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MORAL LICENSING IN THE SWEDISH GREEN ENERGY MARKET*

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Abstract

Moral licensing is a relatively new notion in social psychology and behavioral economics. A handful of experimental studies have shown that people who act in a way that is perceived to be morally meritorious, license themselves to refrain from such moral behavior in the future and may instead undertake immoral actions. The purpose of this study is to examine whether such effects are at play in the Swedish green energy market. We first develop a formal theoretical model that describes moral licensing in relation to energy consumption. We then use difference-in-difference regressions to highlight empirically whether there are differences in energy consumption between customers with standard energy contracts, and customers who switch to a green energy contract. Our results do not show any long-term consumption effects from switching from a standard to a green energy contract.

Key words: moral licensing, green energy, difference in differences

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1 Introduction

The last decade has witnessed a remarkable growth in green energy products in response to the increasing awareness of the climate impact of energy consumption (Chen 2008). It is tempting to view this trend as unequivocally beneficial for the climate. However, a relatively new research field studying a phenomenon denoted *moral licensing* suggests that undertaking such morally meritorious acts may actually induce people to feel licensed to change their behavior in an immoral direction – in the literature, these individuals are said to have accrued a surplus of moral currency (Sachdeva et al. 2009). Indeed, Mazar & Zhong (2009) argue that the purchase of green products may produce the counterintuitive effect of licensing selfish and unethical behaviors from an environmental point of view, by establishing such moral credentials. This argument would hence suggest that it is not certain that the effects of consumers switching to green energy contracts are solely positive. Applied to the climate context, one possible moral licensing effect could be that consumers of green energy feel licensed to consume more energy because of their moral act of switching to green energy.

Intuitively plausible as this argument seems, little is known about whether people in actuality display such behavior, especially in the long-term (see the literature review in Section 2). The purpose of this study is to make a small contribution to the literature on moral licensing by investigating empirically whether households seem to increase their energy consumption when they switch from standard to green energy contracts.

The study is structured as follows. Section 2 reviews the literature on moral licensing. It is argued that there is a lack of field experiments studying potential long-term moral licensing effects. We then describe some general features of the green energy market in Section 3. Section 4 introduces a more formal theoretical model that yields some testable predictions. Our household-level energy consumption data is described in Section 5. Section 6 presents the methodology we employ to analyze the data and to test our theoretical predictions. The results of our investigation are presented in Section 7. Section 8 discusses the results and points to some possible problems with the approach we have employed, and it also provides some suggestions for further research.

2 Previous Research

We will in this Section highlight various contributions to the literature on moral licensing.

2.1 Theory

Past work has shown that peoples self-worth is defined largely by how moral they perceive themselves to be (Dunning 2007). According to Monin & Miller (2001), individuals who fear that their future actions might be (or appear to be) morally dubious, can derive confidence from their past moral behavior. They argue that *moral licensing* occurs because good deeds make people feel secure in their moral self-regard, which licenses them to act immorally. Sachdeva et al. (2009) have a similar interpretation of the notion of moral licensing, suggesting that the choice to behave morally is a balancing act between the desire to do good and the costs of doing so – be the costs time, effort or actual financial costs. They also suggest that affirming a moral identity leads people to feel licensed to act immorally. However, when moral identity is threatened, moral behavior can also be a way to regain some lost self-worth, an action referred to as *moral cleansing*.

It should be noted that the above-mentioned studies put forth the moral licensing notion entirely informally, as opposed to developing it through a mathematically formulated model. Hence, this literature does not provide a precise definition of the notion, when viewed from the perspective of economic theory.

2.2 Experimental Studies

The current research on moral licensing consists mainly of experimental studies from the fields of social psychology and behavioral economics.

One example is a study by Mazar & Zhong (2009), which reports an experiment where students were asked to fill online shopping baskets with up to $25 worth of items. The experiment divided the participants into two groups, where the first group was presented with a store stocked mostly with green products, and the second group was given a store stocked with a majority of conventional products. The participants were later asked to participate in a game where there was an opportunity to lie about their own results to earn more money. The experiment concluded with the participants being shown how much they had won and being told to take the right amount of cash from an envelope. The results showed that the participants who had bought green products were more likely to cheat and steal than those who bought conventional products.

