Does Daylight Saving Save Energy? A Meta-Analysis*

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Abstract

The original rationale for adopting daylight saving time (DST) was energy savings. Modern research studies, however, question the magnitude and even direction of the effect of DST on energy consumption. Representing the first meta-analysis in this literature, we collect 162 estimates from 44 studies and find that the mean reported estimate indicates modest energy savings: 0.34% during the days when DST applies. The literature is not affected by publication bias, but the results vary systematically depending on the exact data and methodology applied. Using Bayesian model averaging we identify the most important factors driving the heterogeneity of the reported effects: data frequency, estimation technique (simulation vs. regression), and, importantly, the latitude of the country considered. Energy savings are larger for countries farther away from the equator, while subtropical regions consume more energy because of DST.

Keywords: Daylight saving time, energy savings, Bayesian model averaging, meta-analysis, publication bias

JEL Codes: C42, Q48

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1 Introduction

As of the year 2016, daylight saving time is used by 77 countries and regions with a combined population in excess of 1.5 billion, making DST one of the most widespread policies in the world. It is also one of the most controversial policies, with dozens of countries and regions having abandoned it in recent decades. While DST has many other effects, in this paper we focus on its impact on energy consumption, which was originally the primary argument advanced in favor of the policy and for which abundant empirical evidence exists. Since the pioneering Ebersole (1974) report, many studies have estimated the effect of DST on energy savings.

Figure 1: Estimates of the DST impact diverge over time

Notes: The figure depicts estimates of the effect of DST on energy consumption reported in individual studies (negative estimates translate to energy savings). The horizontal axis represents the year in which each study was published.

The two major surveys of the literature, Reincke & van den Broek (1999) and Aries & Newsham (2008), show that different researchers obtain substantially different results. One can find empirical evidence in support of energy savings resulting from DST, just as one can find evidence of increased energy demand associated with DST. For example, the most-cited empirical study, Kotchen & Grant (2011), concludes that, contrary to the policy's objective, DST increases energy demand. (The result might be the reason that the study receives so many citations, although it was also published in a prestigious journal, *The Review of Economics and Statistics*.) The survey by Aries & Newsham (2008, p. 1864) concludes that "the existing knowledge about how DST affects energy use is limited, incomplete, or contradictory." As documented by Figure 1, the estimates diverge over time instead of converging to a consensus number. In this paper we propose a systematic and quantitative synthesis of the literature that would allow researchers and the public to take stock of the work on this topic produced over the last four decades.

This study represents, to the best of our knowledge, the first meta-analysis that focuses on the impact of DST on energy consumption. We collect 162 estimates from 44 studies, including research articles, government papers, and energy company reports. The literature implies that, on average, the savings from DST amount to 0.34% of total energy consumption during the days when DST is applied. This mean estimate is consistent with the conclusions of previous (narrative) surveys: Reincke & van den Broek (1999) and Aries & Newsham (2008) place their best estimate of the effect at between 0% and 0.5%. The simple average reported effect is, however, usually a biased estimate of the true effect in economics (Doucouliagos & Stanley, 2013): the distribution of the estimates is often truncated due to publication bias, and the size of the effect is typically driven by study design.

When researchers or journal editors treat statistically significant estimates or estimates consistent with the conventional view more favorably, the distribution of estimates in the literature becomes biased. Random sampling errors occasionally cause estimates to have the "wrong" sign, but suppressing these estimates on a global scale may seriously distort the mean reported effect. For example, Stanley (2005) shows that the price elasticity of water demand is exaggerated fourfold due to publication selection. Nevertheless, unlike most other fields of empirical economics, the DST literature does not exhibit this bias, as we show in the paper. Negative, insignificant, and positive results are treated in a similar way by researchers, editors, and referees. We find, however, that the design of the study has important and systematic effects on the results.

Belzer *et al.* (2008) illustrate how researchers can use different data sets and methods to estimate the DST effect. We explore this influence of data, method, and even publication characteristics on the estimated coefficients. Using Bayesian model averaging we address model uncertainty and find that, among the 14 explanatory variables we codify, several are particularly influential: the choice of the difference-in-differences approach to estimate savings (vs. simple regression, simulation, or extrapolation), the choice of data frequency, and the impact factor of the journal in which the study was published, which we employ as a proxy for unobserved quality aspects. Importantly, we also find that the estimated energy savings increase with higher latitudes (which translates to more savings for countries farther away from the equator).

Our results suggest that the effect of latitude can not only offset the effect of various estimation methods but can also easily outweigh the mean estimated savings and imply increased energy consumption due to DST for countries closer to the equator. The DST policy makes little sense when the amount of daylight does not vary substantially during the year, and in this case the policy constitutes a shock that may well have unintended consequences for energy consumption. In theory, the relationship between latitude and energy savings from DST should be concave because DST also makes little sense near the poles where the difference between winter and summer daylight hours is too large. The human population, however, is concentrated in the subtropical and temperate climate zones, and the estimates in our sample reflect countries and regions of the corresponding latitudes. The positive relationship between latitude and energy savings can thus be regarded as a linear approximation of the underlying relationship.[1]

The remainder of the paper is organized as follows. Section 2 describes the data collection process and the basic properties of the data set. Section 3 tests for publication selection bias in the literature. Section 4 explores country and method heterogeneity in the estimated DST effects and constructs best practice estimates for different countries. Section 5 concludes the paper. An online appendix at `meta-analysis.cz/dst` provides the data and code that will allow other researchers to replicate our analysis.

2 Data

Studies estimating the energy consumption effect of a change from standard time to daylight saving time typically employ econometric analysis. In general, the authors estimate the following model:

$$ln\text{Consumption}_t = \alpha + DST \cdot \text{Treatment effect}_t + \text{Controls}_t + \epsilon, \tag{1}$$

[1] We experimented with adding the square root of latitude and of the number of daylight hours in the Bayesian model averaging analysis, but these variables were not important in any specification.

where *Consumption* is the average energy consumption during time t for a given hour, day, and year. The variable *Treatment effect* is a dummy variable for a selected treatment group and usually equals 1 for all hours when daylight saving time applies. *Controls* are explanatory variables that reflect seasonality and holidays, weather (precipitation, humidity, temperature, wind, and pressure), the intensity of sunlight, heterogeneity among consumption units, and other specific effects such as economic activity or oil prices, possibly including interaction terms and lags. The error term is denoted by ϵ.

From the studies reporting the DST effect we collect the treatment coefficient *DST* from (1). This coefficient represents the effect of daylight saving time on energy consumption, or the difference in electricity consumption for a particular time period between the treatment group and the control group. These groups might be defined differently, for example as the period before the start and end of DST versus the period after the start and end of DST, the period when DST is not observed versus the period when DST is in place, the period when DST is observed versus the period to which DST is extended, or the period of midday and midnight hours versus the period of morning and evening hours. Multiple studies examine the pattern in energy use before and after the spring and fall time change (for example Kandel & Metz, 2001). Other studies, such as Mirza & Bergland (2011) and Kotchen & Grant (2011), examine the differences in consumption for hours unaffected and affected by the DST policy. Belzer *et al.* (2008) examines the impact of an extended DST policy.

Apart from econometric analysis, researchers can use simulation techniques to estimate the effect of DST on energy consumption. Here the authors usually construct a model of energy flows within different representative buildings and attempt to extrapolate this model to the country level. Such an approach entails multiple assumptions and simplifications, and it is thus more challenging to incorporate it into the meta-analysis framework. Despite the difficulty, we include these estimates in our analysis following the approach of Havranek *et al.* (2015b), who apply meta-analysis to simulation-based estimates of the social cost of carbon and show substantial publication bias in the literature.

Some studies report estimates incomparable with the rest of the literature. Our criteria for including studies in the meta-analysis are that 1) the study reports the effect of a change from standard time to daylight saving time (the effect of a one-hour clock shift during summer

