untreated states. This specification has the benefit of being less sensitive to outliers, such as an unusually large change in the number of deaths on the transition day in a specific large city. To be consistent with our story, we expect not only changes in total number of deaths after transition, but also changes in total number of counties having at least one fatality.

Results obtained are again in accordance with our predictions. The proportion between number of deaths and number of hospitalizations, at best, varies very little in treated states (a increase in 1% is identified in column 3) and none in untreated states (column 4). Finally, we observe that DST results in an 8.3% increase in the number of counties having at least one fatality related to patients hospitalized with DM condition.

Table 8: RD estimates of the impact of entering DST on DM mortality

	Mortality rate		Probability of death occurring	
States	Treated	Untreated	Treated	Untreated
	(1)	(2)	(3)	(4)
DST_{LATE}	0.009*	0.000	0.083***	-0.015
	(0.005)	(0.006)	(0.031)	(0.028)
Bandwidth	20	24	12	17
Obs. to the left	1,134	1,737	684	1,326
Obs. to the right	1,192	1,798	741	1,404
Total	2,326	3,535	1,425	2,730

Note: We use the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012). All specifications use a first order polynomial and a uniform kernel. Robust Standard errors in parentheses. ***, ** and * represent p<1%, p<5% and p<10% respectively.

These results on mortality from DM call for more insightful interpretations concerning the social cost they imply, as they represent the direct outcome of an interference brought up by policy on a subject as delicate as the loss of human lives (and the value society put on these lives). A established approach in the economic literature is the concept of Value of Statistical Life (VSL), which correspond to the subjective value, in monetary figures, of a marginal change in the likelihood of death of one individual. It is widely used to evaluate wage-fatality risks trade-offs in the labour market and comes normally from econometric estimates using occupational and demographic variables, but applications have been widened to assess mortality costs in a broad range of issues. It is beyond the scope of this paper to thoroughly calculate such estimates from the case we observe here, but back of envelope calculations should suffice in providing a rough estimate on what these costs represent. Building on the VSL in Knieser et al (2012), which ranges from \$4 to \$10 million, and given we observe about 52 deaths per day on treated states, we estimate DST causes an increase of 4.42 deaths per day, leading to 155 death increase at a social cost of \$.62-1.55 billion within the first of week of DST over the five year sample period we analyze.

1.5.3 Robustness Checks

Following the common practice in causal inference literature, we provide support for the identifying assumption by estimating placebo causal treatment effects which, under the hypothesis of identification, are supposed not to be statistically significant (Imbens, 2004). Naturally, not rejecting the hypothesis that a similar effect is zero is not sufficient to prove that identification is achieved, but makes this assumption considerably more plausible. We also provide additional robustness by evaluating how our empirical model respond to restrictions in the sample of observations and to changes in its technical parameters. The set of results shown below therefore support the hypothesis that the discontinuity found in DST entrance transition is not a mere statistical coincidence.

Our first exercise is to propose a falsification test by estimating treatment effects on different groups of diseases that should not respond to the transition to DST. Table 4 displays estimates of the impact of entering DST on hospital admissions for two different groups of respiratory diseases: one is Influenza and Pneumonia, which is identifyed with codes J09-J18 at ICD-10; and the other is Bronchitis and Asthma, which is identifyed with codes J40-J47 and J20-J22. As it is the case with DM, these conditions also have strong impact over the older population, figuring among the main causes of hospitalizations for Brazilian adults with 60 years old or more (Loyola Filho et al., 2004). Other risk factors such as smoking/drinking habits, use of medications and emotional problems are commonly referred to be independently associated with reports for respiratory diseases in that population in Brazil (DonalisioI et al., 2006; Daufenbachet al., 2009) but, to our knowledge, no relationship have so far been established with any kind of sleeping disorder. Our placebo tests confirm that the two groups of respiratory diseases evolve smoothly around the transition date.

Table 9: RD estimates of the impact of entering DST on incidence of placebo diseases

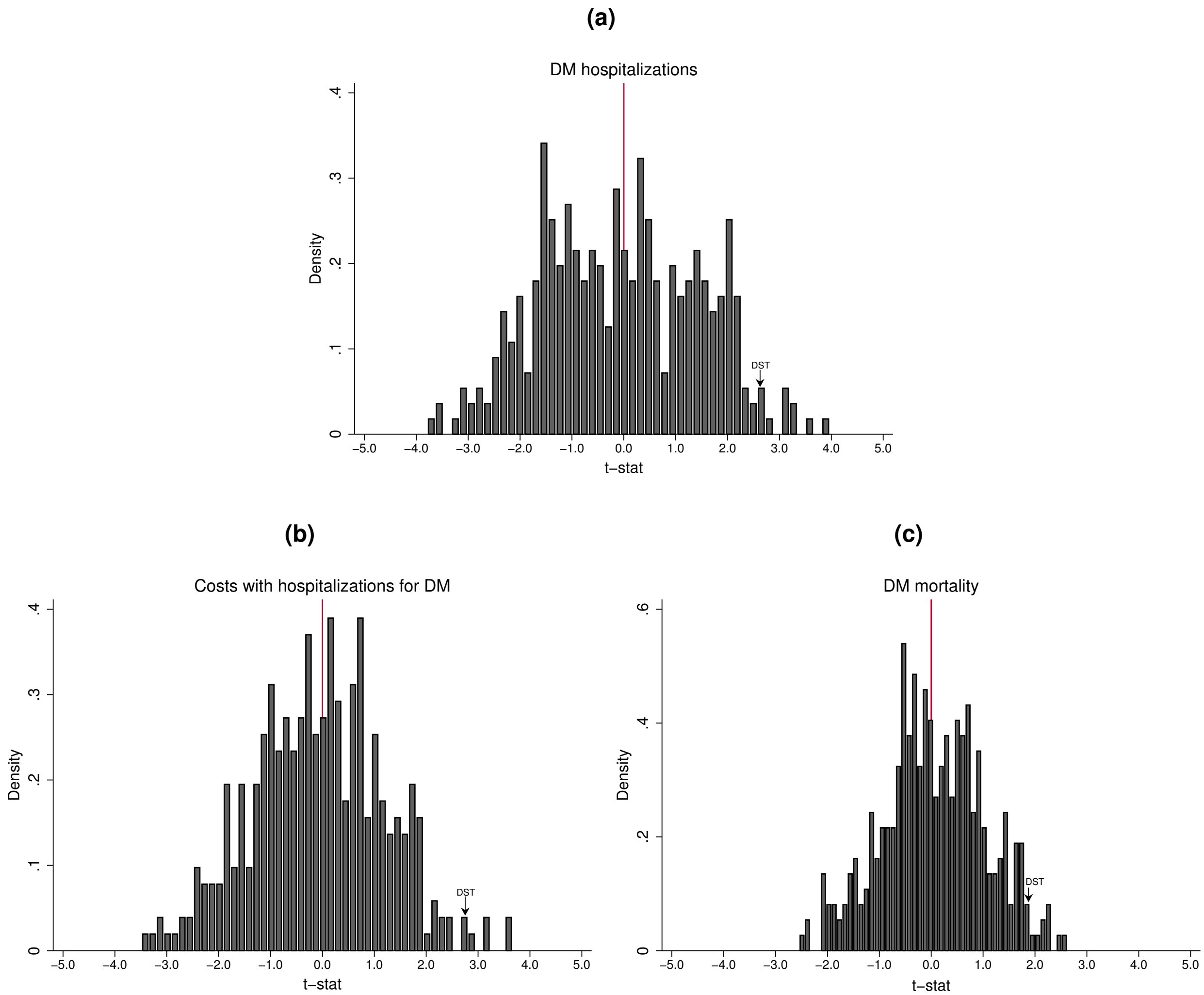
	Influenza and Pneumonia	Bronchitis and Asthma
	(1)	(2)
DST_{LATE}	0.040	0.035
	(0.025)	(0.034)
Bandwidth	24	22
Obs. to the left	1,368	1,254
Obs. to the right	1,425	1,311
Total	2,793	2,565

Note: We use the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012). All specifications use a first order polynomial and a uniform kernel. Robust Standard errors in parentheses. ***, ** and * represent $p<1\%$, $p<5\%$ and $p<10\%$ respectively.

Our next exercise is to assign placebo treatments to other days in the year not correspondent to the actual DST transition and estimate their respective coefficients, in order to evaluate their responses on our outcomes of interest. We have theorised that the investigated effect happens due to the one hour curtailment on the length of the day in which transition to DST takes place, so as this policy obviously imposes no changes on the number of hours of preceding and posterior days, it would not be plausible to observe too many false-positives with this exercise - that is, statistically significant shifts to DM outcomes in other days of the year brought about by a false transition to DST.

In the histograms shown in Figure 1.3, we calculate t-statistics of the estimates for 364 days of the year not correspondent to transition day to DST, ordered by their position as leads or lags from DST transition. The calculated values are displayed in the horizontal axis of the graph and a red vertical line marks the correspondent null hypothesis of zero. The actual effect of DST is indicated in the figure with an arrow. We also exclude outlying observations that are likely influenced by holidays, elections and other such relevant events, for the purpose of designing a proper comparison with regular days not affected by any other exogenous factor. Results thus obtained are also in consonance with our previous findings. For two of our main outcomes, hospitalization and costs, the placebo effects follow a bell-shaped distribution around zero and the real observed effects of DST are between the very few positive estimated values surpassing two standard deviations. For mortality, the distribution of the placebo effects follows a same pattern but with a somewhat higher kurtosis, and although the real effect of DST does not reach two standard deviations (p-value $= 0.058$), it is still between the very few statistically significant estimated values at the positive extreme of the histogram.

Figure 1.3: Histograms with t-statistics of RD estimates for pre-treatment and post-treatment impacts of entering DST



Note: Calculated values are displayed in the horizontal axis, measured in units of standard deviations. The values correspond to preceding and following days to the actual DST transition plus the actual effect of DST, indicated in the figure with an arrow. A red vertical line marks the correspondent null hypothesis of zero.