Effron et al. (2009) show that voters who were given an opportunity to endorse Barack Obama for president were more likely to later favor white people for job openings. On average, Obama supporters in a baseline condition said that both races are equally well suited for working as a police officer; by contrast, those who earlier had an opportunity to express their support for Obama, later stated that the police force job was better suited for whites. Presumably, the act of expressing support for a black presidential candidate made them feel that they no longer needed to prove their lack of prejudice in other contexts.

The studies by Mazar & Zhong (2009), and Effron at al. (2009), show how the performance of a moral act may license immoral actions. However, it turns out that even just imagining helping others can also sometimes do the trick. This was shown by Khan & Dhar (2006) who asked participants in an experiment to make hypothetical choices between purchasing a relative luxury (e.g., designer jeans) or a relative necessity (e.g., a vacuum cleaner), after having imagined having behaved in certain ways. The study showed that participants who were asked to imagine having done something altruistic (e.g., volunteering for charity) chose the luxury item more often than those who had not. In other words, participants who imagined doing good deeds thought of themselves as having acted in a moral manner, licensing them to later make more immoral choices with less guilt, despite the fact they had not done undertaken the good act in actuality.

Another study of moral licensing effects of merely imagined acts is the earlier mentioned study by Sachdeva et al. (2009) where participants in an experiment were asked to write a self-relevant story containing words referring to either positive or negative traits. In the experiment, the participants were later asked if they wanted to make a small donation (up to 10$) to a charity of their choice. The results from the experiment showed that participants who wrote a story referring to the positive traits donated one fifth as much as those who wrote a story referring to the negative traits. The authors concluded that people who are primed to think well of themselves behave less altruistically than those whose moral identity is threatened.

The experimental research methodology that has been employed to establish moral licensing effects has been criticized in the literature. For instance, as a reaction to Mazar & Zhong (2009) study, Roser et al. (2010) made a statement claiming that economic psychology and behavioral economics are important disciplines to help us understand the dynamics and limitations of human cognition, potentially resulting in irrational behavior and decision-making. But the authors emphasize that research from this field only demonstrates what happens if people are forced to consume green in a laboratory experiment. They argue that the mere exposure to green products is unlikely to have the long-term effects implied by an overly liberal interpretation of the findings' external validity and potential application. Levitt & List (2007) also advice caution in generalizing on the basis of laboratory results due to systematic reasons, which is something that naturally concerns all the experimental studies conducted to show the effect of moral licensing.

2.3 Field Studies

There is a lack of natural field studies investigating the potential effects of moral licensing. The few natural field experiments that we have referred to are not particularly aimed at showing the moral licensing effect, but do so indirectly.

One of these field experiments is a study by Davis (2008) that discusses the effects of changing from top-loaded to more energy efficient front-loaded washers. The participants of the study received an energy-efficient frontloading clothes washer free of charge in exchange for keeping careful records of their clothes washing before and after the transition to the new washer. According to the author, the quasi-random assignment of clothes washers makes it possible to treat durable goods as exogenous in estimating the production technology and intensity of use decision. The study showed that consumers increased washing by nearly 6 percent after having switched to the more energy-efficient frontloading washers.

Another natural field experiment that has been claimed to show a moral licensing effect is the Pruckner & Sausgruber (2009) field study of the honesty-based payment system at the newspaper booths in Vorarlberg, Austria. In this natural field experiment, the publisher asked for a certain payment for the newspaper, but the actual payment was done anonymously. The authors studied how much money different groups of people actually paid for the magazines. The field experiment showed that, all other things being equal, regular churchgoers were 20% less likely to pay anything at all.

As with the experimental studies, there are several shortcomings in the design of the field studies if one seeks to filter out a pure moral licensing effect. This is not surprising, given the fact that these studies, as mentioned above, were not designed to prove such an effect, but have simply been interpreted by others to show this effect.