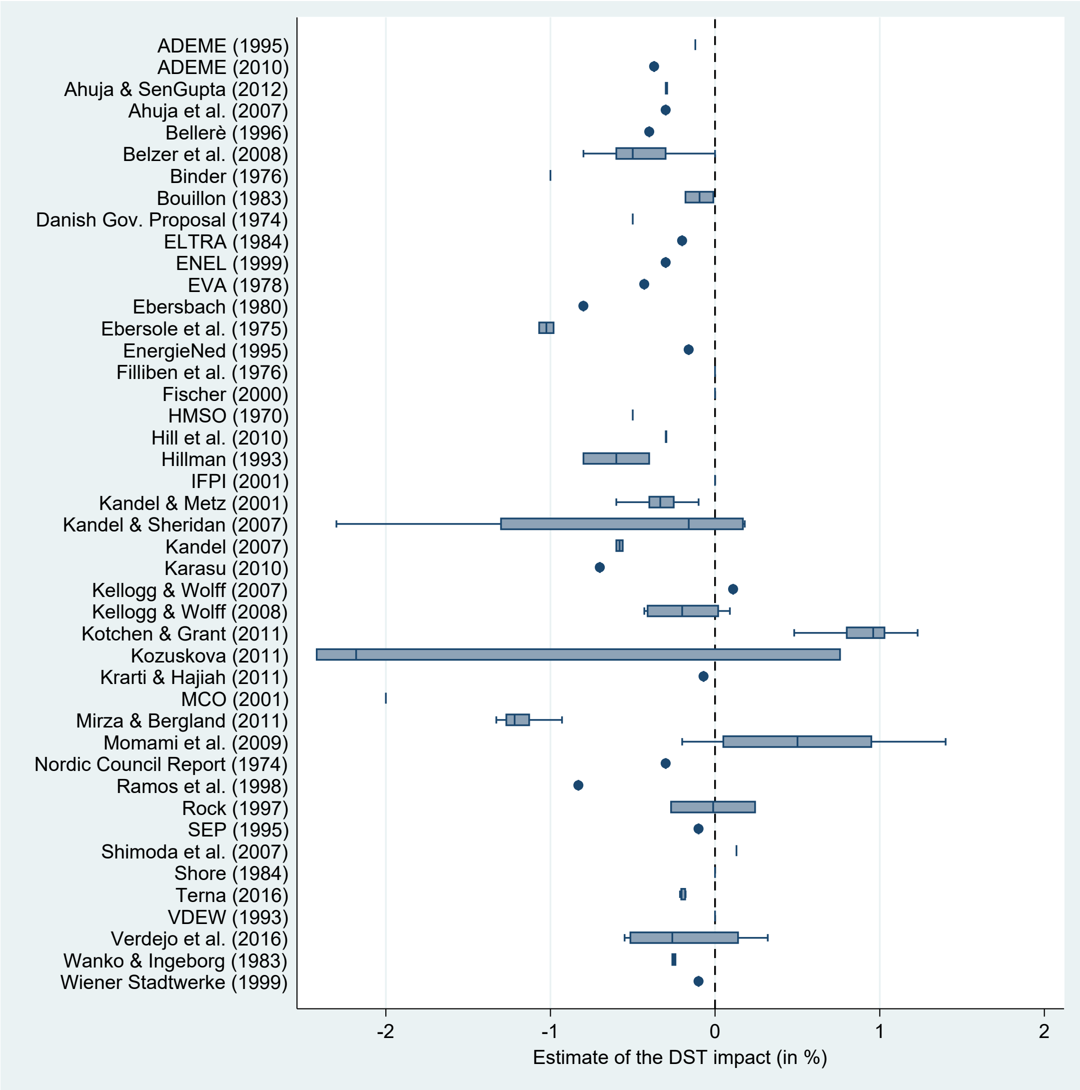
Table 1: Studies used in the meta-analysis

Independent studies:		
ADEME (2010)	Hill *et al.* (2010)	Krarti & Hajiah (2011)
Ahuja & SenGupta (2012)	Hillman (1993)	Mirza & Bergland (2011)
Ahuja *et al.* (2007)	HMSO (1970)	MCO (2001)
Belzer *et al.* (2008)	IFPI (2001)	Momani *et al.* (2009)
Bellere (1996)	Kandel (2007)	Nordic Council (1974)
Binder (1976)	Kandel & Metz (2001)	Ramos & Diaz (1999)
Bouillon (1983)	Kandel & Sheridan (2007)	Rock (1997)
Danish Government Report (1974)	Karasu (2010)	Shimoda *et al.* (2007)
Ebersbach & Schaefer (1980)	Kellogg & Wolff (2007)	Shore (1984)
Ebersole *et al.* (1975)	Kellogg & Wolff (2008)	Terna (2016)
Filliben *et al.* (1976)	Kotchen & Grant (2011)	Verdejo *et al.* (2016)
Fischer (2000)	Kozuskova (2011)	Wanko & Ingeborg (1983)
Independent estimates from Reincke & van den Broek (1999):		
ADEME (1995)	EnergieNed (1995)	VDEW (1993)
ELTRA (1984)	EVA (1978)	Wiener Stadtwerke (1999)
ENEL (1999)	SEP (1995)	

months), 2) the study reports the estimate in a way that enables us to extract an estimate in percent per day for each day the DST policy is implemented, and 3) the study focuses on electricity consumption (there are few estimates for other energy sources). To avoid comparing apples to oranges, we have to exclude several studies or individual estimates within studies. For example, Littlefair (1990), Crowley *et al.* (2014), Fong *et al.* (2007), and Rock (1997) report the effect of double DST; Kotchen & Grant (2011) report several estimates of the effect of a change from DST to standard time. Some studies only report lighting energy savings, such as Fong *et al.* (2007) or Rajaram & Rawal (2011); Pout (2006) does not include electricity use for lighting in her analysis. Other studies (for example Innanen & Innanen, 1978; Basconi, 2007; Sarwar *et al.*, 2010; Pellen, 2014) report DST savings in such detail or manner that we were unable to recalculate them to be comparable with the rest of the sample.

Our final data set comprises 162 estimates taken from 44 independent studies reported in Table 1. We take advantage of the previous literature surveys on the energy savings from DST by Reincke & van den Broek (1999) and Aries & Newsham (2008), which identify the major studies on the DST effect published prior to 2008. Additionally, we search Google Scholar for studies published thereafter; the search query is available in the online appendix at `meta-analysis.cz/dst`. We identify 34 primary sources, i.e., studies directly estimating the DST effect (either as a treatment coefficient *DST* from (1) in the regression framework or as

Figure 2: Estimates of the DST savings effect vary across and within studies

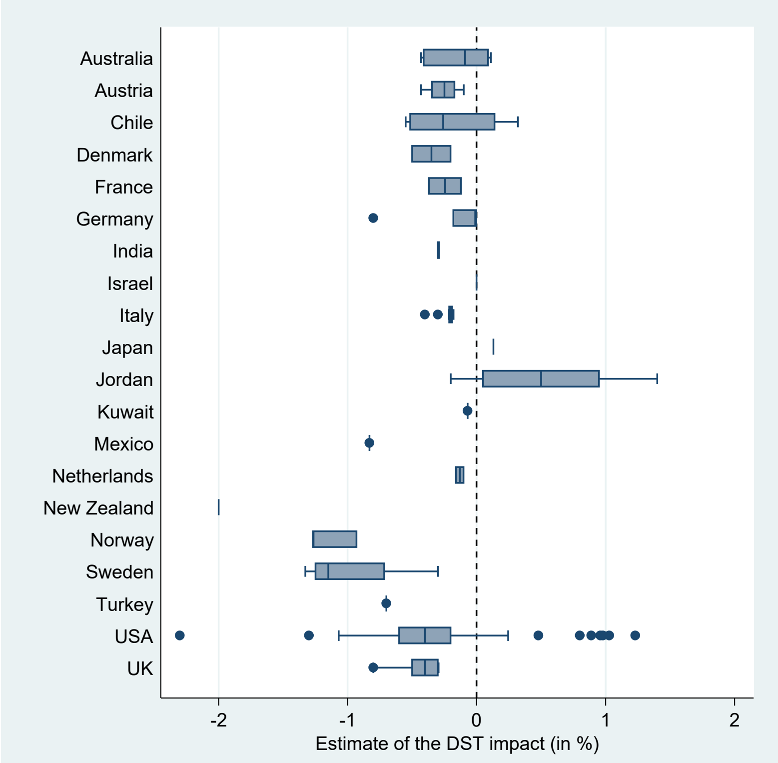


Notes: The figure shows a box plot of the estimates of the DST effect on energy savings reported in individual studies. Negative estimates denote energy savings. Outliers are excluded from the figure but included in all statistical tests.

a result of simulation or extrapolation) and one secondary source, Reincke & van den Broek (1999), who report the results of 8 independent unpublished studies with DST estimates collected from interviews with public or private energy companies. We also inspect the references of all the studies in our sample published after 2008 to determine whether we missed papers. We add the last study on April 30, 2016.

We collect all the estimates reported in the studies. Therefore, we have an unbalanced panel data set, since different studies provide a different number of estimates. Some researchers conducting meta-analysis prefer to collect only one representative estimate from each study, but

Figure 3: Some countries may consume more energy because of DST



Notes: The figure shows a box plot of the estimates of the DST effect on energy savings reported for different countries. Negative estimates denote energy savings. Outliers are excluded from the figure but included in all statistical tests.

we follow Stanley (2001, p. 135), who suggests that it is "better err on the side of inclusion." Figure 2 shows that there is substantial heterogeneity in the estimates between and within studies, which might stem especially from the differences in methods and data. Moreover, Figure 3 shows the heterogeneity of estimates between different countries. It follows that it is important to control for the variations in the design of the study. Thus, we collect 16 aspects of study design for all estimates (details can be found in Table 4 of Section 4). The final data set is available online at `meta-analysis.cz/dst`.