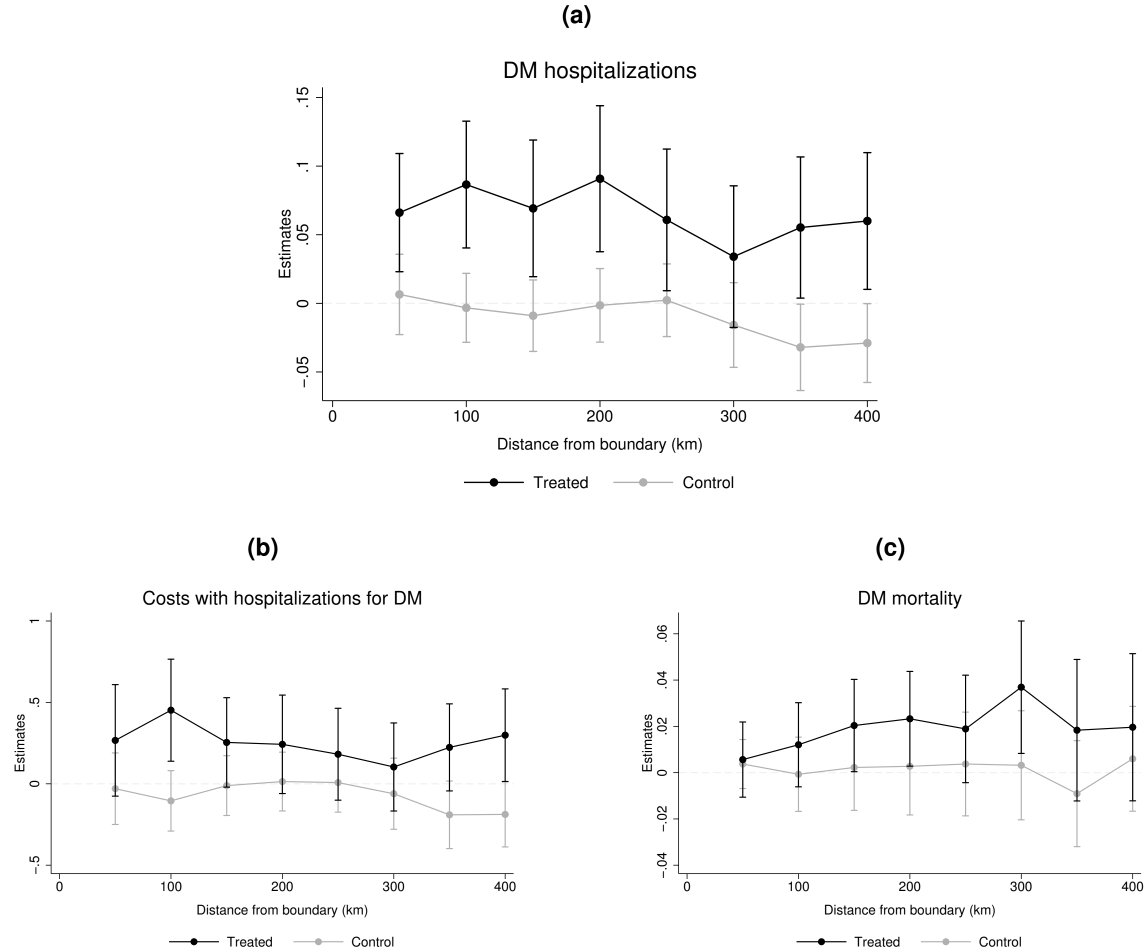
In the next exercise, we design a more conservative comparison between treated and non-treated estimates by looking at those municipalities which are allegedly more balanced in terms of geographical features. Geography is a very influential factor to economic development in Brazil. Regions with milder climate such as the South and Southeast have historically prospered from climate-dependent economic activities such as agriculture and extensive livestock, whereas development in much of the Northeast is still hampered by dry weather and strong droughts. Such conditions are certainly reflected in many municipality-level socioeconomic indices such as poverty and health.

The idea is to truncate our whole sample of observations conditional on their municipality falling within a certain distance from the geographical border dividing the group of states that are treated from the group that is not treated. The border is drawn as an imaginary line separating the Midwest, Southeast and South regions (treated) from the North and Northeast regions (non treated)[18] We exclude all data corresponding to 2011 and 2012 since Bahia and Tocantins adopted DST in those respective years, thus changing the geographical border defined between adopting and non-adopting states for the years of 2008 through 2010. By focusing on municipalities that are closer to state borders, we also avoid possible outlying values from large urbanized areas such as state capitals, which are all located further from this imaginary line.

Results are summarized in the three graphics presented in figure 1.4. The value of each estimate is measured in the y-axis and conditioning distance from the border (in km) is indicated in the x-axis. Black dots correspond to estimated RD values using observations in the treated side and grey dots are the same for the non-treated side. 90% confidence intervals for each value based on their standard deviations are also given. Estimates are noticeably more striking for our main outcome of interest, DM hospitalizations, where every estimate of the effect of DST transition in treated states is positive and statistically different from zero (except only for the truncated sample of 300 km from the border) and the same estimates for non-treated states are indifferent from zero. This same pattern is observed in estimates for costs and mortality, although estimates of the effect of DST transition in treated states are less regular in terms of statistical significance. For these outcomes, most estimates for treated states are positive and for non-treated states they are invariably null.

[18] See figure 2.1. The imaginary line is the one dividing states in black (RS, SC, PR, SP, RJ, ES, MG, GO, MS, MT, DF) from states in grey and light grey (AC, AM, RR, PA, AP, MA, PI, CE, RN, PB, PE, AL, SE, TO, BA).

Figure 1.4: Graphics with RD estimates for impacts of entering DST.



Note: Samples are conditional to distance from the boundary between treated and non-treated states. The value of each estimate is measured in the *y*-axis and conditioning distance from the border (in km) is indicated in the *x*-axis. Black dots correspond to estimated RD values using observations in the treated side and grey dots are the same for the non-treated side. 90% confidence intervals for each value are given.

As a final robustness exercise, we examine whether the estimated effect is robust to several assumptions concerning alternative bandwidths, RD polynomials and kernels, as in Dell et al. (2015). Results for these alternative specifications are displayed in panels A, B and C of table 10, in that respective order. We observe no substantial departures in terms of values or statistical significance from our main results, presented in table 3.

Table 10: RD estimates of the impact of entering DST on DM: Alternative Bandwidths, Polynomials and Kernels

	Panel A: Alternative bandwidths				Panel B: Alternative polynomials				Panel C: Alternative kernels			
	Treated		Untreated		Treated		Untreated		Treated		Untreated	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	DM hospitalizations											
DST_{LATE}	0.075**	0.036*	0.004	0.002	0.107***	0.073*	0.051	0.080	0.077**	0.077**	-0.001	0.001
	(0.030)	(0.022)	(0.035)	(0.024)	(0.033)	(0.043)	(0.045)	(0.055)	(0.032)	(0.031)	(0.033)	(0.033)
Bandwidth					CCT	CCT	CCT	CCT	CCT	CCT	CCT	CCT
	30	60	30	60	28	29	22	26	15	17	23	25
Polynomial Order	1	1	1	1	2	3	2	3	1	1	1	1
Kernel	Uni	Uni	Uni	Uni	Uni	Uni	Uni	Uni	Epa	Tri	Epa	Tri
Obs.	3,477	6,897	4,758	9,438	3,249	3,363	3,510	4,134	1,767	1,995	3,666	3,978
	Costs with hospitalizations for DM											
DST_{LATE}	0.145**	0.099*	0.120	0.016	0.134*	0.184*	-0.001	0.0127	0.184***	0.167**	0.055	0.060
	(0.067)	(0.052)	(0.157)	(0.109)	(0.072)	(0.098)	(0.168)	(0.208)	(0.068)	(0.066)	(0.139)	(0.139)
Bandwidth					CCT	CCT	CCT	CCT	CCT	CCT	CCT	CCT
	30	60	30	60								
Polynomial Order	1	1	1	1	2	3	2	3	1	1	1	1
Kernel	Uni	Uni	Uni	Uni	Uni	Uni	Uni	Uni	Epa	Tri	Epa	Tri
Obs.												
	DM mortality											
DST_{LATE}	0.087**	0.057*	0.014	-0.023	0.094**	0.087	0.028	0.028	0.089**	0.087**	0.032	0.032
	(0.043)	(0.030)	(0.030)	(0.022)	(0.046)	(0.058)	(0.036)	(0.039)	(0.044)	(0.044)	(0.033)	(0.032)
Bandwidth					CCT	CCT	CCT	CCT	CCT	CCT	CCT	CCT
	30	60	30	60								
Polynomial Order	1	1	1	1	2	3	2	3	1	1	1	1
Kernel	Uni	Uni	Uni	Uni	Uni	Uni	Uni	Uni	Epa	Tri	Epa	Tri
Obs.												

Notes: CCT refers to the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012). Indicated otherwise, all specifications use a first order polynomial and a uniform kernel. Robust Standard errors in parentheses. ***, ** and * represent p<1%, p<5% and p<10% respectively.

1.6 Concluding Remarks

We provide empirical evidence that short-term restrictions on sleeping time can impact the risk of developing or aggravating conditions of DM. Most particularly, we estimate, using a regression discontinuity design, that the reduction of one hour of sleep in one single night provoked by DST policy (which induces individuals' routines in adopting states to be abruptly advanced in one hour on transition day) increases the amount of admissions for this specific condition in Brazilian hospitals in around 6% to 8% between specifications, while no effect is observed in states that do not adopt DST policy. We also find no impact of leaving DST on hospital admissions due to DM in any of the adopting states, which further implies that there is no significant effect on the risk of DM provoked by a short-term increase on sleeping time.

These results are shown to remain consistent when our sample in decomposed in macroregions. Furthermore, age and gender decomposition show that this increase in hospitalization is mostly evident in the male population above sixty years of age, which ties with previous studies showing that DM is slightly more prevalent in that group, specifically. Carrying forth our analysis, we find that health care expenses for diabetes treatment and mortality also respond to the policy and increase in around 18.9% and 8.5%, respectively, both serving as additional evidence of the risks implied by sleep deprivation over DM. As with hospitalization, none of these changes are observed in states that do not adopt DST policy. Our estimates imply DST increases health care expenses by around $3 million, and cause a total of 155 deaths at a social cost of $.62-1.55 billion over the 5 year sample period we analyze.

Patients with DM are required to constantly regulate their blood sugar levels and to prevent several other complications which are knowingly sensitive to even the most transient shocks in their routines in order to manage stability over their conditions, making them highly prone to frequent visits to the health care system. It has also been shown that the impact of partial sleep deprivation during only a single night can be sufficient to induce insulin resistance even in healthy individuals. These facts lead to the reasoning that the immediate impact of a one-hour sleep restriction on DM management is evident through the observed increase in DM hospitalization during transition day to DST. Regardless of the underlying mechanism, our results show a clear causal link from DST transition to hospitalization, health care costs and number of deaths of patients with DM.

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1.8 Appendix

1.8.1 DM subgroups in ICD-10

The list below depicts the details of the diseases included in the group Diabetes Mellitus (E10-E14) from the chapter IV of the International Statistical Classification of Diseases and Related Health Problems 10th Revision (ICD-10), versions of 2008 and 2010.