Indeed, there are a number of problems with the above-mentioned studies. For instance, the increase in washing that occurred after households had switched to a more energy-efficient front loaded clothes washer that was shown in Davis (2008) does not necessarily stem from a moral licensing effect but may simply reflect the fact that the new washers are more effective and more easily operated and therefore are used more often. The fact that the participants of the study were given a new product thus makes it unsuitable to draw conclusions about a moral licensing effect from this study. Another serious concern is the selection due to the fact that people were not randomly assigned in this study but a population was asked if they wanted to participate. It is not unlikely that those who participated in the study were just tired of their old washers and the increased washing simply expressed excitement over having a new washing machine.

There are also a number of potential problems concerning the field study conducted by Pruckner & Sausgruber (2009). As the authors point out themselves, one reason for the church attendees higher rate of theft might be that they lacked the coin money to make a proper payment. Active religious participation is high in the region, and on a typical Sunday morning, the day on which the study was conducted, it is plausible that many people might have donated some of their coin money to the church. Even if this is not the case, the field study at best, simply shows a short term-moral licensing effect.

2.4 Concluding Remarks on Previous Research

Due to the shortcomings in the current literature on moral licensing, we are of the opinion that there is a need for more natural field experiments that are designed specifically to investigate if there is a long-term moral licensing effect. To study a long-term effect is of importance since it would potentially imply larger consequences than the short-term effects that have been indicated in earlier research. From the shortcomings discussed above regarding natural field experiments, two conclusions can be drawn for the design of natural field experiments to investigate if there is a long-term moral licensing effect.

First, the easiest way to capture a moral licensing effect is to look at the consumption of a good that initially gives people a sense of guilt, but after a certain event leaves them feeling less guilty about their consumption. According to the theory of moral licensing, the consumption should then rise as a response to the lower moral burden. Secondly, the product itself should remain the same throughout the whole period of the experiment. In order to be able to confirm a moral licensing effect, it is crucial that the potential increase in consumption is a result of a lower moral constraint and not an effect of acquiring a new product.

3 The Market for Green Energy Contracts

As a response to the climate debate of the last decades, we have witnessed a remarkable growth of the global market for green products (Chen 2008). The notion of "green products" typically refers to products that are environmentally sustainable, produced using renewable and non-polluting energy sources. In recent statistics from the Swedish Environmental Protection Agency, 89% of the respondents claimed that they are climate-aware and 52% answered that they get feel a sense of guilt when they act in ways that affect the climate negatively (Naturvårdsverket 2009). Energy consumption is the biggest contributor to our climate impact (IPCC 2007) and the existence of green energy alternatives proves that consumers select energy goods not only on price and quality criteria (Monroe 1976) but also on their social and moral values (Caruana 2007, Irwin & Baron 2001). We have therefore chosen to focus our study on green energy, since it fulfills our criteria in terms of its likely effect on peoples' conscious. Another important characteristic for the purpose of our study is that energy is homogenous from a physical point of view: what is delivered through the electricity grid is physically the same regardless of whether it is produced on one fashion or the other. Hence any difference in consumption behavior between households that have one contract form or the other, but which in other respects are identical, is likely to depend on (perceived) differences in the contracts.

In this study we will use data provided by Fortum, which is one of the largest suppliers of green energy in the Nordic region.[1] Fortum is the third largest energy company in Sweden with 14% of the Swedish customers (Svensk Energi 2011) and acts as both a network company and a supplier. Fortum produces energy from hydro-, nuclear- and wind power. The company also has some production through combustion of fossil fuel but the output varies and is mainly used to even out variations in output from hydropower. Currently Fortum offers their private consumers a standard product that consists of 100 percent nuclear power. The company also offers a special green energy alternative that is certified by the Swedish Society for Nature Conservation. The certification is called Good Environmental Choice (GEC) and has been offered since 1996. The content of this certification has changed over time but it currently includes 95% hydropower and 5% wind power (Fortum 2011). The green contract is in practice an addition to the standard contract and usually implies an extra cost per kWh for the green energy.

[1] Interview with Jesper Petersson, Product Manager Dept. of Ecolabelling and Green Consumption, Swedish Society for Nature Conservation, [2011-04-10].