Table 2 reports the mean of the DST savings' estimates for different groups of study design characteristics. On the left-hand side we report simple averages; on the right-hand side the averages are weighted by the inverse of the number of observations reported per study. This type of weighting does not allow large studies to dominate the mean. Assigning each study the same weight yields an overall mean estimate of -0.34, which suggests energy savings of 0.34 percent of total electricity consumption during the days when the daylight saving policy is applied. The 95% confidence interval of $(-0.43, -0.26)$ indicates considerable uncertainty

Table 2: DST effects vary across subsets of data, method, and publication characteristics

		Unweighted			Weighted		
DST est. by group	No. of observations	Mean	95% conf. int.		Mean	95% conf. int.	
Data characteristics							
Hourly data	139	-0.361	-0.428	-0.295	-0.335	-0.412	-0.258
Daily data	15	-0.687	-1.220	-0.155	-0.654	-1.099	-0.209
Main estimate	67	-0.250	-0.410	-0.091	-0.338	-0.475	-0.202
Europe	43	-0.474	-0.651	-0.297	-0.386	-0.527	-0.245
USA	94	-0.341	-0.441	-0.241	-0.307	-0.436	-0.178
Design of the analysis							
Regression analysis	117	-0.395	-0.495	-0.295	-0.418	-0.544	-0.293
Simulation analysis	21	-0.241	-0.408	-0.073	-0.259	-0.395	-0.123
Other analysis	24	-0.120	-0.384	0.144	-0.320	-0.550	-0.091
Residential consumption	17	0.219	-0.132	0.570	-0.117	-0.417	0.184
Commercial consumption	145	-0.399	-0.480	-0.319	-0.382	-0.471	-0.293
Lighting consumption	7	-0.337	-0.621	-0.053	-0.304	-0.586	-0.021
Difference-in-differences	94	-0.407	-0.520	-0.294	-0.449	-0.619	-0.279
Publication characteristics							
Journal publication	41	-0.026	-0.250	0.199	-0.121	-0.284	0.043
Unrefereed publication	121	-0.439	-0.517	-0.361	-0.446	-0.544	-0.348
Observations with SE	101	-0.402	-0.518	-0.286	-0.411	-0.577	-0.244
All observations	162	-0.334	-0.419	-0.250	-0.343	-0.429	-0.257

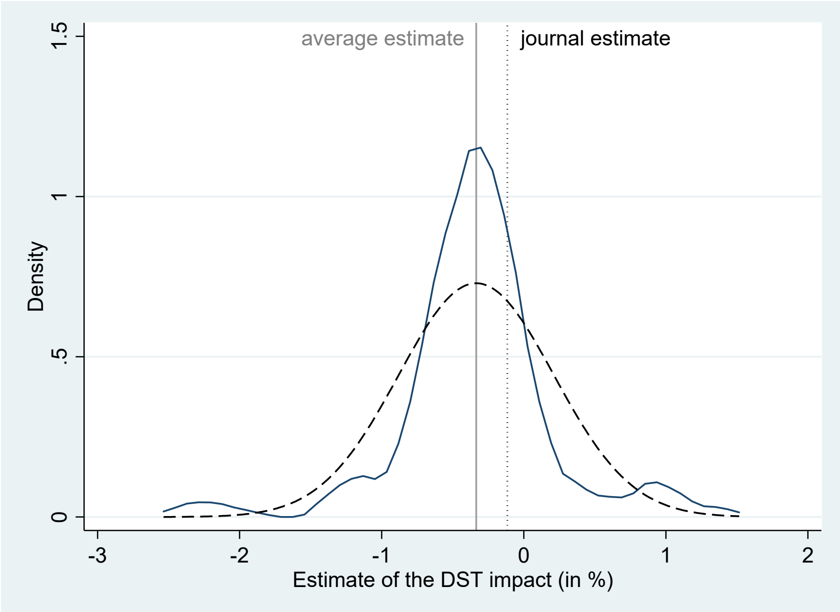
Notes: The table presents mean estimates of the DST effect on energy consumption (in %) for selected groups of data, method, and publication characteristics (see details in Table 4). On the right-hand side of the table the DST estimates are weighted by the inverse of the number of estimates reported per study. SE = standard error.

around the mean. This finding is consistent with existing surveys: Reincke & van den Broek (1999) and Aries & Newsham (2008) place the mean estimate between 0% and 0.5%.

Table 2 documents that the means of DST energy savings effects vary substantially across data and method choices. We observe that using hourly data instead of daily data in the analysis tends to reduce the estimate of savings. We also observe that the simulated results tend to be smaller than those obtained by regression or other means of analysis. When a study estimates the savings effect in the residential sector alone, we observe that the upper confidence interval of our estimate suggests energy penalties instead of energy savings. The difference-in-differences approach seems to be associated with higher estimated savings.

Figure 4 depicts the distribution of the estimates of DST savings. The distribution is approximately symmetrical, and the mean estimate of -0.33 is very close to the median estimate of -0.3, suggesting that there are not many outlying observations; thus, we do not need to exclude any estimates from our analysis. From Table 2 we see that the estimates that the authors prefer tend to be close to the average (when we assign each study the same weight). Nevertheless, studies in peer-reviewed journals appear to publish smaller estimates (see Table 2), which might indicate that factors other than the methodological reasons we can directly observe are responsible for the conservative estimates—it is an indication of potential publication selection.

Figure 4: Journal publications report smaller savings from DST



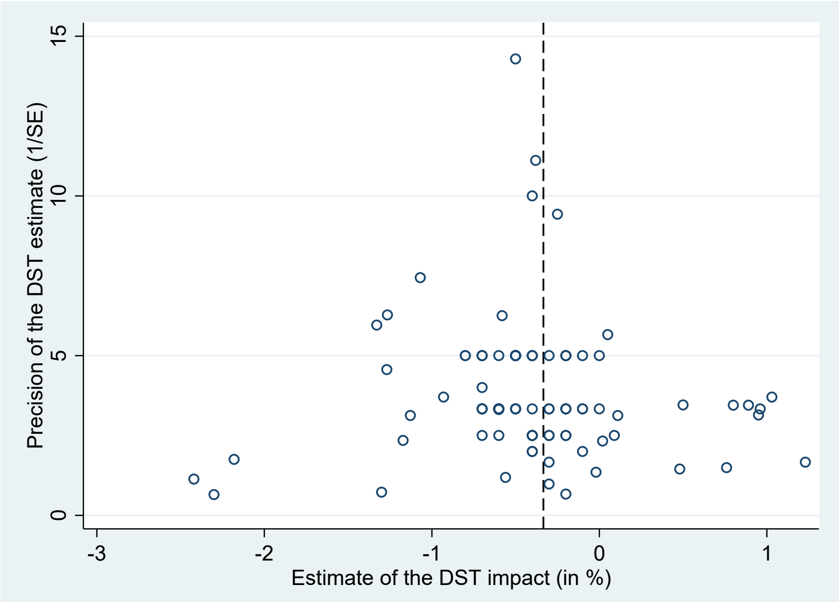
Notes: The figure depicts the Epanechnikov kernel density of the DST effect estimates. The dashed curve denotes the normal distribution density, the solid vertical line denotes sample mean of the DST estimate, and the dotted vertical line denotes the mean of the DST estimates coming from journal publications.

3 Publication Bias

The preference of authors and editors for a certain magnitude or statistical significance of an estimate is a common phenomenon in the economics literature (Doucouliagos & Stanley, 2013). The literature on the effects of DST on energy consumption is unique in the character of publication outlets: many of the estimates come from the reports of government or energy companies. These institutions may have different reasons to prefer higher or lower estimates; there is, however, little reason for the authors from research institutes to succumb to such bias. Statistically insignificant estimates, however, might be more easily overlooked, leading to the the so called file-drawer problem. Some cases of publication bias have been previously documented even in the field of energy economics (for example, Havranek *et al.*, 2012; Reckova & Irsova, 2015; Havranek & Kokes, 2015; Havranek *et al.*, 2015b).

The so-called funnel plot is one of the most common tools used to detect publication bias. It is a scatter diagram with the estimate of the effect on the horizontal axis and the precision of the estimate (the inverse of the standard error) on the vertical axis (see Stanley, 2005). For the majority of the estimates we consider, the authors report t-statistics and, therefore, assume the

Figure 5: Funnel plot suggests little publication bias



Notes: The figure depicts a funnel plot of the estimates of the DST effect. In the absence of publication bias, the funnel should be symmetrical around the most precise estimates of the DST effect on energy savings. The dashed vertical line denotes the mean of the estimates. Outliers are excluded from the figure but included in all statistical tests.

estimated coefficient and its standard error to be independent of one another.[2] This property implies there should be no relationship between an estimate and its standard error. Thus, regardless of the magnitude of the true effect, the estimates in the plot should vary randomly and symmetrically around the true effect. With decreasing precision, the estimates become more dispersed, thus creating an inverted funnel.

From Figure 5 we conclude that there is little evidence of publication bias in the literature on DST energy savings: when selection process is related to the magnitude of the effect, the funnel plot becomes asymmetrical; when the selection process favors statistical significance, the funnel becomes hollow and wide. We observe that Figure 5 does not exhibit either of these properties: the funnel is not hollow and is relatively symmetrical. Nevertheless, the funnel plot is only a simple visual test, and the dispersion of the estimates might suggest the presence of heterogeneity; therefore, we still need more rigorous tests to support our claim that there is no bias present in the literature.

[2] Some estimates of DST savings result from simulations, and thus the ratio of the estimate to its standard error does not follow the t-distribution. We use the approach of Havranek *et al.* (2015b) and account for the simulated estimates and their standard errors (even for the estimates with asymmetric confidence intervals we compute approximated standard errors, such as in Havranek, 2015). This approach yields 11 additional observations, but it is worth noting that our results would hold even if these estimates were excluded.

As we have noted, in the absence of publication bias the estimates of DST savings and their standard errors should be uncorrelated (Stanley, 2005):

$$DST_{ij} = DST_0 + \beta \cdot SE(DST_{ij}) + u_{ij}, \tag{2}$$

where DST_{ij} and $SE(DST_{ij})$ are the i-th estimates of the effect of DST on energy savings and its standard error reported in the j-th study and u_{ij} is the error term. DST_0 represents the true effect beyond potential publication bias captured by β. If there were no publication bias present in our sample, β would equal zero. In Table 3 we show that various versions of this test corroborate our conclusion of insignificant publication bias in the DST literature.