E10: Insulin-dependent diabetes mellitus

Includes:
diabetes (mellitus):

- brittle
- juvenile-onset
- ketosis-prone
- type I

Excludes:
diabetes mellitus (in):

- malnutrition-related (E12.-)
- neonatal (P70.2)
- pregnancy, childbirth and the puerperium (O24.-)

glycosuria:

- NOS (R81)
- renal (E74.8)

impaired glucose tolerance (R73.0)
postsurgical hypoinsulinaemia (E89.1)

E11: Non-insulin-dependent diabetes mellitus

Includes:
diabetes (mellitus)(nonobese)(obese):

- adult-onset
- maturity-onset
- stable
- type II

non-insulin-dependent diabetes of the young

Excludes:
diabetes mellitus (in):

- malnutrition-related (E12.-)
- neonatal (P70.2)
- pregnancy, childbirth and the puerperium (O24.-)

glycosuria:

- NOS (R81)
- renal (E74.8)

impaired glucose tolerance (R73.0)
postsurgical hypoinsulinaemia (E89.1)

E12: Malnutrition-related diabetes mellitus

Includes:
malnutrition-related diabetes mellitus:

- insulin-dependent
- non-insulin-dependent

Excludes:
diabetes mellitus in pregnancy, childbirth and the puerperium (O24.-)
glycosuria:

- NOS (R81)
- renal (E74.8)

impaired glucose tolerance (R73.0)
neonatal diabetes mellitus (P70.2)
postsurgical hypoinsulinaemia (E89.1)

E13: Other specified diabetes mellitus

Excludes:
diabetes mellitus (in):

- insulin-dependent (E10.-)
- malnutrition-related (E12.-)
- neonatal (P70.2)
- non-insulin-dependent (E11.-)
- pregnancy, childbirth and the puerperium (O24.-)

glycosuria:

- NOS (R81)
- renal (E74.8)

impaired glucose tolerance (R73.0)
postsurgical hypoinsulinaemia (E89.1)

E14: Unspecified diabetes mellitus

Includes:
diabetes NOS

Excludes:
diabetes mellitus (in):

- insulin-dependent (E10.-)
- malnutrition-related (E12.-)
- neonatal (P70.2)
- non-insulin-dependent (E11.-)
- pregnancy, childbirth and the puerperium (O24.-)

glycosuria:

- NOS (R81)
- renal (E74.8)

impaired glucose tolerance (R73.0)
postsurgical hypoinsulinaemia (E89.1)

1.8.2 Additional Tables

Table A1: RD estimates of the impact of DST on DM hospitalizations for treated states - additional robustness

	No Conversion		No Transition	
	(1)	(2)	(3)	(4)
DST_{LATE}	0.095***	0.053*	0.073*	0.062*
	(0.031)	(0.031)	(0.037)	(0.033)
Bandwidth	CCT	IK	CCT	IK
	17	26	11	25
Obs. to the left	969	1,482	627	1,425
Obs. to the right	1,026	1,539	627	1,425
Total	1,995	3,021	1,254	2,850

Note: CCT refers to the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012); IK is Imbens and Kalyanaraman (2012). All specifications use a first order polynomial and a uniform kernel. Robust Standard errors in parentheses. ***, ** and * represent $p<1\%$, $p<5\%$ and $p<10\%$ respectively.

CHAPTER 2

AMBIENT LIGHT AND HOMICIDES[1]

2.1 Introduction

In this paper we use hourly data on mortality over a period of six years to investigate the influence of additional light-time on homicide occurrences. For that we use the natural experiment induced by Daylight Saving Time (hereafter, DST) as an exogenous shock to ambient light. Although recent literature has already exploited DST to identify causal effects on crime, there is scarce evidence about its effect on homicides.

For the purpose of this exercise we analyze one of the most violent countries in the world – Brazil. In 2012, the country led the ranking in homicide counts with 50,108 deaths, way above the second place, India, which hosted 43,355 intentional homicides (UNODC, 2013). In that same year, the homicide rate in the country was 25.2 per 100,000 and , approximately seven times the rate observed in the United States and more than eight times that observed in Europe. Like many developing countries, much of this violence is associated with drugs and wide availability of guns in the black market, with young, male citizens being both main victims and perpetrators (Reichenheim et al., 2011). Matching this profile, homicide represents the leading cause of death for men aged 15-44 in Brazil, with 90% of the cases involving firearms (De Souza et al., 2007).

These alarming numbers, combined with existing estimates showing the direct costs associated with violence and crime to range from 3 to 5 percent of annual GDP (Heinemann and Verner, 2006; World Bank, 2006), were preponderant for the Government to adopt several policies to reduce

[1]This work has as coauthors Robson Tigre e Breno Sampaio.

crime in the mid-2000s. In 2003, for instance, the Government passed laws aimed at controlling the flow of firearms into the country, instituting strict background checks for gun purchases and registration, and made it illegal for civilians to conceal even registered guns outside their home or business. Those measures, according to recent estimates, saved between 2,000 and 2,750 lives from 2004 to 2007 in cities with more than 50,000 inhabitants in the state of São Paulo alone (Cerqueira and De Mello, 2015).

None of these changes, however, were sufficient to shift the dynamics of homicides substantially in the country. In 2014, the number of registered intentional homicides was 52,336, 3.8% larger than the numbers registered in 2013. Many blame low levels of law enforcement and a wide sense of impunity for this persistence, which ultimately lead a considerable share of the nation to believe that good criminals are dead criminals.[2]

While the national media portraits perpetrators as risk-loving individuals unafraid of getting caught, empirical findings suggest that criminal decision-making relates intimately to the standard labor-supply model, in which individuals' decision to participate in the market is based on the ratio of expected benefits and expected costs (Abrams, 2012; Doleac and Sanders, 2015). Considering the gains in utility derived from a successful criminal engagement, agents' cost is function of two main parameters: (*a*) the probability of getting caught, and (*b*) the disutility from punishment, once caught. While there are convincing theoretical arguments for how changes in those parameters individually affect criminal decision (Becker, 1968), empirical evidence to support such models usually suffer from at least two important shortcomings. First, the effectiveness of law enforcement, which affects the probability of getting caught, can barely be disentangled from characteristics of the legal structure, which explains the magnitude of disutility derived from punishment. Second, even when a variation in one of those factors does not depend on the other, there is still a challenge of overcoming simultaneity bias, as long acknowledged in Levitt (2002).

In this context, we follow the rationale from the literature on crime deterrence that states

[2]http://www1.folha.uol.com.br/internacional/en/brazil/2015/10/1690283-half-of-brazil-believes-that-good-criminals-are-dead-criminals.shtmlFolha de Sao Paulo, "Half of Brazil Believes That 'the Only Good Criminal Is a Dead Criminal'." Accessed on January 21st of 2016.

luminosity during otherwise high-crime hours facilitate witnesses and law enforcement agents to detect perpetrators (Doleac and Sanders, 2015), thus increasing perpetrators' expected cost, and exploit an exogenous intra-day shift in light period caused by Daylight Saving Time to estimate the effect of additional light-time on homicide occurrences. The natural experiment induced by Daylight Saving Time (hereafter, DST) can serve as an alternative potentially as good as randomization to identify the effect of interest, as the source of variation is completely exogenous to criminal decision-makers (Imbens and Lemieux, 2008; Angrist and Pischke, 2014).

Although recent empirical literature has already exploited DST to identify causal effects on a broad range of outcomes, including criminal activity (Doleac and Sanders, 2015),[3] to the best of our knowledge the present paper is the first to find robust evidence of additional light time influencing homicide occurrence. Efforts to estimate the effect of light on homicides have already been registered in (Doleac and Sanders, 2015), but due to the rarity of homicides in the U.S. compared to Brazilian levels, they lost statistical power to detect the desired effect, finding only statistically significant effects for robberies. This analysis is specially important for a developing country like Brazil, where the lack of public presence falls disproportionally heavier on those less well-off. If we consider intensity of night light a proxy for public outside illumination, while the average population density in Brazil is 600% greater than in Canada, both countries have a similar pattern in intensity and distribution of night time illumination (Henderson et al., 2012). Our findings thus may suggest that policies as simple as providing adequate public illumination can significantly deter lethal violence.

Given the tremendous role firearms play on homicides in Brazil, we focus on deaths for which firearm discharge was the cause, using data from the Information System on Mortality (SIM) implemented by the Brazilian Ministry of Health. To provide a statistically well-founded and accurate estimate of the effect of light on homicides deterrence during transition from standard time to daylight saving time, we use local-polynomial regression-discontinuity estimators with

[3] Other examples are Kountouris and Remoundou (2014), who analyse the impact of DST on individual well-being, Smith (2016), who studies the impact of this variation on fatal vehicle crashes, and Toro et al. (2015) which investigate the effect of sleep disturbances on myocardial infarction.

bias-corrected non-parametric confidence intervals (Henderson et al., 2012). We provide results using two data-driven bandwidth selectors; the optimal-selection procedure recently proposed in (Calonico et al., 2014), hereafter CCT, and, as benchmark, that outlined in Imbens and Kalyanaraman (2012), hereafter IK.

We first document that redistribution in lightness significantly disincentives homicides, decreasing its occurrence by 14.4% on the days after the transition. Using hourly registered crimes, we then show that our findings are consistent with more specific theoretical predictions that suggest a strong decrease in criminal behavior in the hours most affected by DST policy, those around sunset. We also examine whether the estimated effect is robust to several assumptions concerning alternative bandwidths, RD polynomials, and kernels, as in Dell (2015).

While those results are consistent with previous findings and predictions, a constant concern in empirical studies is the possibility of the treatment being correlated with unobservable factors, leading to a spurious estimated effect. In our framework, this means that the timing of DST adoption may coincide with an event neglected by the analysis that is the actual driver of homicide deterrence, though through channels other than ambient light. Regarding this possible scenario, and in contrast with other papers that exploit DST variation, we propose a falsification test on the basis of a well defined control group, namely Brazilian states not affected by the policy, to show that homicides evolve smoothly around the period of transition to DST in the absence of DST. We then proceed by using this state variation in treatment status in a differences-in-differences framework to estimate the effect of DST not only on the transition but also on the three-month period in which the policy is adopted. We find that homicides by firearms decreased during DST months by about 3%. More importantly, this effect is mostly concentrated on the hours around sunset, which observe a decrease of 6.5-8.1%.

These estimates imply Daylight Saving Time is responsible for saving about 3,850 potential victims from 2006-2011. This number is 30% above the total number of homicides that occurred in 2013 in the nineteen countries located in Northern and Western Europe. Building on the value of statistical life in Kniesner et al. (2012), which ranges from $4 to $10 million, we estimate DST resulted in $2.57 billion in annual social cost savings from avoided homicides. In addition to this massive direct effect, a significant reduction in homicides could also have large long-lasting indirect effects. For instance, recent estimates provided by Koppensteiner and Manacorda (2016) using data from Brazil show that exposure to a homicide during the first trimester of pregnancy considerably reduces gestational length and birth-weight. This further reinforces the importance of our empirical findings.