The main reason why Fortum is one of the biggest suppliers of the GEC green energy contracts is that they have chosen to include their GEC-certification in their *Fortum Enkel* contract; a default contract given automatically to all customers who move in to a house or apartment where Fortum owns the network, before the customer chooses his or her own supplier.[2] However, there are also consumers who actively choose to switch to the green energy alternative. These consumers have been offered the green contract through telemarketing sales, often slightly before their current energy contracts expire. In these cases, consumers are been offered to pay the same price for the GEC contract as for the standard contract as an incentive to stay with Fortum, or they have had to pay an extra cost of 1,5 öre per kWh.[3]

Against this background we now turn to our own investigation of a potential moral licensing effects when switching from standard to green energy contracts, starting by formulating a theoretical model capturing such an effect.

4 Model

Consider an individual who consumes x amount of energy at the price of p per kWh. The individual also consumes y units of other goods at a price set to 1. The individual faces a moral cost, $0 < m < 1$, capturing the extent to which the individual feels bad about consuming energy, which in turn affects how much energy the individual chooses to consume. The individual's demand for energy is also affected by the individual's exogenous need of energy, denoted z (hereafter referred to as *need*), which can be seen as reflecting the size of the house, or the energy efficiency of the house.

Let the consumer's preferences be captured by the following utility function:,

$$U(x, y, m, z) \equiv W(x - z) + (-m)x + y$$

where x is the level of energy consumption, and y is the consumption of other goods. We assume that the function W is increasing but strictly concave.

[2] The Swedish energy market is deregulated and each customer can choose his or her own supplier of energy.

[3] Interview with Johanna Åberg, Retention Manager, Fortum, [2011-02-25].

When deciding on the consumption levels x and y, the individual faces a budget constraint:

$$px + y = I,$$

where I is the consumer's income. Rewriting the budget constraint as

$$y = I - px,$$

the individual can be seen as choosing x so as to solve:

$$\max_x U(x, m, z) = W(x - z) + (-m)x + I - px.$$

The associated first-order condition is

$$\frac{\partial U}{\partial x} = W'(x - z) - m - p = 0. \tag{1}$$

Expression (1) defines the demand function for energy $x(m, z, p)$. Note that the income I does not affect demand. This follows from the additive separability of the utility function in x and y, and from the fact that y enters linearly. The first-order condition hence states that the optimal volume is such that the benefit from the consumption of another unit of energy, $W' - m$, equals the marginal cost in terms of forgone consumption of other goods, p.

The second-order condition for a maximum is fulfilled since

$$\frac{\partial U^2}{\partial x^2} = W'' < 0.$$

In order to highlight the properties of the demand function, we totally differentiate the first-order condition (1):

$$W''dx - W''dz - dm - dp = 0,$$

From this expression, it follows that demand decreases in the moral cost:

$$\frac{dx(m,z,p)}{dm} = \frac{1}{W''} < 0. \tag{2}$$

We also observe that an increase in the price of energy will lead to a decreased demand:

$$\frac{dx(m,z,p)}{dp} = \frac{1}{W''} < 0. \tag{3}$$

Finally, energy consumption will increase with a larger need, such as a larger size of the house or a less energy-efficient house:

$$\frac{dx(m,z,p)}{dz} = 1 > 0. \tag{4}$$

Let us now turn to the choice between energy contracts. We assume that a switch to green energy will cause the disutility from energy consumption to disappear, so that $m = 0$ in this case. Consumption will then be $x(0, z, p)$, resulting in the utility level $U(x(0, z, p),0, z)$.

We will assume that there is a fixed cost F for the consumer to switch contract. This could represent the time and efforts required in order undertake the change. We will also allow for the possibility that the price of green energy (p_g) may be higher than the price of standard energy (p_s); $p_g > p_s$.