Table 3: Funnel asymmetry tests show no publication bias

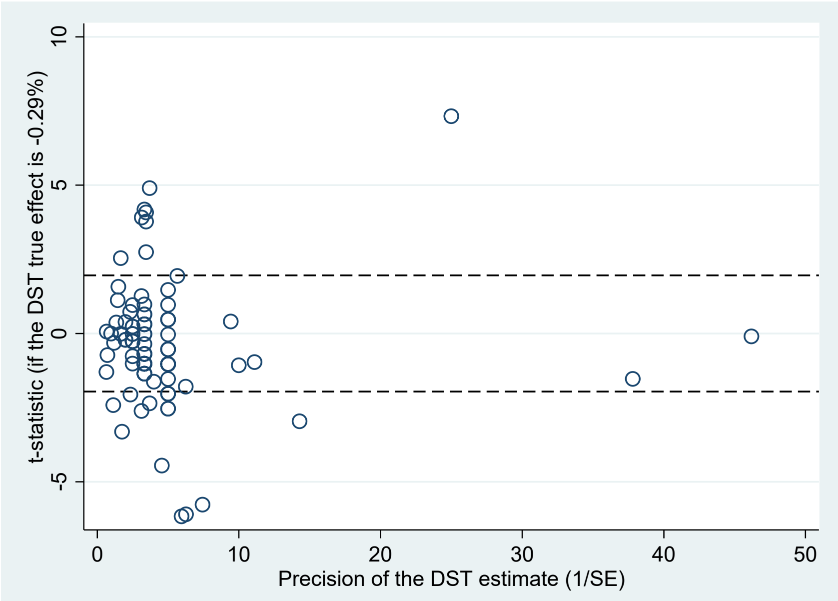
	OLS	FE	BE	Country	ME	IV
SE (*publication bias*)	-0.410	-1.217	-0.410	-0.496	-0.449	0.226
	(0.265)	(0.790)	(0.757)	(0.805)	(0.688)	(1.088)
Constant (*true effect*)	-0.293***	-0.222***	-0.294***	-0.278***	-0.291***	-0.445*
	(0.000778)	(0.0700)	(0.00812)	(0.0459)	(0.00731)	(0.243)
Observations	101	101	101	101	101	90

The table presents the results of a regression $DST_{ij} = DST_0 + \beta \cdot SE(DST_{ij}) + u_{ij}$, where DST_{ij} and $SE(DST_{ij})$ are i-th estimate of the effect of DST on energy savings and its standard error reported in the j-th study. The model is estimated by weighted least squares with the inverse of the reported estimate's standard error taken as the weight. OLS = ordinary least squares, FE = study-level fixed effects, BE = study-level between effects, Country = country-level fixed effects, ME = study-level mixed effects, and IV = instrumental variable estimation, where the instrument for the standard error is the number of observations (if the study is based on regression analysis). Standard errors in parentheses are clustered at the study and country level (two-way clustering follows Cameron *et al.*, 2011).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The first column of Table 3 presents the baseline model of the funnel asymmetry test from (2). The coefficient β, estimated by OLS, is not statistically significant (p-value = 0.12), and the constant DST_0 places the true effect of daylight savings at approximately -0.29%. In the second column we add study-level fixed effects to the baseline specification. Using within-study variation for identification only marginally decreases the true effect, as does using within-country variation in the fourth column. The estimated bias becomes even less significant in other specifications: the model in the third column uses between-study variation and provides nearly the same mean effect as our baseline model. The mixed effects model in the fifth column is convenient for our unbalanced panel, since it employs restricted maximum likelihood and thus essentially assigns each study the same weight; the results are again similar to the baseline case. In the last column, we use the number of observations as an instrument for the standard error.

The instrumental variable estimation is naturally less precise, but the result complies with the rest of the analysis: there is no publication bias present in the literature on energy savings from daylight saving time.

Figure 6: Galbraith plot suggests some publication selection or heterogeneity



Notes: The horizontal black lines form the boundary of the (−1, 96; 1, 96) interval, which should not be surpassed in more that 95% of cases if there is no publication bias related to statistical significance and no heterogeneity. Outliers are excluded from the figure but included in all statistical tests.

As a complementary robustness check we depict the Galbraith plot (Galbraith, 1988), which specifically concentrates on the likelihood of reporting significant results. It is a funnel plot rotated 90 degrees and adjusted to remove heteroskedasticity (Stanley, 2005). We follow Havranek (2010) to define the adjusted t-statistics $T(DST_{ij})$:

$$T(DST_{ij}) = \frac{DST_{ij} - DST_0}{SE(DST_{ij})}, \qquad (3)$$

where DST_0 represents the true effect estimated by the funnel asymmetry test and DST_{ij} represents the i-th estimate of the daylight saving effect with $SE(DST_{ij})$ as the corresponding standard error reported in the j-th study. For DST_0, we employ the baseline true effect from the first column of Table 3, −0.293, and plot the final statistics in Figure 6. If there is no systematic relationship between the effect and the precision, the observations should be randomly distributed around zero and the computed t-statistic should not be outside the interval (−1.96, 1.96) in more than 5% of cases. Our results indicate that nearly 24% of the estimates would be significant if the true effect were 0.293%. Such a result could create some formal grounds for the presence of publication bias related to the significance of estimates. Nevertheless, Figure 6 merely shows the presence of excess variation since the extreme values of t-statistics, on average, offset one another (Stanley, 2005), and therefore the mean effect is not biased. Moreover, the value of the true effect in Table 3 also needs to be challenged. There could be possible dependencies in study design and country heterogeneity that affect our previous estimates, and we will address these issues in the next section.

4 Heterogeneity

4.1 Variables and Estimation

We have seen from Figure 2 and Figure 3 that the estimates of the DST effect vary considerably, but we have not been able to explain the variance by sampling error and selective reporting. There is, however, another type of variation that might have a systematic influence on the estimated effects of DST. Aries & Newsham (2008) note that different studies estimate the DST effect using different data sets and methods. We will attempt to explain these variations using meta-regression analysis (as in Havranek & Irsova, 2011, who show how broadly estimates of an economic effect can vary across methods and countries). Since we do not observe publication bias in our sample, we remove the standard error from (2) and replace it with explanatory variables related to data and methodology. In so doing, we eliminate the apparent heteroskedasticity affecting the equation and control for heterogeneity among the estimates.

Table 4: Description and summary statistics of regression variables

Variable	**Description**	**Mean**	**SD**	**WM**
Daylight savings	The estimate of the impact of daylight saving time (DST) on energy consumption in % per day of DST.	-0.334	0.547	-0.343
SE	The estimated standard error of DST savings.	0.339	0.266	0.400
Data characteristics				
Data period	The number of years used in the estimation.	2.30	1.72	2.12
Main estimate	= 1 if the estimate is preferred by the authors of the study.	0.41	0.49	0.79
Hourly data	= 1 if the data are examined on hourly or higher than hourly granularity.	0.09	0.29	0.14
Daily data	= 1 if the data are examined on a daily basis.	0.09	0.29	0.14
Daylight hours	Average time between sunrise and sunset on the longest day for the country or region under examination (Source: U.S. Naval Observatory Astronomical Applications Department).	15.19	1.26	15.57

Continued on next page

Table 4: Description and summary statistics of regression variables (continued)

Variable	Description	Mean	SD	WM
Europe	= 1 if European countries are examined.	0.27	0.44	0.52
USA	= 1 if US data are examined.	0.58	0.50	0.23
Design of the analysis				
Regression analysis	= 1 if the primary study is based on regression analysis.	0.72	0.45	0.39
Simulation analysis	= 1 if the study is based on simulation.	0.13	0.34	0.26
Difference-in-diff.	= 1 if the difference-in-differences approach is employed.	0.58	0.50	0.21
Residential cons.	= 1 if only residential consumption is examined.	0.10	0.31	0.15
Lighting cons.	= 1 if total energy savings are reported as a result of lighting reduction.	0.04	0.20	0.13
Publication characteristics				
Publication year	The publication year of the study (base = 1970).	34.8	9.5	27.1
Journal article	= 1 if the study was published in a peer-reviewed journal.	0.25	0.44	0.32
Impact factor	The recursive RePEc impact factor of the outlet.	0.07	0.26	0.05
Citations	The logarithm of the total number of citations of the study in Google Scholar.	1.91	0.94	1.60

Notes: SD = standard deviation. WM = mean weighted by the inverse of the number of observations reported per study. All variables except for citations and the impact factor are collected from studies estimating the DST effect (the search for studies was terminated on April 30, 2016). Citations are collected from Google Scholar and the impact factor from RePEc. The data set is available at `meta-analysis.cz/dst`.

The explanatory variables capturing the variation in data and methodology are listed in Table 4; the table provides the definition of these variables and their summary statistics. The last column of the table presents the mean of the variables weighted by the inverse of the number of observations extracted from a study. We divide the variables into three groups. First, we collect information on data characteristics capturing the data set and geographical specifics. Second, we collect information on the design of the analysis to capture methodological differences. Third, we collect information on publication characteristics, such as the journal impact factor. Our intention here is not to provide an exhaustive survey of the methods used in the DST literature but to identify the main reasons for the heterogeneity affecting the estimates.

Data characteristics We consider the number of years examined in a study as a potentially useful explanatory variable: it might show that savings become more apparent in the long run when firms and households become better adapted to the policy. We also control for what the authors find to be their own preferred estimate in a particular study, which might indicate whether their own best-practice estimate is systematically different from the rest of the reported results. Another source of heterogeneity could be the granularity of the data: the information in daily data is less detailed than the information in hourly data, for which researchers directly observe changes in consumption during the morning and evening hours. We capture the country-specific differences by including the variable for the duration of sunlight. Specifically, we identify the average coordinates of the place, which relates either to the country or the city for which the daylight savings effects were estimated. For this geographical centroid, we identify the longest day of 2016 and its respective number of sunlight hours. We also include dummy variables for the United States and European countries.