The remainder of the article is organized as follows. Section 2.2 describes structure and institutional framework of DST in Brazil, while sections 2.3 and 2.4 present the data set and empirical strategies we exploit. Finally, section 2.5 discusses the results and conclusions are presented in section 2.6.

2.2 Daylight Saving Time in Brazil

DST is an energy policy adopted worldwide that takes advantage of variation in the distribution of sunlight time between seasons and reallocates ambient light to the evenings by shifting the relationship between clock time and sunset by (usually) one hour.[4] This policy is observed in 76 countries in 2016 and affects more than 1.5 billion people yearly. In Brazil, DST has been adopted every year since 1986.

Historically DST in Brazil has been governed by Federal enactments, usually based on information from technical reports provided by The Electric System National Operator (ONS). The National Operator suggests to the Federal Government which states should adopt DST and the duration of the regime, which usually starts on the third Sunday of each October, when clocks skip forward from 12am to 1am, and extends until midnight of the third Sunday of each February, when clocks fall back one hour to standard time. Given the core of this policy, which is the summer solstice in the Southern Hemisphere, DST implementation does not provide benefits for states closer

[4] A review of the origins, early adoptions and further discussion on DST is presented in Aries and Newsham (2008).

to the Equator line, which leads to variation in the treatment status across the country. Its technical basis, provided by the ONS, jointly with the compliance enforced by Federal legislation favor our identification strategy since it provides variation in DST adoption both between (i.e., adopters vs. non-adopters) and within states (i.e., among those that adopt; standard time vs. DST). Having non-adopter states helps us in designing a robust placebo test, given other factors affecting homicides besides DST must evolve smoothly around the transition date in states that did not adopt it.[5]

Figure 2.1: DST policy in Brazil

Note: States in black adopted DST from 2006 to 2011 and together constitute the Midwest, Southeast and South administrative regions. States in grey adopted DST in only once in this period (Bahia in 2011). States in light grey did not adopt DST between 2006 and 2011.

Throughout the entire time span we analyze (2006-2011), all Brazilian states within Mid-western, Southern and Southeastern administrative regions, where light incidence vary the most across seasons, adopted DST, while no states in Northern and Northeastern geopolitical regions did, except for Bahia, which adopted DST in 2011 for political reasons.[6] This is illustrated in Figure 2.1. A detailed list of adopters by each year is provided in Table 1 below.

[5]Doleac and Sanders (2015) and Smith (2016), for example, consider law changes to DST policy in the U.S. to account for endogeneity, since DST occurs simultaneously across 48 states (Arizona and Hawaii do not observe DST) and at approximately the same time each year.

[6]Results are qualitatively the same regardless the inclusion of this state in our estimations.

Table 1: List of adopters by years

Year and Begin	End	Length (days)	States	
2006-2007	Nov 5 2006	Feb 25 2007	112	RS, SC, PR, SP, RJ, ES, MG, GO, MT, MS, DF.
2007-2008	Oct 14 2007	Feb 17 2008	126	RS, SC, PR, SP, RJ, ES, MG, GO, MT, MS, DF.
2008-2009	Oct 19 2008	Feb 15 2009	119	RS, SC, PR, SP, RJ, ES, MG, GO, MT, MS, DF.
2009-2010	Oct 18 2009	Feb 21 2010	126	RS, SC, PR, SP, RJ, ES, MG, GO, MT, MS, DF.
2010-2011	Oct 17 2010	Feb 20 2011	126	RS, SC, PR, SP, RJ, ES, MG, GO, MT, MS, DF.
2011-2012	Oct 16 2011	Feb 26 2012	133	RS, SC, PR, SP, RJ, ES, MG, BA, GO, MT, MS, DF.

Note: In this table we present a detailed list of the Brazilian states that adopted DST from 2006-2011. We report also the date of transition from standard time (ST) to DST and from DST to ST. Source: `http://www.mme.gov.br/`. State codes: RS - Rio Grande do Sul; SC - Santa Catarina; PR - Paraná; SP - São Paulo; RJ - Rio de Janeiro; ES - Espírito Santo; MG - Minas Gerais; BA - Bahia; GO - Goiás; MT - Mato Grosso; MS - Mato Grosso do Sul; DF - Distrito Federal.

2.3 Data

We use data on homicides for the period of 2006-2011 retrieved from the *Sistema de Informações sobre Mortalidade* (SIM), the national information system on mortality, implemented by the Brazilian Ministry of Health. The System was designed to provide daily individual-level information on mortality to local and federal authorities, claiming global coverage within national borders. To this end SIM relies on legal certificates of death as its data input, which are strictly regulated by the Federal Government.[7]

Official declarations of death contain two features that help us support our claims, namely time and cause of death according to the most recent revision of the International Classification of Diseases, ICD-10. Among the "environmental events and circumstances" listed in ICD-10, we

[7]Published in October 9th of 2003, Federal Ordinance MS/SVS nº 20 provides a rulebook on the filling out of declaration of death forms, which are standardized and distributed by the Ministry of Health.

focus on deaths caused by firearm discharge, as they play an important role in criminal interactions in Brazil (De Souza et al., 2007; Reichenheim et al., 2011). Since there is serious evidence of a large share of intentional homicides being misclassified as fatal incidents of undetermined intent (Cerqueira, 2012, 2013), we consider both deaths due to assault and with undetermined intent in this study.[8] The complete list of ICD-10 codes we use to construct our dependent variable is provided in Table 2.

It is important to notice that fatal incidents of undetermined intent do not include accidental deaths caused by firearms, which are classified under the ICD-10 codes W32-W34. Additionally, we opt to exclude homicides resulting from legal intervention from our estimates, since according to ICD's methodology those are the result of law-enforcement agents on duty in the course of arresting or attempting to arrest lawbreakers, and therefore would mistakenly favor our hypothesis by inflating estimates as deaths from legal intervention are expected to be positively correlated with lethal criminal activity.

Table 2: ICD-10 - homicides involving firearm discharge

Code	Description
X93	Assault by handgun discharge
X94	Assault by rifle, shotgun and larger firearm discharge
X95	Assault by other and unspecified firearm discharge
Y22	Handgun discharge, undetermined intent
Y23	Rifle, shotgun and larger firearm discharge, undetermined intent
Y24	Other and unspecified firearm discharge, undetermined intent

Note: In this table we present a detailed list of ICD-10 codes we use to construct our dependent variable.

In all specifications our dependent variable is the natural log of the number of homicides according to the definition presented in Table 2 (hereafter homicides). To eliminate persistent day-of-week effects (for instance, it might be the case that homicide occurrence is higher on weekends than weekdays), state differences and long-term time trends, we follow Smith (2016) and Toro et

[8]In fact, evidence regarding systematic misclassification comes from the Institute for Applied Economic Research (Ipea), a federal public institution directly linked to the Secretariat of Strategic Affairs of the Presidency of the Republic (SAE/PR).

al. (2015) and demean the log of homicides by day-of-week, state and year.

As in Smith (2016), we aggregate our outcome to the state level for two reasons. First, the frequency of homicides occurring daily at more desegregate level (i.e., municipalities) tends to zero for many units. Second, aggregating allows us to gain statistical power and smooths out potential confounders that could affect homicides at the county level but are less likely to affect homicides at the state level. Finally, following a procedure by Janszky and Ljung (2008) and also carried out in Smith (2016), we multiply the number of homicides on the first day of DST by 24/23 to account for a possible distortion coming from the fact that the first day after the transition to DST ends up being one hour shorter than the rest of the days in a year (23 hours).[9]

In table 3 we present average number of homicides, unadjusted for day-of-week and time trend, for one week prior and one week after DST transition. The first column shows averages across all states and years within a window of one week around the transition. In columns 2 and 3 we consider homicides per day while in columns 4 and 5 we consider only those around sunset. We note that for states that adopted the policy, there are on average 3.264 homicides per day on the week prior to transition to DST. On the week following transition, this number decreases to 2.962, a reduction of almost 10%. If we look only at those occurring around sunset, we observe a decrease of about 42%. These patterns are not observed when looking at states that did not adopt the policy. A reduction of 1.4% is observed when looking at daily totals and an increase of 1.5% is observed when looking at those around sunset.

[9] We provide evidence that our results are not driven by this hour adjustment.

Table 3: Average number of homicides per state for one week before and one week after Daylight Saving Time

States	Total	All-day		Sunset	
		Week Pre-DST	Week Post-DST	Week Pre-DST	Week Post-DST
	(1)	(2)	(3)	(4)	(5)
Treated	3.101	3.264	2.962	0.396	0.229
	(2.862)	(3.013)	(2.734)	(0.725)	(0.554)
Untreated	2.037	2.051	2.022	0.266	0.270
	(3.051)	(3.097)	(3.006)	(0.610)	(0.625)

Note: All-day homicides represent the average number of homicides per day on the week prior and week after DST per state. Sunset hour data are the average of total homicides occurring in the hour of sunset and that directly following sunset (dusk). Standard deviations are in parentheses.

2.4 Empirical Strategy

2.4.1 Regression Discontinuity

In this section we present the empirical strategy used to identify the short-term causal effect of light on the number of homicides in Brazil using a regression discontinuity design (RDD). In particular, for an optimally-chosen time interval, we compare the number of homicides before entering DST to the number of homicides after its initiation for those states that adopted DST (as listed in table 1). For that, consider the following reduced-form model.

$$\log Homicides_{isy} = \tau I(Transition_{isy} \geq 0) + g(Transition_{isy}) + \varepsilon_{isy} \tag{2.1}$$

where $\log Homicides_{isy}$ is the natural logarithm of the number of homicides in day i, state s and year y. $Transition_{isy}$ is the running variable, defined as the number of days to/from transition to DST, which is equal to zero on the first day after transition and positive (negative) after (before), while g is a non-parametric function of that variable and ε is a random error term.

We use local-polynomial point estimators and recently developed robust bias-corrected non-parametric confidence intervals (Calonico et al., 2014). In this framework bandwidth selection is crucial since it imposes a trade-off between bias and variance. Therefore, in contrast with previous literature on the field, we rely on optimal data-driven bandwidth selectors to set time interval used for comparison around the transition date. Specifically, we show results using two bandwidth selectors: that outlined in Imbens and Kalyanaraman (2012), hereafter IK, and our preferred one, proposed in Calonico et al. (2014), hereafter CCT.[10]

Consistently estimating the parameter of interest requires the outcome to evolve smoothly around the transition date in the absence of treatment once we control for day-of-week and other fixed effects, an assumption that cannot be directly tested due to the nature of observational counterfactual analysis. Given the institutional setup of DST policy in Brazil, we use a well defined control group of untreated states to test whether unobserved factors at the national level correlate with the period of DST transition and are the real cause for shifts in homicides and provide several robustness exercises and falsifications tests to support results obtained through our main specification.