In light of the above, it will pay for a consumer with house z and moral cost m to switch if

$$U\left(x\left(0, z, p_g\right), 0, z\right) - U(x(m, z, p_s), m, z) > F.$$

The marginal consumer with respect to moral cost, denoted $\widehat{m}$, would then be given by the expression:

$$U\left(x\left(0, z, p_g\right), 0, z\right) - F = U(x(\widehat{m}, z, p_s), \widehat{m}, z).$$

Note that since $\frac{dU(d)}{dm} = -x(d)$, the right-hand side is falling in m. Hence, for a given size of the house, and for given prices, consumers with $m > \widehat{m}$ will choose to switch to the green contract, and consumers with $m < \widehat{m}$ will stay with the standard energy.

The moral licensing notion holds that a switch of contract to green energy should lead to an increase in the purchased volume of energy. For a given price, this would mean that:

$$x(0, z, p) - x(m, z, p) > 0. \tag{5}$$

As can be seen from (2), this is indeed a prediction of the model. Our model thus in this regard seems to capture a basic moral licensing effect. In practice however, when consumers switch contracts, they also often (although not always) face a higher price. The consequence of this is to tend to reduce consumption, as can be seen from expression (3). In our empirical examination below, we will control for this effect.

5 Data

We will in this section present our data.

5.1 Energy Consumption Data

There is a severe shortage of the consumption data that is needed for the purpose of the present study. Before July 2009, monthly reading were not mandatory and Swedish households were charged on the basis of estimated consumption, with the assessments constructed using the households' previous year's annual energy consumption. However, since July 2009 energy companies have been legally compelled to perform monthly readings using remote reading devices and to report these to their customers (SFS 2006/15:90). Even though the law was officially put into practice in July 2009, most households had their remote reading devices installed from the beginning of 2009, and already in April the vast majority of Swedish households had their devices installed. This allows us to use data from April 2009 until November 2010 for those households that have had their remote reading installed.

We have obtained data from Fortum on monthly energy consumption for domestic houses and apartments where each household has a fixed-price contract[4] with a single electricity meter. All consumers in the data have lived in the same facility (i.e., their energy consumption has been monitored using the same meter) during the entire observation period. The energy consumption is measured in kilowatt-hours (kWh) and read remotely by Fortum on the last day of the month. This means that all customers get monthly information about their energy consumption and customers are charged on actual consumption rather than estimated consumption.[5]

The monthly consumption has been aggregated and divided into three time periods: before the switch, during the switch and after the switch. Since we are interested in comparing the before

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[4] One or three year contracts.

[5] Note that this data only measures energy consumption from the grid and not energy consumption from e.g. geothermal heating, district heating, central heating and other off-grid energy sources.

and the after period we have chosen the length of the periods to balance two central considerations. The first is to get as many observations as possible, in terms of number of customers who switch to green energy. Second, since we are interested in the long-term effects, we need to get sufficiently long periods both before and after the switch and that these two correspond with respect to which months they include. As discussed above, our data is also restricted in terms of when the remote readers were installed, and we therefore use the time-periods laid out in Table 5.1 below.

The data is further divided into two subcategories: households that switch from a standard contract to a green energy contract during the observed period (switchers), and households that keep their standard contract the entire observation period (our control group). For the switchers, we exclude all consumers with Fortum Enkel contracts since these customers have not actively chosen to switch to green energy (as described in the Swedish green energy background above). For the control group, a built-in randomization function in Fortum's own Oracle database program has been used to restrict the sample. However, the sample was large enough to fulfill the needs of our study.

One potential problem with our standard dataset (A) described above is if the control group and the group that switches are systematically different (further explained in empirical methodology). One indication of this is the comparison made in the descriptive statistics section below, where we can see differences between the switch and control group in the distribution between counties and Mosaic groups. Even though there seems to be a parallel trend between the control and switch group in the logged dataset A, we have chosen to use a second dataset (B) as a complement. While dataset A compare switchers (switch group) to non-switchers (control group), dataset B compares early and late switchers of green energy. As described in Table 5.1, the observation period in dataset B has been altered in order to construct a control group consisting of switchers that switch outside of the chosen observation period, denoted late switchers. Customers that switch to green energy contracts during the chosen observation period are called early switchers.