Design of the analysis DST estimates come either as a result of econometric analysis, simulation, or another type of analysis such as extrapolation or comparison. Among the econometric analyses, which generate more than 70% of our estimates, we observe frequent use of the difference-in-differences technique. The difference-in-differences approach accounts for differences between a control group (a time period that should not be affected by DST) and a treatment group (a time period that should be affected by DST). The set of other moderator variables included in the regression analysis also differs, as does the functional form. In most cases, a log-level model is employed obtain the difference-in-differences estimate; nevertheless, for example, Shore (1984), Basconi (2007), and Kandel & Sheridan (2007) employ a level-level model directly examining the magnitudes of energy consumption only (the elasticity is then computed using sample means). The level-level model is, however, scarce in our data set, and therefore, we do not add a corresponding dummy since it would display very little variation.

Nearly 30% of our estimates come from a type of analysis other than regression. Typically, these estimates are produced by simulation or by more or less sophisticated extrapolation. The simulations vary in their specification; moreover, the specification is not always reported in detail. Assumptions of the simulations are derived either from regression analysis, simple historical data analysis, or survey findings. The control variables are then similar to those specified in regression analysis with the exception that buildings and households are modeled in much greater detail. Therefore, the obvious benefit of simulation is that it is able to investigate the energy consumption patterns in greater depth; however, researchers must be more confident in the correctness of the model specification. Extrapolation is usually based on shifts in the daily load curves. In comparison with the previous approaches, extrapolation is somewhat less sophisticated because this type of analysis makes it more difficult to control for other relevant influencing factors.

We also control for the type of end-use and the type of end-user of the electricity considered in an analysis. The largest share of electricity consumption goes to lighting, heating, cooling, and appliances. Our original intention was to collect the estimates on overall consumption subsuming all these energy uses. Due to the small sample size for the individual categories, however, we decided to only retain in our data set those observations for which the DST estimate is defined as the effect of lighting electricity on overall electricity consumption, but we control for this aspect of methodology to determine whether these estimates differ from more general estimates of DST energy savings. Moreover, some researchers only estimate the DST effect for residential areas, while the rest of the literature does not differentiate between residential and business consumption. As the daily consumption cycle for households differs from that for commercial or industrial buildings, we also control for the type of end-customer assumed in an analysis.

Publication characteristics There might be methodological advances in the literature that we are not able to capture directly by method variables (the number of studies and the number of estimates is not large). We employ several publication characteristics as proxies for such aspects. For example, advances in methodology should be captured by publication year. We also use several variables that control for publication quality, which may also reflect unobserved aspects of data and methods. We examine whether studies yield consistently different results when they are published in a peer-reviewed journal and in a journal with a higher or lower impact factor and whether the number of citations is correlated with the result.

In the end we have 14 aspects of study design. Ideally, we would like to regress all these explanatory variables on the estimates of the DST effect we collected. Having a relatively large number of variables, however, we face the problem that some of them might prove redundant—in other words, there is substantial model uncertainty. Redundant variables inflate the variance of all other parameters, and researchers usually attempt to eliminate the insignificant variables one by one. Such a general-to-specific method is not statistically valid because t-tests are not designed to be run conditionally on one another. Following Havranek *et al.* (2015a) and a plethora of studies that address model uncertainty in economics, we employ Bayesian model averaging instead.

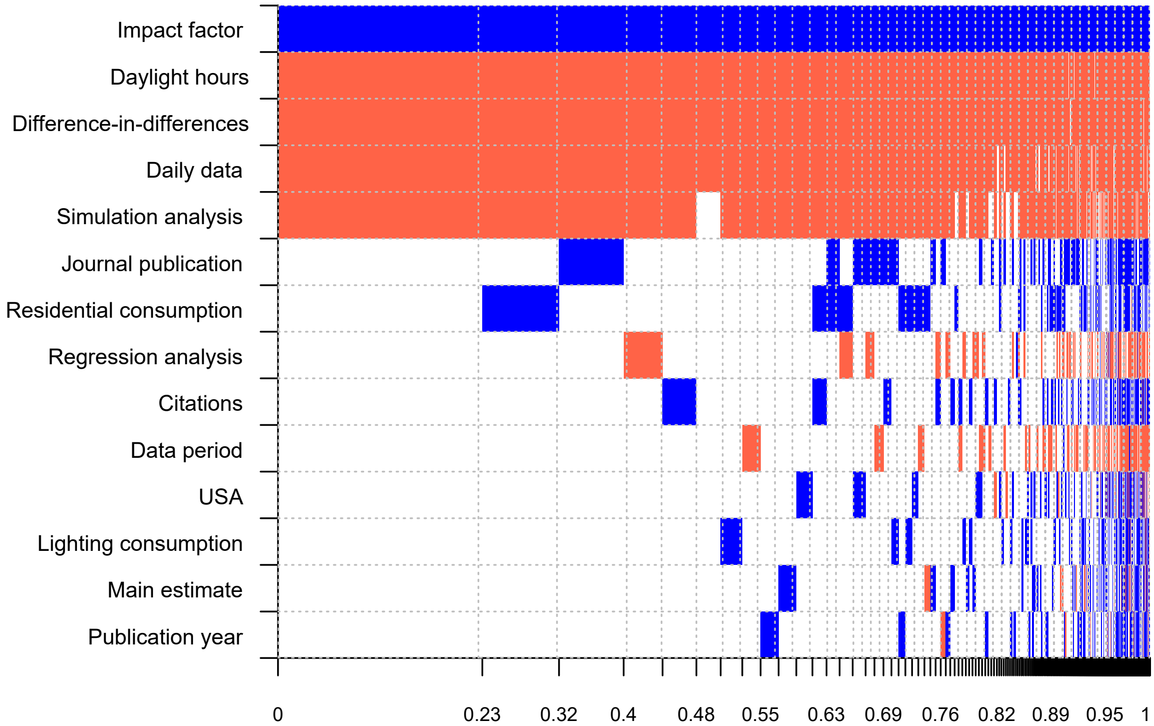
Bayesian model averaging (BMA) estimates a number of models that use subsets of the 14 explanatory variables on the right-hand side. For the estimation we use the `bma` package in R (Feldkircher & Zeugner, 2009) and a Markov Chain Monte-Carlo sampler that only goes through the most important part of the model mass (there are 2^{14} possible models in total). Each estimated coefficient (posterior mean) is the average coefficient of all the models weighted by the posterior model probability, which is akin to adjusted R^2 in frequentist econometrics. Another important concept, posterior inclusion probability, is the sum of all posterior model probabilities of the model in which a particular variable is included and reports how likely the variable is to be included in the true model. The posterior standard deviation is analogous to the standard error and follows the distribution of a coefficient from all estimated models. Further details on BMA can be found, for example, in Eicher *et al.* (2011).

4.2 Results

The BMA results are depicted in Figure 7. Each row in the figure identifies a variable, and rows are sorted in descending order according to the posterior inclusion probability. Each column in the figure identifies a model, and columns are sorted from left to right in descending order according to the posterior model probability. Each cell in the figure identifies a variable included in a model: if the cell is red (lighter in grayscale), the sign of the variable is negative; if the cell is blue (darker in grayscale), the sign of the variable is positive. A cell with no color identifies variables excluded from the model. Five out of the 14 variables are included in the best model, and their estimated signs are robust to the inclusion of the other variables in the model.

We report the numerical results of BMA in Table 5. The posterior inclusion probability is at least *substantial* (which is, according to Kass & Raftery, 1995, above 0.9) for five variables: *Impact factor*, *Daylight hours*, *Difference-in-differences*, *Daily data*, and *Simulation analysis*. For the rest of the variables, the posterior inclusion probability is very weak (below 0.23), which suggests that they are not particularly important in determining the magnitude of the estimate of the DST effect. In addition, we run a frequentist check, reported on the right-hand side of the table, as a simple OLS with standard errors clustered at both the study and country level. The OLS results are consistent with our results from BMA: the highly significant variables correspond to those with high posterior inclusion probability, and the coefficients in both models are fairly similar in value and display the same signs. Additional diagnostics of the BMA exercise are available in Table 7 and Figure 8 in Appendix A. Additional robustness

Figure 7: Model inclusion in Bayesian model averaging



Notes: Response variable: the estimate of the DST effect on energy savings. The columns denote individual models; the variables are sorted by posterior inclusion probability in descending order. Blue color (darker in grayscale) = the variable is included and the estimated sign is positive. Red color (lighter in grayscale) = the variable is included and the estimated sign is negative. No color = the variable is not included in the model. The horizontal axis measures cumulative posterior model probabilities. A detailed description of all variables is available in Table 4; numerical results of the BMA estimation are reported in Table 5.

checks employing different variants of the BMA specification can be found in Appendix B, and they corroborate our baseline results.