We first consider the effect of DST on daily homicide rates. In this way we may estimate the net effect of light on criminal activity. Nevertheless, the number of homicides may not respond to the transition if criminals relocate their activity from hours directly affected by the shift in daylight to other dark hours. In this scenario DST would have an insignificant effect on homicides even though light might still affect criminal behavior. To investigate this possibility, we then estimate the effect of interest by hour of the day. Following the crime deterrence rationale, we expect to see a strong decrease in homicides on hours of the day that are directly affected by the transition, i.e., hours that were dark before transition and light after transition.

[10] As discussed in Calonico et al. (2014), previous bandwidth selectors tend to yield large bandwidths, leading to biased confidence intervals. Therefore, we adopt CCT as our main procedure and present results using IK mainly for comparison. Estimations using ad hoc bandwidth selections are also presented.

2.4.2 Differences-in-Differences

The RDD strategy discussed provides a local average treatment effect, which means that comparisons are made for a subset composed by observations around the transition date within DST adopters. Moreover, the non-parametric estimator usually exploited in RDD estimation assigns greater weights the closer observations are to the cutoff. While those features are responsible for most of the appeal of regression discontinuity design due to the incontestable internal validity it provides, it is of interest to provide generalizable estimates for the policy in question. In addition to exploiting the within-state variability on the adopters, we take advantage of variation in treatment status between states using a fixed effects model. Equation 2.2 displays a general specification to this approach, in which $\log Homicides_{isy}$ is the natural logarithm of the number of homicides in day i, state s and year y, DST_{isy} is an indicator variable that assumes value one for adopters states as from the date of transition, and λ_i, λ_s, and λ_y represent respectively day, state and year fixed effects.

$$\log Homicides_{isy} = \beta_0 + \beta_1 DST_{isy} + WeekDay_i + \lambda_i + \lambda_s + \lambda_y + \varepsilon_{isy} \tag{2.2}$$

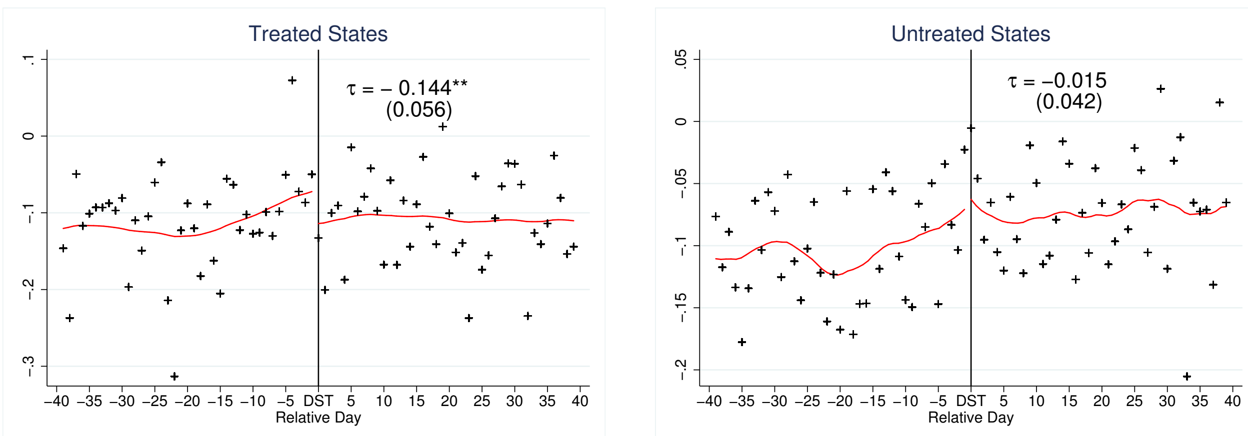
Note that the parameter β_1 identified here has a different meaning from the τ obtained from estimating equation 2.1. The former measures the average effect of the whole period of DST when compared to the rest of the year, exploiting not only time variation within treated states around the transition but also variation in the treatment status, i.e., states that adopted and states that did not adopt. This framework lends itself to a differences-in-differences estimation, given the existence of well defined treatment and control groups, and a treatment that is exogenous to individual decision makers in its technical basis and compliance, which is enforced by the Federal government. In this set up, we also provide results for specifications including state-specific time trends, municipality-level daily precipitation, daily maximum temperature and daily minimum temperature (Jacob et al., 2007).

2.5 Results

2.5.1 Main results

Figure 2.2 displays our main finding graphically for log $Homicides$ demeaned by day-of-week, state, and year around the DST transition date. In the top panel, which represents states that adopted DST, we observe that points to the right of the cutoff are slightly shifted below, implying lower incidence of homicides after transition even when we partial out day-of-week and other fixed effects. The panel on the bottom of figure 2.2 considers contemporaneous homicide levels for the untreated states. As expected, we find no significant discontinuity around the cutoff for those states that did not adopt DST.[11]

Figure 2.2: DST entrance transition



Note: Crosses represent residuals from the regression of ln $Homicides$ on day-of-week, state and year dummies while solid lines are predicted outcome values based on a local linear regression as specified by equation 2.1.

In table 4 we present detailed results of what we showed above for treated and untreated states. Results imply that after transition homicides decreased by about 14% in treated states while on states that did not adopt the policy the difference in homicide levels after the transition is statistically zero. Since unobservables are likely to be balanced near the threshold, given that outcomes

[11] Estimations for non-adopters follow a general equation of the form shown in equation 2.1 unless stated otherwise. Although non-adopters do not experience an actual transition into DST, their indicator function $I(Transition_{isy} \geq 0)$ assumes value one contemporaneously to the transition actually experienced by adopters.

for treated states are likely to be influenced by the same unobservables that determine outcomes for untreated states, those results make a strong case for the reduced form causal relation between DST and crime. This number is substantially larger than the one previously obtained by Doleac and Sanders (2015), who find no consistent impacts for murder using data from the U.S. and a similar identification strategy.

Table 4: RD estimates of the impact of entering DST on Homicides for treated and untreated states

States	Treated	Not treated
	(1)	(2)
DST_{LATE}	-0.144**	-0.015
	(0.056)	(0.043)
Bandwidth	CCT	CCT
	25	28
Obs. to the left	1,650	2,688
Obs. to the right	1,716	2,784
Total	3,366	5,472

Note: CCT refers to the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012). Robust Standard errors in parentheses. ***, ** and * represent p<1%, p<5% and p<10% respectively.

The results presented above indicate a net decrease in homicide levels when we aggregate the outcome daily, and provide evidence of a reduced form relationship between DST and crime occurrence. However, the supposed channel through which DST affects homicides is by shifting ambient light to hours otherwise dark and of typically high crime occurrence, as previously discussed. Therefore, in table 5, instead of providing results on the daily-level of homicides, we restrict the sample to hours around sunset. As in Doleac and Sanders (2015), we expect homicides to decrease mostly on those hours directly affect by the transition, namely those in the periods covering the hour of sunset and that directly following sunset (dusk). Our results strongly support the idea that ambient light has a substantial influence on crime, since we observe a significant decrease (around 12%) in homicides exactly on the hours mostly affected by Daylight Saving Time.[12] Note

[12]Here we focus on the entrance transition. Unfortunately, as shown on table 1, the transition back from DST to

that we observe no change on hours prior to sunset or following dusk. For states not affected by the policy, presented in panel B of table 5, results are precisely zero for all three time intervals we consider.

Table 5: RD estimates of the impact of entering DST on Homicides for both treated and not treated states for hours around sunset

	Hours before Sunset	Sunset	Hours after Sunset
	3/2	0/1	2/3
Panel A: Treated states			
DST_{LATE}	-0.018	-0.117***	0.009
	(0.035)	(0.038)	(0.047)
Bandwidth	CCT	CCT	CCT
	25	25	26
Obs. to the left	1,650	1,650	1,716
Obs. to the right	1,716	1,716	1,782
Total	3,366	3,366	3,498
Panel B: Untreated states			
DST_{LATE}	-0.007	0.013	-0.008
	(0.019)	(0.028)	(0.027)
Bandwidth	CCT	CCT	CCT
	31	28	28
Obs. to the left	2,976	2,688	2,688
Obs. to the right	3,072	2,784	2,784
Total	6,048	5,472	5,472

Note: CCT refers to the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012). Robust Standard errors in parentheses. ***, ** and * represent p<1%, p<5% and p<10% respectively. Regressions are grouped in two hour periods, as indicated in the columns. Hours since sunset are calculated using data on the hour of sunset for each county on the day prior to the beginning of Daylight Saving Time (DST).

Although quite convincing, this hourly approach is not without caveat, since intra-day disaggregation is more prone to systematic measurement error in the recording of time of death than daily data, what would bias our estimates. As an example, there can be a considerable time lapse between the events of getting shot and end up dying; the victim may be hospitalized hours before being declared dead. We tackle this potential issue in the Robustness Checks section by further restricting the sample to observations for which victims died on site and show consistent results

ST coincides with the annual Brazilian Carnival. This introduces difficulties in isolating the effect of carnival on homicides from that of light on homicides.

that support our claims.

An additional remark regarding our main results is that so far our regressions considered variation in $\log Homicides_{isy}$ for states within a given treatment status; to say, we compared what happened in terms of homicides before and after crossing the DST transition to states that adopted DST and, *separately*, to states that did not adopt DST, which is the standard in this literature that exploits variation caused by DST (Doleac and Sanders, 2015; Smith, 2016; Toro et al., 2015). This approach is exceptionally convincing since unobservables are likely to be balanced near the threshold and, at the national level, outcomes for treated states are likely to be influenced by the same unobservables that determine outcomes for untreated states. However, to provide comparability between the two groups (at a cost of lesser internal validity) for a wider time span, we exploit both (*a*) variation across the transition date within DST adopters, and (*b*) treatment status variation between adopters and non adopters using a fixed effects model.

In table 6 we provide Differences-in-Differences estimates for varying sets of fixed effects, time trend and municipality-level controls such as daily precipitation, daily maximum temperature and daily minimum temperature (Jacob et al., 2007). In columns 1-3 we obtain that homicides decrease by about 3% throughout DST period. Note that the inclusion of additional control variables as well as state-specific time trends marginally changes the parameter of interest, adding robustness to our causal claim. In columns 4-7 we estimate the same model, but considering only crimes that occurred around sunset. The effect is negative and statistically significant on the period mostly affected by the Daylight Saving Time, and even larger when we include state-specific time trends. We observe a reduction of about 6.5-8.1% on the periods covering sunset and dusk.