The purpose of this distinction is to use the late switchers as control group. The benefit from this is that this latter group is more similar (in terms of consumption trends) to the switch group than the randomly selected control group used in the standard regression. In order for this to be feasible data-wise we have to shorten the after period in this dataset since we could not include the two months during which the late switch group change contracts, which also affects the before period that has to be the same length. In this dataset B, the early switch group switches during the period October 2009 to March 2010. The late switchers (control group) make the switch either in October 2010 or November 2010, which is outside of the observed period (See Table 5.1 below).

Table 5.1 – Time periods

Dataset	Before period	Switch period	After period
Standard dataset (A)	Apr 2009 - Nov 2009	Dec 2009 - Mar 2010	Apr 2010 - Nov 2010
Early- vs. late switchers dataset (B)	Apr 2009 - Sep 2009	Oct 2009 - Mar 2010	Apr 2010 - Sep 2010

Table 5.1 shows the before, switch, and after periods for the two datasets.

5.2 Price Data

The data includes a variable that indicates whether the customer pays an extra cost of 1,5 öre per kWh for their green energy; as explained above some do, but not everyone. Individual customer specific data on actual price is confidential and therefore unavailable. Consequently, we do not have data on the absolute price of energy for each household. However, our theoretical model is not designed to determine the absolute magnitude of the change in consumption from a switch to green energy but rather provide an indication of the direction of the change.

Being unable to control for the absolute price effect, we only include fixed price contracts that have been renewed (control group) or changed to a fixed price green energy contract (switch group) during the switch period. This is done in order to minimize the impact on consumption from changes in prices.

5.3 Control Variable Data

To control for other things that might affect changes in energy consumption we have included data on geography (county) and consumption patterns. We use information on the customer's county of residence to control for temperature differences.

In an attempt to control for customer specific properties that affect energy consumption, we have included Mosaic Lifestyle data (Mosaic data) from Experian.[6] The Mosaic data pertains to groups of consumers rather than individuals. It is as close to consumer specific data we can get,

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[6] For more detail information about the Mosaic Lifestyle groups, please visit Experian's homepage (www.experian.se).

since Fortum is by law not allowed to share customer specific data due to integrity reasons. The Mosaic data set is designed to identify groupings of consumer characteristics based on a number of socio-economic variables. Mosaic divides Sweden into a total of 74 000 quadratic areas (Mosaic areas), where the smallest and most common size is an areas of 125 x 125 meters. An average of 121 individuals and 62 families lives in each such Mosaic area. Each Mosaic area is then described on the basis of a number of socio-economic parameters and categorized into 12 different lifestyle groups. These groups are further divided into more detailed subcategories but these are left out in Table 5.2. The 12 main Mosaic groups are:

Table 5.2 – Mosaic Lifestyle groups

Group	Description
A	Well-educated city dwellers
B	Singles in big city
C	Young singles in apartment
D	Seniors in apartment
E	Apartment in small community
F	Cultural diversity
G	Affluent homeowners
H	Single-family home in suburban area
I	Single-family home and commuting
J	Single-family home in small community
K	Single-family home and industry
L	Rural area

Table 5.2 describes the twelve main Mosaic groups. There are up to four subcategories under each main group. These subcategories are only used in our extension regression and described on Experian's homepage (see note above).

5.4 Descriptive Statistics

Let us now turn to broadly describing our data. The frequency distribution Tables 5.3 and 5.4 reveal that the switch and control groups are much more similarly distributed in the early- vs. late switchers dataset B than in the standard dataset A. People who live in apartments (Mosaic group A to G) and people from Stockholm are overrepresented in the group that switches in our standard dataset A. A large proportion of these individuals live in apartments, and this is probably the reason why the average monthly consumption is much lower in the switch group than in the control group, as can be seen in Graph 5.1a. In this Graph, we also observe that there are slight differences in trends between the switchers and the control group in the standard dataset A. However, we see in Graph 5.1b that the trends are parallel when we use logged consumption data (here and throughout "log" refers to base-10 logarithms, unless otherwise stated).