Data characteristics According to our findings, the more daylight hours there are on the longest day in a year at a specific location, the higher are the energy savings from DST. The variable *Daylight hours* is a proxy for the location's latitude, which corresponds to the countries and regions in our sample (when analyzing DST, it makes more sense to directly consider the length of the day rather than latitude). The implementation of DST has little effect at very high or very low latitudes: at higher latitudes (close to the poles), the length of the day and night change significantly throughout the seasons, meaning that the standard working hours are far from the sunrise and sunset in summer and winter; while at lower latitudes (close to the equator), the daylight hours are nearly constant throughout the year. The time change generates the greatest effect in the zone between the two extremes, where daylight increases

Table 5: Explaining the differences in the estimates of the DST energy savings

Response variable:	Bayesian model averaging			Frequentist check (OLS)		
Estimate of DST savings	Post. mean	Post. SD	PIP	Coef.	Std. er.	p-value
Data characteristics						
Data period	-0.003	0.013	0.111	-0.020	0.037	0.591
Main estimate	0.004	0.030	0.086	0.064	0.082	0.434
Daily data	-0.444	0.152	0.964	-0.413	0.166	0.013
Daylight hours	-0.118	0.031	0.990	-0.101	0.032	0.002
USA	0.008	0.049	0.102	0.185	0.117	0.113
Design of the analysis						
Regression analysis	-0.021	0.071	0.143	-0.116	0.190	0.541
Simulation	-0.361	0.165	0.912	-0.530	0.150	0.000
Difference-in-differences	-0.412	0.110	0.989	-0.438	0.066	0.000
Residential consumption	0.050	0.114	0.228	0.106	0.170	0.532
Lighting consumption	0.010	0.061	0.089	0.058	0.137	0.674
Publication characteristics						
Publication year	0.000	0.001	0.082	0.002	0.007	0.738
Journal publication	0.040	0.092	0.229	0.219	0.239	0.359
Impact factor	0.958	0.167	1.000	0.746	0.165	0.000
Citations	0.007	0.025	0.133	0.021	0.044	0.641
Constant	1.698	NA	1.000	1.316	0.637	0.039
Studies	44			44		
Countries	21			21		
Observations	162			162		

Notes: The response variable is the estimate of the DST effect on electricity consumption (in %). PIP = posterior inclusion probability. SD = standard deviation. The standard errors in the frequentist check are clustered at both the study and country level (two-way clustering follows Cameron *et al.*, 2011). In this specification, we employ a uniform model prior and use the unit information prior on Zellner's g (Eicher *et al.*, 2011). Further details on the BMA estimation are available in Figure 7. A detailed description of all variables is available in Table 4.

sufficiently during summer months to be relevant to working hours and leisure time in the evenings.

One might suspect that the relationship between *Daylight hours* and DST savings is not linear. We tested for the nonlinearity but found the quadratic term, *Daylight hours squared*, to be insignificant. Therefore, we argue that the proportionality of *Daylight hours* and DST savings is a linear approximation of their underlying relationship. Since few people live close to the poles, our sample comprises regions in the subtropical and temperate zones. The results from Table 5 suggest that the further we go from the equator, the higher the energy savings we observe from DST, which is in line with intuition. Numerically, the −0.12 coefficient from the BMA suggests that for each additional hour of sunlight on the longest day in an affected region, the DST policy yields 0.12% more in energy savings (other things being equal). Weinhardt (2013) examines the heterogeneity in the response of residential energy consumption across different latitudes for the USA. Contrary to our findings, he observes lower savings in the norther part of the US and higher savings in the southern part of the US.

Sampling frequency represents another source of heterogeneity in the estimated coefficients of DST savings. The usage of *Daily data* drives the saving estimates upwards; estimates with higher frequency, mostly hourly data, are associated with smaller savings. The effect of daily data is also economically significant, and the estimated coefficient amounts to -0.44. Time aggregation thus introduces a substantial upward bias into the estimated DST savings. The length of the sample period used in an analysis does not appear to be particularly important, and it does not seem to be relevant whether the data come from the US. The estimates that the authors of studies themselves prefer are close to the overall mean.

Design of the analysis Most estimates of DST savings represent the output of either simulation or regression analysis. Our results imply that the choice of methodology entails, on average, systematically different estimates of DST savings. First, the coefficient estimated for *Simulation analysis* indicates that the simulated estimates of DST savings are larger on average by 0.36 than the rest of the data set, which is significant because the mean estimate of DST is only 0.34. This result supports the previous literature: Kellogg & Wolff (2008), for example, also report that their simulation failed to predict the morning increase in consumption related to DST and overestimated the evening decrease. The use of regression analysis does not seem to deliver results different from the baseline case (extrapolation) unless the *Difference-in-differences* approach is used. We observe an even larger impact on DST savings than in the case of simulations: other things being equal, the difference-in-differences specification is associated with savings that are 0.41 greater than the baseline case.

Finally, Kotchen & Grant (2011) argue that residential consumers adjust their behavioral patterns when the time change occurs and that the commercial and industrial electricity adjustment in demand is not particularly important. Nevertheless, the insignificance of the residential consumption variable instead suggests that the savings estimated for overall consumption do not differ substantially from the savings estimated for residential consumption alone. We observe a similar outcome for lighting consumption: the differences between end-customers and end-uses of electricity are not a source of systematic differences among the estimates in our sample.

Publication characteristics While controlling for specific data and method choices, we also include several publication characteristics. Among the proxies for quality, the number of citations and journal publication are found to be less important than the *Impact factor* of a journal. The difference in implied DST savings between a study from a journal with a zero impact factor and an impact factor of one is 0.96; better journals publish more pessimistic estimates of DST savings. This suggests the presence of additional heterogeneity in methods that we could not capture using the methodological variables codified for this study. The coefficient for the year of publication has a low posterior inclusion probability, which suggests that newer publications do not yield substantially different estimates.

The mean reported estimate of -0.34% does not fit all countries, as we observed above. To provide the reader with an example of how the estimates of DST savings for individual countries would be affected if we used the meta-regression results and filtered out the potential biases stemming from small data sets and improper methodology, we estimate the "best practice" DST savings for each country in our sample using the outcome of the BMA exercise. This aspect of our analysis is the most subjective since it involves defining the preferred value for all explanatory variables (except for the number of daylight hours, where the values are given by the country's location and are the most important factor in explaining the heterogeneity among the estimates), and other researchers might have different opinions on what constitutes best practice. We attempt to construct a synthetic study that assigns greater weight to estimates based on broad data sets and reliable methodology and reported in highly cited studies and prestigious journals.

We prefer the maximum number of years available for estimation in the primary study and higher than daily data granularity since we wish to emphasize studies using the most detailed information available (we plug in "9" for the *Data period* and "0" for *Daily data*). We assign greater weight to the authors' most preferred estimates. In terms of methods, we prefer a study to use the difference-in-differences approach, the most commonly employed tool that allows for better identification than simple regression (and we also find it cleaner than simulation and extrapolation). We prefer general estimates of energy savings to partial estimates based on residential consumption and avoid derivations from estimates based solely on lighting consumption.

Next, we plug in the maximum value of publication year from our sample since we prefer recent studies. Moreover, we emphasize publication quality: we place greater weight on studies published in refereed journals and those with the maximum number of citations. We prefer journals with a high impact factor but also need to control for one outlier, (Kotchen & Grant, 2011); therefore, we choose the 95^{th} percentile for the *Impact factor* variable (if we use the sample maximum, we obtain negative energy savings). Finally, we set the dummy variable *USA* to zero for other countries than the United States and control for country heterogeneity using the variable *Daylight hours*, which varies from 13.2 (northern Chile) to 19.8 (southern Norway).

Table 6: DST effects on energy savings differ across countries

	Mean	95% conf. int.	
Australia	0.189	-0.600	0.978
Austria	-0.059	-0.822	0.704
Chile	0.074	-0.701	0.848
Czech Republic	-0.104	-0.865	0.656
Denmark	-0.258	-1.016	0.501
France	-0.037	-0.802	0.727
Germany	-0.130	-0.889	0.630
India	0.248	-0.550	1.047
Israel	0.146	-0.637	0.929
Italy	0.012	-0.756	0.780
Japan	0.112	-0.666	0.891
Jordan	0.150	-0.634	0.933
Kuwait	0.168	-0.618	0.954
Mexico	0.223	-0.572	1.017
Netherlands	-0.165	-0.924	0.593
New Zealand	0.038	-0.733	0.808
Norway	-0.512	-1.286	0.262
Sweden	-0.510	-1.283	0.264
Turkey	0.063	-0.710	0.836
United Kingdom	-0.201	-0.959	0.557
USA	0.087	-0.543	0.716
Europe	-0.083	-0.845	0.679
All countries	-0.014	-0.760	0.732

Notes: The table presents mean estimates of the DST coefficient in % implied by the Bayesian model averaging and our definition of best practice. The confidence intervals are approximate and constructed using the standard errors estimated by OLS.

Table 6 provides the best-practice DST estimates for all 21 countries examined by the studies in our data set. These estimates are calculated as a linear combination using the coefficients from the BMA meta-regression in Table 5 and values of the variables corresponding to our definition of best practice. The resulting global estimate is -0.01%, quite distant from -0.34%, the simple average effect reported in the literature. The 95% confidence interval of our best-practice estimate is wide, $(-0.76, 0.73)$. Nevertheless, plausible changes in the definition of best practice would not typically lead to substantial changes in the result. For example, if we were to prefer simulation analysis instead of the difference-in-differences approach, the change in the result would only be 0.02. We conclude that energy savings from DST are, on average, negligible and unlikely to exceed 0.76% of total electricity consumption during the days when the daylight saving policy is in place.

5 Conclusion

The main reason for implementing the daylight saving time scheme was to reduce energy consumption. Some students of DST, however, question the real effect and find the present evidence on this topic limited and often contradictory (Aries & Newsham, 2008). To shed greater light on this issue, we conduct a meta-analysis of energy savings from DST; using 162 estimates taken from 44 studies, we estimate the underlying effect. We find that the mean estimate, 0.34% savings, is exaggerated if we take into account the impact of data, method, and publication characteristics. When we place greater weight on the estimates that we consider more reliable (that is, studies published in prestigious journals using high data frequencies and the difference-in-differences method), we obtain a mean effect close to zero. In contrast, we find no exaggeration attributable to publication selection, the usual culprit of bias in applied economics (Doucouliagos & Stanley, 2013).