Table 6: Differences-in-Differences estimates of impact of entering DST on Homicides - Adopters vs Non-adopters

	All-day			Before sunset	Sunset		After sunset
				2/1	0/1		2/3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
DST	-0.033***	-0.032***	-0.028**	0.007	-0.065***	-0.081***	0.009
	(0.010)	(0.010)	(0.013)	(0.006)	(0.007)	(0.009)	(0.007)
Additional controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Weekday fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time trend	No	No	Yes	No	No	Yes	No
R^2	0.62	0.62	0.62	0.13	0.17	0.17	0.23
Obs.	59,157	59,157	59,157	59,157	59,157	59,157	59,157

Note: Additional controls included here are daily precipitation (mm), daily maximum temperature (°C) and daily minimum temperature (°C). ***, ** and * represent p<1%, p<5% and p<10% respectively.

2.5.2 Robustness Checks

In this section, we present results of a wide variety of robustness checks and placebo tests to support the findings discussed above.

As a first exercise, we address the issue that some RD specifics may require a judgment call from the econometrician, such as the order of polynomials used in the specification. As pointed by Angrist and Pischke (2014), there is a risk that researchers will cherry pick the model that produces the most appealing results from a subset of RD possibilities when in fact estimates should not be substantially sensitive to marginal changes in the specification. In that regard, we follow Dell (2015) and report supplementary results on how our estimates behave when some aspects of the regression model are changed. Not only we address the possibility of higher degrees of nonlinearity, by including polynomials of higher order, but we also consider different kernels and alternative ad-hoc bandwidths.

Results presented in panel A of table 7 are quite stable across specifications, specially when using the optimal bandwidth selector provided by Calonico et al. (2014) (columns 4-9). Results range from a minimum of 10.3% to a maximum of 14.4% decrease in homicides. When considering ad-hoc bandwidths, results still show significant decreases in homicides (7.3-13.5%), although we lose significance due to a larger variance from the smaller bandwidth (the point estimate, however, is virtually unchanged in columns 1 and 2). In panel B, we report estimates for untreated states. Accordingly, estimates are all statistically insignificant and close to zero.

Table 7: Robustness of RD estimates of the impact of entering DST on homicides: Alternative bandwidths, polynomials and kernels

	Alternative bandwidths			Alternative polynomials			Alternative kernels		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Treated states									
DST_{LATE}	-0.131	-0.135**	-0.072*	-0.103**	-0.119*	-0.125*	-0.144**	-0.125**	-0.130**
	(0.094)	(0.062)	(0.043)	(0.043)	(0.067)	(0.071)	(0.056)	(0.055)	(0.056)
Bandwidth	15	30	60	CCT	CCT	CCT	CCT	CCT	CCT
				19	32	45	25	30	29
Polynomial Order	2	2	2	1	3	4	2	2	2
Kernel	Uni	Uni	Uni	Uni	Uni	Uni	Uni	Tri	Epa
Obs. to the left	990	1,980	3,960	1,254	2,112	2,970	1,650	1,980	1,914
Obs. to the right	1,056	2,046	3,982	1,320	2,178	3,036	1,716	2,046	1,980
Total	2,046	4,026	7,942	2,574	4,290	6,006	3,366	4,026	3,894
Panel B: Untreated states									
DST_{LATE}	0.013	0.015	-0.010	-0.016	0.021	0.013	-0.015	0.000	-0.003
	(0.074)	(0.049)	(0.034)	(0.032)	(0.052)	(0.061)	(0.043)	(0.043)	(0.043)
Bandwidth	15	30	60	CCT	CCT	CCT	CCT	CCT	CCT
				22	33	39	28	32	30
Polynomial Order	2	2	2	1	3	4	2	2	2
Kernel	Uni	Uni	Uni	Uni	Uni	Uni	Uni	Tri	Epa
Obs. to the left	1.440	2,880	5,760	2,112	3,168	3,744	2,688	3,072	2,880
Obs. to the right	1.536	2,976	5,792	2,208	3,264	3,840	2,784	3,168	2,976
Total	2,976	5,856	11,552	4,320	6,432	7,584	5,472	6,240	5,856

Notes: Robust Standard errors in parentheses. ***, ** and * represent p<1%, p<5% and p<10% respectively.

Secondly, for comparison, we consider estimating our main RD specification using the optimal data-driven bandwidth selector proposed by Imbens and Kalyanaraman (2012), although, as argued by Calonico et al. (2014), the bandwidth provided by IK is likely to be larger than the one obtained by CCT due to a first-order bias in the distributional approximation. According to the numbers presented in table 8, this is precisely what we observe, bandwidths are more than twice those presented in tables 1 and 5. Moving to our estimates, column 1 shows the effect of entering DST on homicides occurring during the entire day. As expected, we observe a statistically significant reduction on the number of homicides of 7.5%. This number is substantially larger when we look at hours directly affect by the transition, with homicides decreasing in around 23% around sunset, reinforcing even further our empirical findings. Again, estimates for states that did not adopt the policy are statistically insignificant.

Table 8: RD estimates of the impact of entering DST on Homicides for treated states: Results using Imbens and Kalyanaraman (2012) bandwidth

	All-day	Hours before Sunset	Sunset	Hours after Sunset
		2/1	0/1	2/3
	(1)	(2)	(3)	(4)
		Panel A: Treated states		
DST_{LATE}	-0.0755*	-0.039	-0.234*	0.024
	(0.039)	(0.026)	(0.124)	(0.033)
Bandwidth	IK	IK	IK	IK
	58	55	126	102
Obs. to the left	3,828	3.630	8,316	6,732
Obs. to the right	3,872	3.696	4,818	4,818
Total	7,700	7,326	13,134	10,550
		Panel B: Untreated states		
DST_{LATE}	-0.065	0.001	0.009	0.000
	(0.045)	(0.026)	(0.063)	(0.080)
Bandwidth	IK	IK	IK	IK
	107	96	91	111
Obs. to the left	10,272	9,216	8,736	10,656
Obs. to the right	7,008	7,008	7,008	7,008
Total	17,280	16,224	15,744	17,664

Note: IK is Imbens and Kalyanaraman (2012). Robust Standard errors in parentheses. ***, ** and * represent p<1%, p<5% and p<10% respectively.

Third, since the first day of DST has one hour less than the day before due to the one hour shift in the relationship between clock time and sunset, adjustments to account for 23 instead of 24 hour of records on the outcome are necessary and a common practice in the recent temporal RD literature (Smith, 2016), as we discuss above. To address eventual concerns about this adjustment being a source of bias responsible for the significance of the estimated effects, in columns 1 and 2 of table 9 we provide the following robustness checks: in column 1 we estimate our main RD equation without any hour adjustment; and in column 2 we draw from Barreca et al. (2011) and provide results for a "donut" regression discontinuity in which we exclude observations in the exact day of the transition. If the underlying hypotheses are valid, although we may incur in a loss of precision due to less information available in the sample, we expect to find point estimates similar to those presented in our main specification (Table 1). Results are quite stable and show that homicides decrease by 13.3-15% after transition. These estimates are 5-10 times larger than that observed for untreated states, which are small and statistically insignificant.

A fourth and important issue relates to whether increased mortality around the hours most affected by DST results from a lagged effect of higher crime levels in early hours that ended up being spuriously captured by our regressions. For instance, it is possible that some victims got shot several hours before the ones most affected by DST (around sunset) but ended up dying only during sunset hours due to over crowded hospitals or to reduced hospital staff caused by work shift friction due to DST. Unfortunately, we are not able to match the time of death of deceased patients to the time they were admitted to the hospital in order to check for this potential spurious effect. To tackle this issue, however, we estimate the effect around sunset as in the previous tables while restricting the sample to observations for which victims died on site (i.e., before a possible hospital admission), thus eliminating the possibility of unrelated causes being driving our results. This is a conservative estimate, since we lose observations for which the victim got shot, was quickly admitted to a hospital and ended up dying briefly after admission. Columns 3-5 of table 9 show that the effect is only statistically significant on hours around sunset; we find that the number of victims that where declared dead before being admitted to hospital decrease by 7.2%.

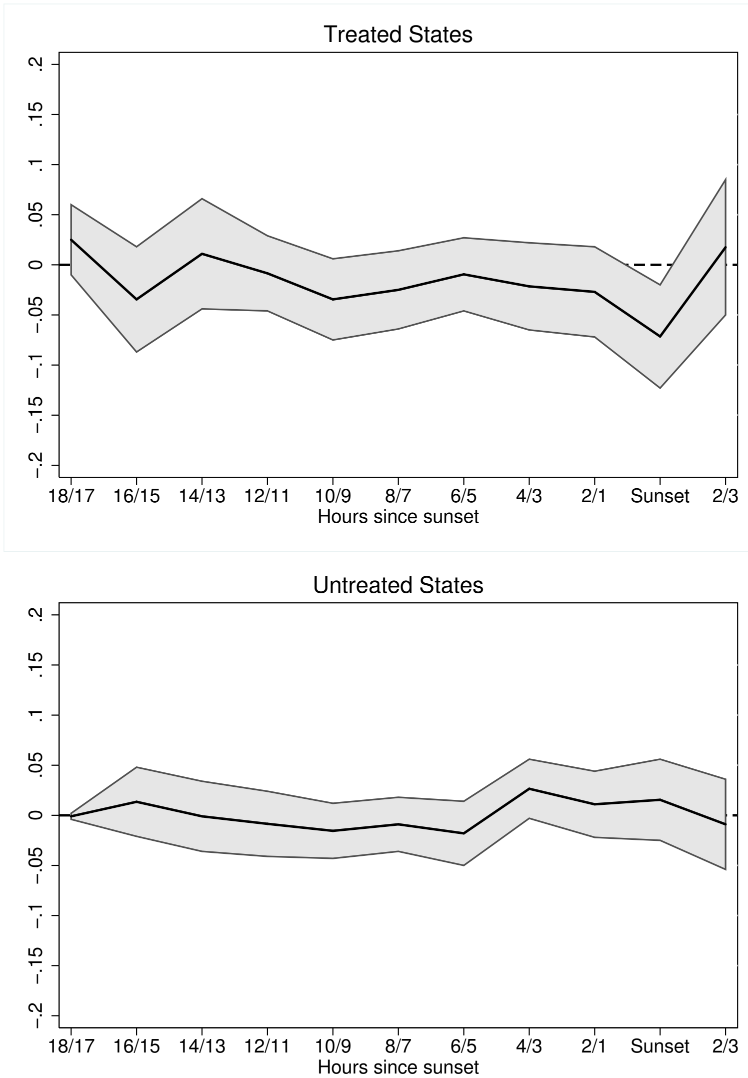
Table 9: RD estimates of the impact of DST on homicides for treated states: additional robustness

	First DST day		Death before admission to hospital			Dummy if municipality had at least a homicide
	No Conversion	No Transition	Hours before Sunset	Sunset	Hours after Sunset	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Treated states						
DST_{LATE}	-0.133**	-0.150**	-0.027	-0.072***	0.018	-0.094*
	(0.058)	(0.061)	(0.023)	(0.026)	(0.035)	(0.050)
Bandwidth	CCT	CCT	CCT	CTT	CCT	CCT
	16	25	32	33	31	25
Obs. to the left	1,584	1,650	2,112	2,178	2,046	1,650
Obs. to the right	1,650	1,650	2,178	2,444	2,112	1,716
Total	3,234	3,300	4,290	4,622	4,158	3,366
Panel B: Untreated states						
DST_{LATE}	-0.015	-0.031	-0.011	0.016	-0.009	-0.001
	(0.042)	(0.061)	(0.017)	(0.021)	(0.023)	(0.037)
Bandwidth	CCT	CCT	CCT	CCT	CCT	CCT
	28	31	23	35	25	28
Obs. to the left	2,688	2,976	2,208	3,360	2,400	2,688
Obs. to the right	2,784	2,976	2,304	3,456	2,496	2,784
Total	5,472	5,952	4,512	6,816	4,896	5,472

Note: CCT refers to the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012). Robust Standard errors in parentheses. ***, ** and * represent $p<1\%$, $p<5\%$ and $p<10\%$ respectively. Hours since sunset are calculated using data on the hour of sunset for each county on the day prior to the beginning of Daylight Saving Time (DST).