Table 5.3 – Distribution between Mosaic groups

	Standard dataset (A)			Early vs. late switchers dataset (B)		
Mosaic group	Switch group	Control group	Difference	Switch group (early)	Control group (late)	Difference
A	19,1%	9,2%	9,9%	18,4%	13,9%	4,5%
B	16,9%	6,0%	11,0%	13,9%	16,3%	-2,3%
C	2,1%	2,2%	-0,1%	1,7%	2,9%	-1,2%
D	10,2%	6,0%	4,2%	9,2%	8,5%	0,7%
E	3,4%	5,6%	-2,2%	4,8%	7,5%	-2,7%
F	8,5%	6,2%	2,3%	6,1%	8,0%	-1,9%
G	9,3%	10,4%	-1,1%	8,8%	8,3%	0,6%
H	3,0%	6,4%	-3,4%	3,7%	4,8%	-1,1%
I	6,4%	10,4%	-4,0%	7,5%	6,7%	0,8%
J	6,8%	11,6%	-4,8%	8,2%	6,9%	1,2%
K	5,1%	9,8%	-4,7%	7,1%	5,3%	1,8%
L	9,3%	16,4%	-7,0%	10,5%	10,9%	-0,4%
Total	100%	100%	0%	100%	100%	0%
Observations	236	501		294	375	

Table 5.3 shows the frequency distribution between Mosaic groups. As we can see in the difference column, the switch- and control group are much more similarly distributed in the early vs. late switchers dataset.

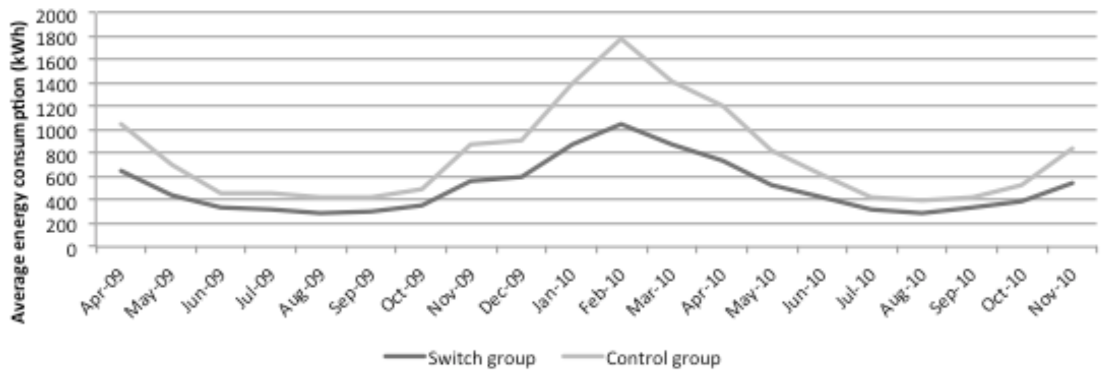
Graphs 5.2a and 5.2b show that the switch and the control groups for the early- vs. late switcher dataset (B) are much more similar both in terms of average monthly consumption, and in terms of parallel trends. The parallel trends of the switch and the control group can be seen in the graphs showing both the logged and the actual average consumption. This is probably due to their similar frequency distributions presented in Tables 5.3 and 5.4.