Our meta-analysis suggests that the cross-country heterogeneity in DST savings can be explained by a country's location. To the best of our knowledge, this is the first empirical analysis of the relationship between the effects of DST and cross-country geography, and the analysis is enabled by our rich meta-data set based on the works of previous researchers. The largest energy savings from DST are enjoyed by countries with the longest daylight summer hours; the closer to the equator we go, the smaller the savings we observe. Our results also indicate that the method choices systematically influence the estimated savings: the use of simulation analysis or the difference-in-differences approach both result in larger estimated savings compared with simple regression or extrapolation. Moreover, higher data frequencies tend to be associated with smaller estimated savings, and studies published in journals with high impact factors also tend to be more pessimistic about the effects of DST.

In any case, the effects of daylight saving time on energy consumption are too small to justify the biannual time-shifting. Other aspects of DST will probably prove more important, but they are often difficult to estimate. The DST policy may affect traffic safety (Ferguson *et al.*, 1995), crime rates (Doleac & Sanders, 2015), business and commercial activities (Kamstra *et al.*, 2000;

Muller *et al.*, 2009), and usable leisure time and can even induce physiological (Lahti *et al.*, 2010; Toro *et al.*, 2015) and psychological effects (Shapiro *et al.*, 1990; Olders, 2003; Kuehnle & Wunder, 2014). The topic calls for a cost-benefit analysis, but to conduct such analysis we would need estimates of the aforementioned effects. On the cost side, one could use the estimates of the number of traffic casualties attributable to DST, which in the US amounts to 366 per year according to Coate & Markowitz (2004). Multiplying this by the mean value of a statistical life, $2.74 million, reported by Doucouliagos *et al.* (2012) and corrected for publication bias, we obtain $1 billion in yearly losses for the US alone (in 2000 dollars). An implication of our study is that it is time for the research literature to reorient itself to examine the more subtle and covert effects of DST.

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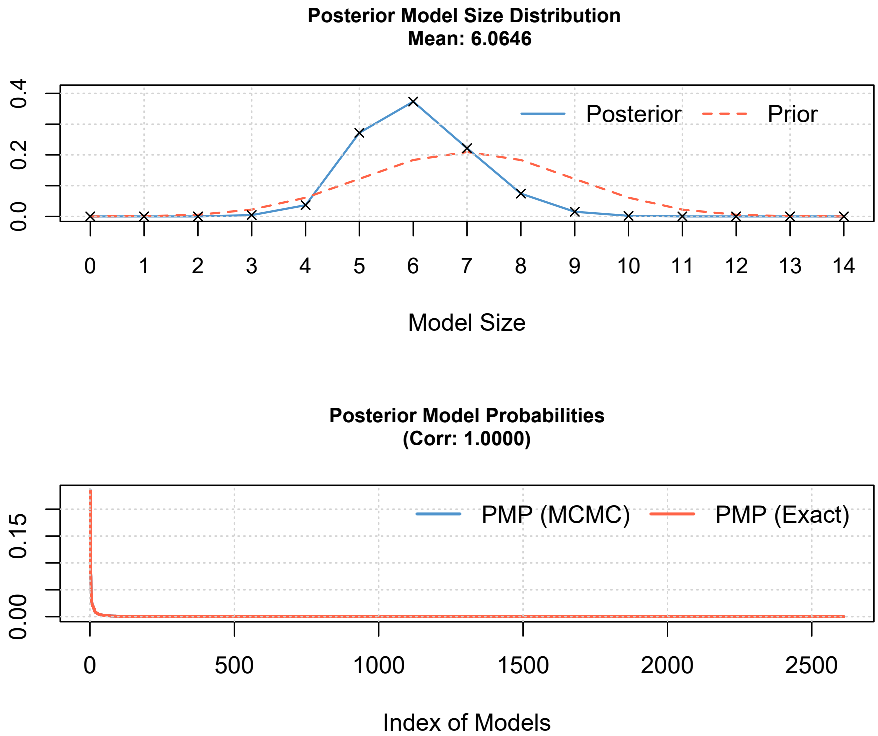
A Diagnostics of BMA

Table 7: Summary of BMA estimation: UIP

Mean no. regressors	*Draws*	*Burn-ins*	*Time*	*No. models visited*
6.0646	$2 \cdot 10^6$	$1 \cdot 10^6$	5.024066 mins	560,236
Modelspace	*Visited*	*Topmodels*	*Corr PMP*	*No. obs.*
16,384	34%	100%	1	162
Model prior	*g-prior*	*Shrinkage-stats*		
Uniform	UIP	Av = 0.9939		

Notes: In this specification, we employ the priors suggested by Eicher *et al.* (2011), who recommend using the uniform model prior (each model has the same prior probability) and the unit information prior (the prior provides the same amount of information as one observation of the data).

Figure 8: Model size and convergence, BMA with priors according to Eicher *et al.* (2011)



B Robustness Checks (For Online Publication)

Table 8: Explaining the differences in DST estimates: robustness checks

Response variable:	Bayesian model averaging: BRIC			Bayesian model averaging: hyper-g		
Estimate of DST savings	Post. mean	Post. SD	PIP	Post. mean	Post. SD	PIP
Data characteristics						
Data period	-0.004	0.016	0.105	-0.008	0.021	0.405
Main estimate	0.003	0.028	0.067	0.016	0.063	0.361
Daily data	-0.415	0.179	0.910	-0.401	0.148	0.973
Daylight hours	-0.114	0.035	0.971	-0.105	0.034	0.987
USA	0.005	0.043	0.076	0.057	0.112	0.440
Design of the analysis						
Regression analysis	-0.016	0.064	0.110	-0.051	0.102	0.432
Simulation	-0.310	0.193	0.800	-0.406	0.151	0.969
Difference-in-differences	-0.390	0.128	0.965	-0.394	0.113	0.993
Residential consumption	0.037	0.100	0.167	0.082	0.134	0.497
Lighting consumption	0.007	0.053	0.066	0.026	0.112	0.356
Publication characteristics						
Publication year	0.000	0.001	0.061	0.001	0.003	0.359
Journal publication	0.030	0.082	0.173	0.104	0.139	0.557
Impact factor	0.977	0.162	1.000	0.797	0.190	1.000
Citations	0.005	0.021	0.097	0.016	0.040	0.413
Constant	1.627	NA	1.000	1.454	NA	1.000
Studies	44			44		
Countries	21			21		
Observations	162			162		

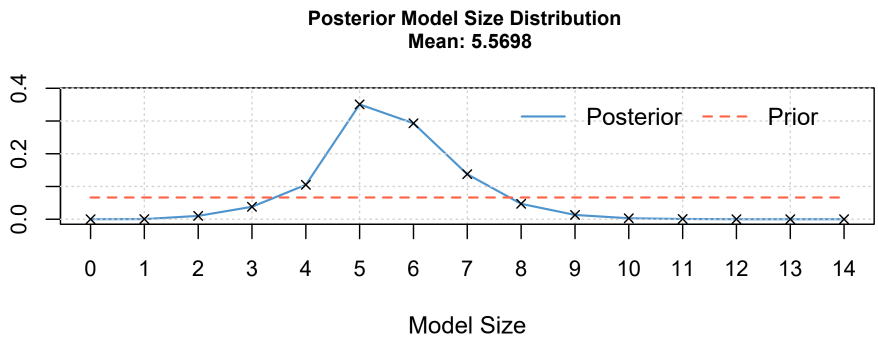
Notes: The response variable is the estimate of the DST effect on electricity consumption (in %). PIP = posterior inclusion probability. In the left hand-side specification, we employ a random model prior, which refers to the beta-binomial prior advocated by Ley & Steel (2009): the prior model probabilities are identical for all possible model sizes. In this specification, we set Zellner's g prior following Fernandez *et al.* (2001). In the right hand-side specification, we employ a random model prior and use the data-dependent hyper-g prior suggested by Feldkircher & Zeugner (2012), which should be less sensitive to noise in the data. Further details on both BMA estimations are available in Figure 11 and Figure 12. A detailed description of all variables is available in Table 4.

Table 9: Summary of BMA estimation: BRIC

Mean no. regressors	*Draws*	*Burn-ins*	*Time*	*No. models visited*
5.5698	$2 \cdot 10^6$	$1 \cdot 10^6$	4.995537 mins	489,541
Modelspace	*Visited*	*Topmodels*	*Corr PMP*	*No. obs.*
16,384	29.88%	100%	1	162
Model prior	*g-prior*	*Shrinkage-stats*		
Random	BRIC	Av = 0.9949		

Notes: The "random" model prior refers to the beta-binomial prior advocated by Ley & Steel (2009); Zellner's g prior is set according to Fernandez *et al.* (2001).

Figure 9: Model size and convergence, BMA with priors according to Fernandez *et al.* (2001)



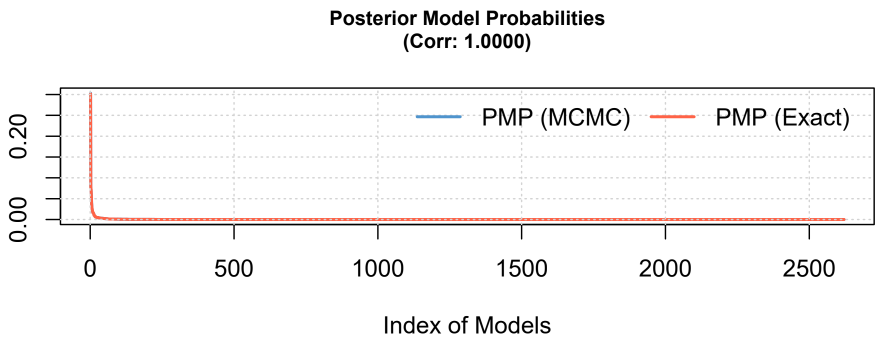


Table 10: Summary of BMA estimation: hyper-g

Mean no. regressors	*Draws*	*Burn-ins*	*Time*	*No. models visited*
8.7791	$2 \cdot 10^6$	$1 \cdot 10^6$	8.367627 mins	1,285,508
Modelspace	*Visited*	*Topmodels*	*Corr PMP*	*No. obs.*
16,384	78.46%	100%	0.9995	162
Model prior	*g-prior*	Shrinkage-stats		
Random	hyper (a=2.0102)	Av = 0.9949, Stdev=0.042		

Notes: This specification of the "random" model uses the hyper-g prior suggested by Feldkircher & Zeugner (2012).