Based on this sample of victims that died before a possible hospital admission, for which we have a more precise estimate to hourly homicides, we investigate the possibility of intra-day reallocation of crime across the day. Figure 2.3 plots RD point estimates along with 95% confidence intervals for regressions restricted to two-hour periods around the normalized sunset hour, extending the results presented in table 5 to hours throughout the day. We show that for the treated states there is no significant estimated effect along the day but for the hours around "sunset", which displays a strongly significant decrease in homicides. Specifically, this indicates absence of reallocation of homicidal activity within the day, suggesting the net effect obtained in 1 is exclusively due to the hours affected by ambient light. Note that the same pattern does not hold true for the untreated states, for which none of the estimates is statistically significant.

Figure 2.3: DST entrance transition: estimates by hours since sunset



Note: RD estimates along with 95% confidence intervals for regressions restricted to two-hour periods the normalized sunset hour. Dashed line represents threshold for a null estimated effect.

Still, one may argue that an event not related to DST may cause specific (large) cities to experience unusual shocks to homicides, thus increasing total homicides observed statewide, biasing our estimates. To test for this, we proceed in three ways. A first test involves estimating our RD model using as dependent variable the total number of municipalities that experienced a positive number of homicides in a specific day and state. This specification has the advantage of being less sensitive to outliers, since a large shock to mortality in a large city wont cause changes to the dependent variable. Results considering this outcome are presented in column 6 of table 9 for treated and control states. We observe a decrease of 9.4% on the number of municipalities experiencing at least one homicide per day after transition. For untreated states, the point estimate is -0.001 and we observe no statistical difference between the days before and following transition.

As a second exercise, we assign $I(Transition_{isy} \geq 0)$ to weeks preceding the actual DST transition, an exercise analogous to that proposed in (Autor, 2003) to check for the existence of anticipatory effects. Results are shown in Figure 2.4. If our identifying assumption holds, we expect leads to have no statistical relevance in explaining shifts in homicides. In a dynamic framework, anticipatory effects can naturally arise from expectation adjustments of forward-looking agents (Malani and Reif, 2015). In the present setup and for the channels through which light is argued to affect criminal behavior however, there is no reason to expect anticipation in homicides if not with a positive sign, i.e., individuals anticipating crimes to previous weeks due to the deterrence effect they will face in the future, which is very unlikely. For this exercise, we restrict the sample to the years of 2007, 2009 and 2011, since elections are held on Sundays coincidentally 14 days before DST transition on even years. We estimate regressions assigning placebo DST transitions for one to seven weeks before the actual transition and show absence of any anticipatory effects, regardless of the sign.

Figure 2.4: DST entrance transition: weeks preceding actual transition

Note: RD estimates along with 95% confidence intervals for regressions restricted to weeks preceding actual DST transition. Dashed line represents threshold for a null estimated effect.

As a third and final empirical exercise, we draw on Dell (2010) and Lalive et al. (2014), and design a more conservative comparison between treated and non-treated estimates by looking at those municipalities which are allegedly more balanced in terms of geographical features. Geography is a very influential factor to economic development in Brazil. Regions with milder climate such as the South and Southeast have historically prospered from climate-dependent economic activities such as agriculture and extensive livestock, whereas development in much of the Northeast is still hampered by dry weather and strong droughts. Such conditions are certainly reflected in many municipality-level socioeconomic characteristics such as poverty, illiteracy, unequal educational opportunities and crime.

The idea is to truncate our whole sample of observations conditional on their municipality falling within a certain distance from the geographical border dividing the group of states that are treated from the group that is not treated. The border is drawn as an imaginary line separating the Midwest, Southeast and South regions (treated) from the North and Northeast regions (non treated)[13] We exclude data corresponding to Bahia in 2007 since this state adopted DST in that

[13] See figure 2.1. The imaginary line is the one dividing states in black (RS, SC, PR, SP, RJ, ES, MG, GO, MS, MT, DF) from states in grey and light grey (AC, AM, RR, PA, AP, MA, PI, CE, RN, PB, PE, AL, SE, TO, BA).

year and thus is not eligible to be part of this analysis. By focusing on municipalities that are closer to state borders, we also avoid possible outlying values from large urbanized areas such as state capitals, which are all located further from this imaginary line.

Results are presented in table 10. Each column represents an estimate conditional on a given distance from the boarder (in km). Panels A and C present estimates for treated states. In panel A we use daily data while in panel C we analyze only thos homicides occuring around sunset. We observe that estimates are statistically significant for all distances, either using daily data or hourly data around sunset. In panels B and D we present estimates for untreated states. Accordingly, estimates are statistically insignificant.

Table 10: RD estimates using Distance to DST boarder

Distance	200km	400km	600km	800km	1000km
	(1)	(2)	(3)	(4)	(5)
			Panel A: All-day treated states		
DST_{LATE}	-0.067**	-0.069*	-0.092*	-0.112**	-0.158***
	(0.028)	(0.041)	(0.050)	(0.049)	(0.054)
Bandwidth	CCT	CCT	CCT	CCT	CCT
	28	23	22	23	20
Total	3,762	3,102	2,960	3,102	2,706
			Panel B: All-day untreated states		
DST_{LATE}	0.019	0.012	-0.016	0.026	-0.026
	(0.014)	(0.018)	(0.024)	(0.034)	(0.035)
Bandwidth	CCT	CCT	CCT	CCT	CCT
	35	30	33	27	31
Total	6,816	5,856	6,432	5,280	6,048
			Panel C: Sunset treated states		
DST_{LATE}	-0.026**	-0.048***	-0.059**	-0.061**	-0.097***
	(0.012)	(0.019)	(0.023)	(0.029)	(0.031)
Bandwidth	CCT	CCT	CCT	CCT	CCT
	20	21	25	23	25
Total	2,706	2,838	3,366	3,102	3,366
			Panel D: Sunset untreated states		
DST_{LATE}	0.004	0.003	0.021*	-0.004	-0.003
	(0.009)	(0.018)	(0.012)	(0.023)	(0.022)
Bandwidth	CCT	CCT	CCT	CCT	CCT
	28	27	27	24	34
Total	5,472	5,280	5,280	4,704	6,624

Note: CCT refers to the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012)). All specifications use a second order polynomial and a uniform kernel. Robust Standard errors in parentheses. ***, ** and * represent p<1%, p<5% and p<10% respectively.

2.6 Conclusion

We exploit variation in daylight hours caused by transition from Standard to Daylight Saving Time (DST) to estimate the influence of ambient light on the number of homicides by firearms in Brazil. Using daily data on mortality and regression discontinuity techniques, our framework allows us to carry out both within and between-states comparisons around the transition date. We find robust evidence in favor of a significant decrease to the number of homicides in Brazilian treated states (14%), but no statistical relationship among untreated states. Moreover, comparing treated to untreated states in a Differences-in-Differences approach, we find that homicides by firearms decreased during DST months by about 3%. These effects are persistent especially for dark evening hours that were more affected by the shift in clock time. Finally, robustness checks show that our findings are consistent across alternative polynomials, kernels and bandwidths Angrist and Pischke (2014), and falsification tests find no discontinuity for weeks before the actual transition nor for a set of deaths theoretically not directly affected by DST.

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CHAPTER 3

DAYLIGHT SAVING TIME AND INCIDENCE OF MYOCARDIAL INFARCTION: EVIDENCE FROM A REGRESSION DISCONTINUITY DESIGN[1]

3.1 Introduction

Empirical researchers in epidemiology and related disciplines have for long been interested in estimating the causal effect of circadian variations in the incidence of acute myocardial infarction (hereafter, AMI).[2] Most literature on the subject, however, lack proper identification strategies needed to vouch for causal interpretation. Since randomization is unfeasible, generally owing to ethical constraints, the natural experiment induced by Daylight Saving Time (hereafter, DST) serves as an alternative potentially as good as randomization to identify the effect of interest (Imbens and Lemieux, 2008; Angrist and Pischke, 2014).

This sudden disturbance caused by DST to individuals' daily routine has recently been shown to affect outcomes such as fatal vehicle crashes (Smith, 2014), criminal activity (Doleac and Sanders, 2015) and individual well-being (Kountouris and Remoundou, 2014) using robust econometric techniques. In the medical literature, Janszky and Ljung (2008) provide one of the first recognized piece of evidence relating DST and the incidence of AMI. Since then, other papers

[1]This work has as coauthors Robson Tigre e Breno Sampaio. It was published in Economics Letters. `http://www.sciencedirect.com/science/journal/01651765/136`

[2]According to experimental evidence, circadian variations caused by disturbance in sleep patterns may increase high-sensitivity C-reactive protein (CRP) levels, a stable marker of inflammation, which has been shown to be predictive of cardiovascular morbidity (Meier-Ewert et al., 2004).

using specific-hospital admissions have flourished. Their analyses are usually based on incidence ratios calculated by dividing the incidence just after transition by the incidence two weeks before transition, therefore considering small (and selected) samples, ad-hoc definitions for bandwidths around the discontinuity and strategies that strongly depend on almost ideal unconfoundedness conditions. Our aim in this paper is therefore to provide a well-founded and accurate estimate of the effect of DST on AMI using Brazilian data and a regression discontinuity design.