Figure 10: Model size and convergence, BMA with priors according to Feldkircher & Zeugner (2012)

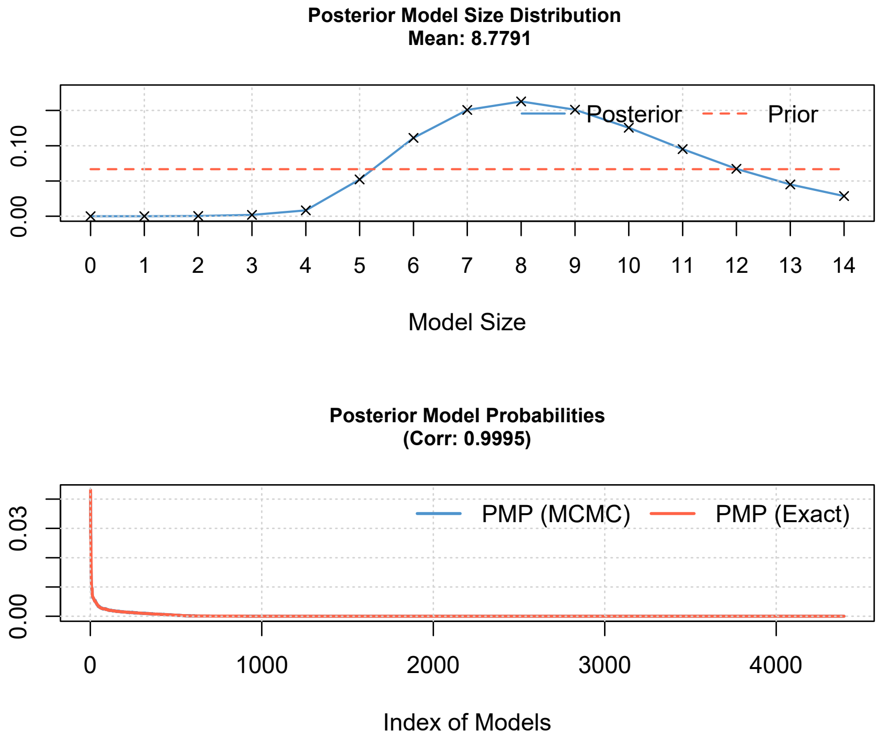
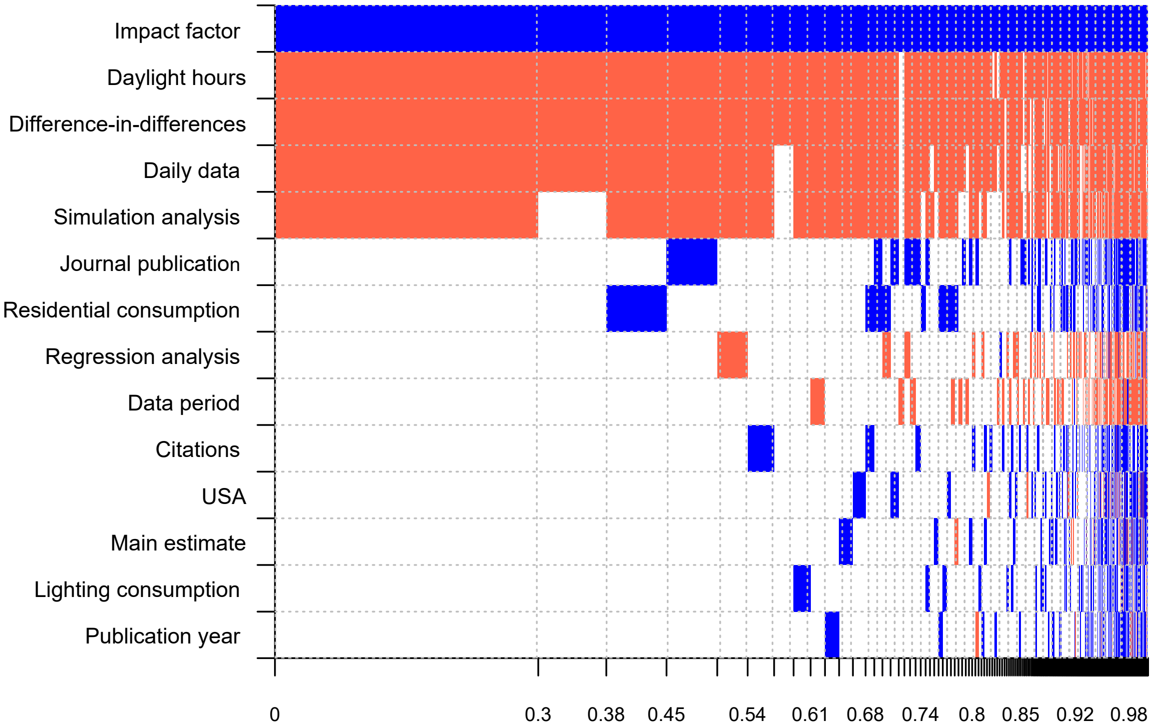
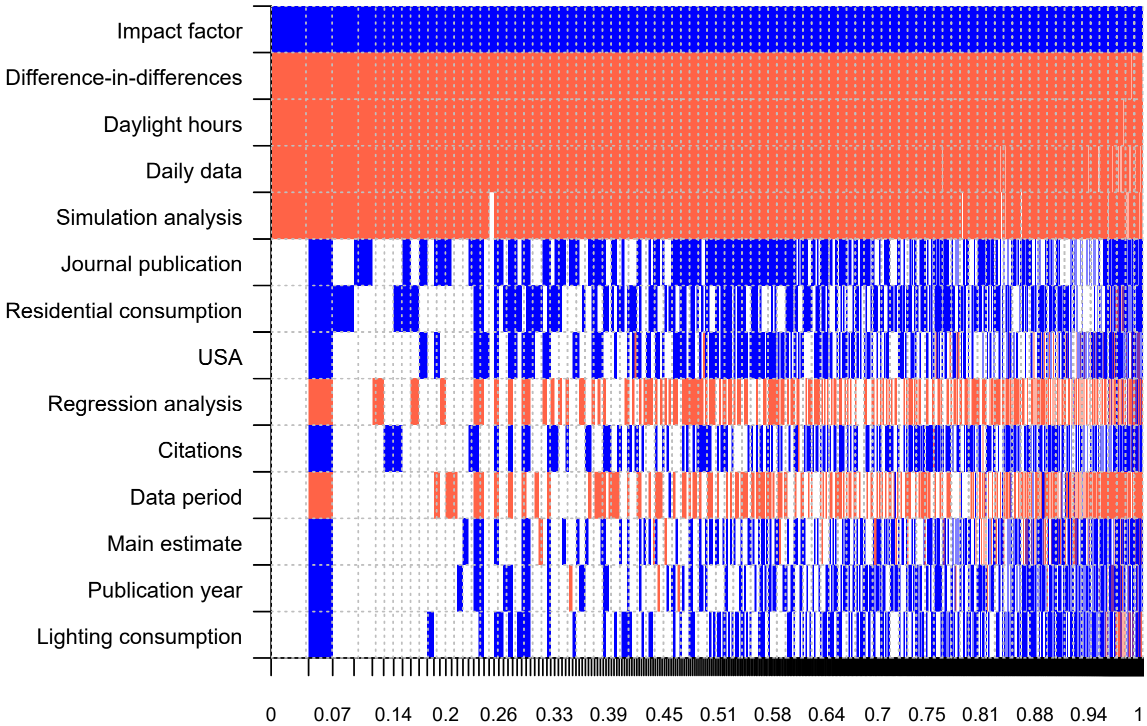


Figure 11: Model Inclusion in BMA with priors according to Fernandez *et al.* (2001)



Notes: Response variable: estimate of the DST effect in energy savings. Columns denote individual models; variables are sorted by posterior inclusion probability in descending order. Blue color (darker in grayscale) = the variable is included and the estimated sign is positive. Red color (lighter in grayscale) = the variable is included and the estimated sign is negative. No color = the variable is not included in the model. The horizontal axis measures cumulative posterior model probabilities. A detailed description of all variables is available in Table 4; numerical results of the BMA estimation are reported in Table 8.

Figure 12: Model Inclusion in BMA with priors according to Feldkircher & Zeugner (2012)



Notes: Response variable: estimate of the DST effect in energy savings. Columns denote individual models; variables are sorted by posterior inclusion probability in descending order. Blue color (darker in grayscale) = the variable is included and the estimated sign is positive. Red color (lighter in grayscale) = the variable is included and the estimated sign is negative. No color = the variable is not included in the model. The horizontal axis measures cumulative posterior model probabilities. A detailed description of all variables is available in Table 4; numerical results of the BMA estimation are reported in Table 8.