In Brazil, DST is governed annually by means of Federal enactments based on information of technical reports provided by The Electric System National Operator (ONS). The National Operator indicates the States that should adopt DST as well as the duration of the regime, which usually starts on the third Sunday of each October, when clocks skip forward from 12am to 1am, and extends to midnight of the third Sunday of each February. This policy favors our identification since it provides variation in DST adoption within and across States. Having non-adopter States helps us in designing a robust placebo test, given other factors affecting AMI, besides DST, must evolve smoothly around the transition date.[3]

The remainder of the article is organized as follows. In section 3.2 we present the data set and methodological approaches. Section 3.3 discusses the results. Finally, conclusions are presented in section 3.4.

3.2 Data and Empirical Strategy

In this section we present the data and the empirical strategy we adopt to identify the causal effect of DST on AMI. In particular, we compare the incidence of AMI on the day before entering DST with the incidence of AMI on the first day after its initiation for States that adopted DST using a regression discontinuity (RD) design. For that, consider the following reduced-form model

[3] Deleac and Sanders (2015) and Smith (2014), for example, consider law changes to DST policy in the US to account for endogeneity, since DST occurs simultaneously across 48 states (Arizona and Hawaii do not observe DST) and at approximately the same time each year.

$$\ln AMI_{isy} = \tau I(Transition_{isy} \geq 0) + g(Transition_{isy}) + \varepsilon_{isy} \tag{3.1}$$

where $\ln AMI_{isy}$ is the logarithm of AMI in day i, state s and year y, $Transition_{isy}$ is defined as the number of days to transition to DST, which is equal to zero on the first day after transition and is positive (negative) after (before) then, g is a non-parametric function and ε is a random term. To eliminate persistent day-of-week effects (it might be the case that AMI incidence is higher on weekends than weekdays, for example), State differences and long-term time trends, we follow Smith (2014) and demean the log of AMI incidence by day-of-week, State and year.

We utilize local-polynomial regression-discontinuity point estimators with robust bias-corrected non-parametric confidence intervals (Calonico et al., 2014). Instead of ad-hoc bandwidths adopted by previous literature, we rely on two optimal data-driven bandwidth selectors outlined in Imbens and Kalyanaraman (2012; hereafter, IK) and Calonico, Cattaneo, and Titiunik (2014; hereafter, CCT), and on an alternative cross-validation method, as done by Ludwig and Miller (2007; hereafter, CV). We also propose falsification tests on the basis of (a) a well defined control group, namely untreated States, and (b) placebo diseases that in principle should not be affect by the transition.

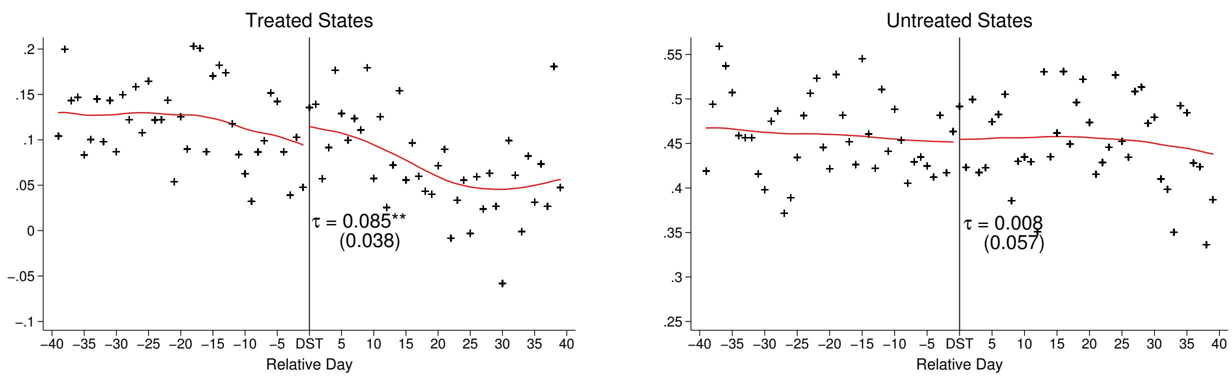
We use individual-level mortality data from the Mortality Information System (SIM), compiled by the Brazilian Ministry of Health. The system provides almost global coverage within the Brazilian territory, containing daily information on cause of death following International Classification of Diseases (ICD) codes. To ensure the best reliability of our data, we consider the years from 2007 to 2012. During this period, all states within Midwestern, Southern and Southeastern region, where light incidence vary the most during the year, adopted DST. Bahia (Northeastern region) and Tocantins (Northern region) adopted DST only in 2011 and 2012, respectively.[4]

[4]Results are unchanged if we exclude these two States.

3.3 Results

Figure 3.1 presents our main results graphically. Demeaned values of log(AMI) by day-of-week, State and year are plotted, centered on the DST transition date. The graph on the left, which consider States that adopted DST, shows that points to the right of the cutoff are slightly shifted above, implying higher incidence of AMI after transition. This is not observed when looking at untreated States.[5] Table 1 presents formal results considering all bandwidth selection procedures for treated and untreated States. After transition, AMI incidence increased by about 7.4-8.5% for treated States.[6] For untreated States, results are precisely zero.

Figure 3.1: DST entrance transition - residuals plot



Note: Residuals are generated from a regression of ln(infarction) on day-of-week, State and year dummies. Fitted lines represent locally weighted regression.

[5]For untreated States, we consider as if DST was adopted in the same time period as treated States. Hence, in this falsification test, we estimate equation 3.1 using data on $\ln AMI_{isy}$ for untreated States.

[6]These numbers are slightly higher than the ones previously obtained in this literature. For example, Janszky and Ljung (2008) find that incidence of myocardial infraction increased by 5.1% using data from the Swedish registry of acute myocardial infarction.

Table 1: RD estimates of the impact of entering DST on incidence of AMI

	Treated States			Untreated States		
	(1)	(2)	(3)	(4)	(5)	(6)
DST_{LATE}	0.085**	0.071**	0.074*	0.008	0.011	0.018
	(0.038)	(0.033)	(0.042)	(0.057)	(0.041)	(0.041)
Bandwidth selector	CCT	IK	CV	CTT	IK	CV
Bandwidth	17	34	26	25	63	76
Obs. to the left	969	1,938	1,482	1,950	4,914	5,928
Obs to the right	1,026	1,995	1,539	2,028	4,992	5,835
Total	1,995	3,933	3,021	3,978	9,906	11,763

Notes: ***, ** and * represent p<1%, p<5% and p<10% respectively.

A common practice in causal inference literature suggests support for the identifying assumption can be offered by estimation of the causal effect of a treatment that, under the hypothesis of identification, is supposed not to have any effect (Imbens, 2004). Not rejecting the hypothesis that a similar effect is zero would not prove that identification is achieved, but would make this assumption considerably more plausible. Below we provide a set of results to support that the discontinuity found in DST entrance transition is not a mere statistical coincidence. First, we show in table 2 that there is no significant effect of leaving DST on incidence of AMI for both treated and untreated states.

Table 2: RD estimates of the impact of leaving DST on incidence of AMI

	Treated States			Untreated States		
	(1)	(2)	(3)	(4)	(5)	(6)
DST_{LATE}	0.065	0.021	-0.032	-0.010	-0.012	-0.010
	(0.059)	(0.047)	(0.034)	(0.064)	(0.053)	(0.049)
Bandwidth selector	CCT	IK	CV	CCT	IK	CV
Bandwidth	10	36	45	20	58	54
Obs. to the left	570	2,052	2,565	1,482	3,784	3,769
Obs to the right	627	2,109	2,622	1,560	4,524	4,290
Total	1,197	4,161	5,227	3,042	8,308	8,059

Notes: ***, ** and * represent p<1%, p<5% and p<10% respectively.

Secondly, we check for causality in the spirit of Granger (1969) and estimate the coefficients of pretreatment or anticipatory effects, a common test in the Differences-in-Differences framework to provide robustness to the results (see Autor, 2003). We assign, therefore, $I(Transition_{isy} \geq 0)$ for Sundays preceding the actual DST entrance transition. If our identifying hypothesis holds, we expect leads to have no statistical relevance in explaining shifts to AMI incidence. Results presented in table 3 confirm this hypothesis.[7]

Table 3: RD estimates of anticipatory impact of entering DST on weeks preceding the actual DST entrance transition

	1 week before	2 weeks before	3 weeks before	4 weeks before
	(1)	(2)	(3)	(4)
DST_{LATE}	0.005	-0.036	0.010	0.017
	(0.039)	(0.038)	(0.036)	(0.035)
Bandwidth	18	20	21	23
Obs. to the left	1,026	1,140	1,197	1,311
Obs to the right	1,083	1,197	1,254	1,368
Total	2,109	2,337	2,451	2,679

Notes: ***, ** and * represent p<1%, p<5% and p<10% respectively.

Finally, we propose a falsification exercise by testing different groups of diseases that should not respond to the transition. Table 4 displays estimates of the impact of entering DST on incidence of neoplasia, viral infections and parasitic diseases. Our placebo tests confirm these diseases evolve smoothly around the transition date.

Table 4: RD estimates of the impact of entering DST on incidence of placebo diseases

	Neoplasia	Viral infections	Parasitic diseases
	(1)	(2)	(3)
DST_{LATE}	0.028	-0.010	0.049
	(0.033)	(0.045)	(0.041)
Bandwidth	16	18	20
Obs. to the left	912	1,026	1,140
Obs to the right	969	1,083	1,197
Total	1,881	2,109	2,337

Notes: ***, ** and * represent p<1%, p<5% and p<10% respectively.

[7] For the sake of brevity and saving space, we provide only results for the CCT bandwidth selector in tables 3 and 4. All other strategies, IK and CV, also yielded statistically insignificant results (available upon request).

3.4 Conclusion

We analyze the effects of acute minor sleep deprivation and circadian rhythm disturbances induced by DST on the incidence of AMI using daily data for Brazil and a regression discontinuity design. We find robust evidence in favor of significant increases (7.4-8.5%) to the number of AMIs in Brazilian treated States, but no statistical relationship among untreated States. These effects are quite large when compared to other factors that affect AMI. For instance, Teo et al. (2006) show the risk of AMI increases by 5.6% for every additional cigarette smoked daily. Using simple back-of-the-envelope calculations, if the effect we estimated persists for the first 7 days after transition, our numbers imply an increase of 196.35 deaths per year.[8] Over a period of 10 years, this yields a social cost ranging from \$7.9 to \$19.6 billion.[9] Finally, our falsification tests show no discontinuities to exist on weeks prior to transition and on a set of diseases theoretically not directly affected by DST.

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[8] We base these calculations on an estimated increase of 8.5% in AMI and an average of 330 deaths per day for all the States in the treated group. The number of deaths caused by DST shift during a period of 7 days each year would be, therefore, $.085\times330\times7=196.35$.

[9] Social cost is based on Kniesner et al. (2012) value of a statistical life, which ranges from \$4 to \$10 million.

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