Table 5.4 – Distribution across counties

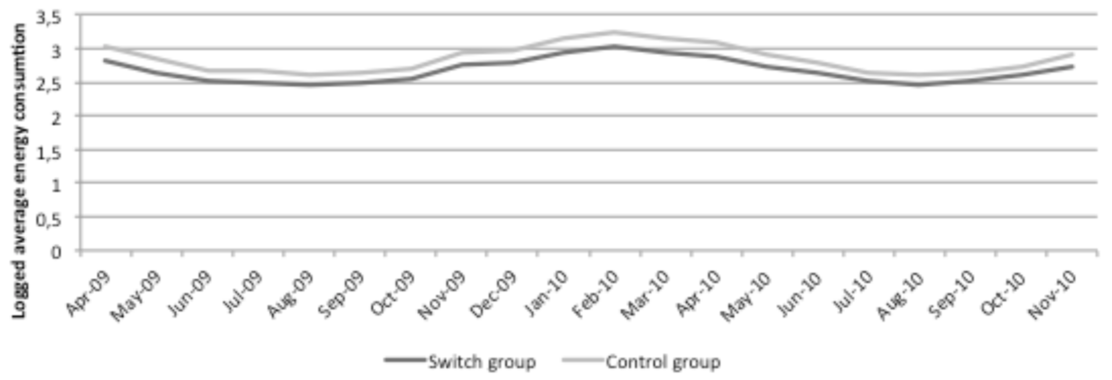
	Standard dataset (A)			Early vs. late switchers dataset (B)		
County	Switch group	Control group	Difference	Switch group (early)	Control group (late)	Difference
Blekinge	0,0%	0,4%	-0,4%	0,0%	0,3%	-0,3%
Dalarna	0,8%	1,8%	-0,9%	0,3%	1,1%	-0,7%
Gävleborg	3,4%	8,6%	-5,2%	5,4%	9,3%	-3,9%
Halland	3,4%	3,8%	-0,4%	2,7%	2,9%	-0,2%
Jämtland	0,0%	0,2%	-0,2%	0,0%	0,3%	-0,3%
Jönköping	1,3%	4,0%	-2,7%	1,7%	1,6%	0,1%
Kalmar	1,7%	2,0%	-0,3%	1,7%	0,3%	1,4%
Kronoberg	0,0%	0,4%	-0,4%	0,0%	0,0%	0,0%
Norrbotten	0,4%	0,0%	0,4%	0,3%	0,0%	0,3%
Skåne	1,7%	2,4%	-0,7%	1,7%	1,3%	0,4%
Stockholm	62,3%	32,9%	29,4%	55,1%	50,9%	4,2%
Södermanland	0,4%	0,4%	0,0%	0,3%	0,3%	0,1%
Uppsala	0,0%	0,8%	-0,8%	0,0%	0,8%	-0,8%
Värmland	11,0%	14,6%	-3,6%	11,9%	9,6%	2,3%
Västernorrland	0,0%	0,6%	-0,6%	0,0%	0,5%	-0,5%
Västmanland	0,0%	0,4%	-0,4%	0,3%	0,0%	0,3%
Västra götaland	10,6%	19,6%	-9,0%	14,3%	16,5%	-2,2%
Örebro	3,0%	6,6%	-3,6%	4,1%	3,7%	0,3%
Östergötland	0,0%	0,6%	-0,6%	0,0%	0,5%	-0,5%
Total	100%	100%	0%	100%	100%	0%
Observations	236	501		294	375	

Table 5.4 shows the frequency distribution between counties for the two datasets. As we can see in the difference column, the switch- and control group are much more similarly distributed in the early- vs. late switchers dataset.

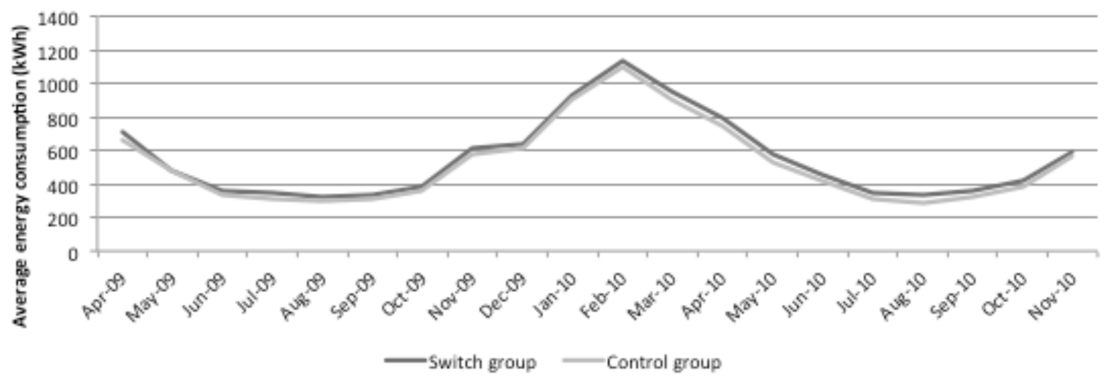
Graph 5.1a - Average energy consumption (Standard dataset)



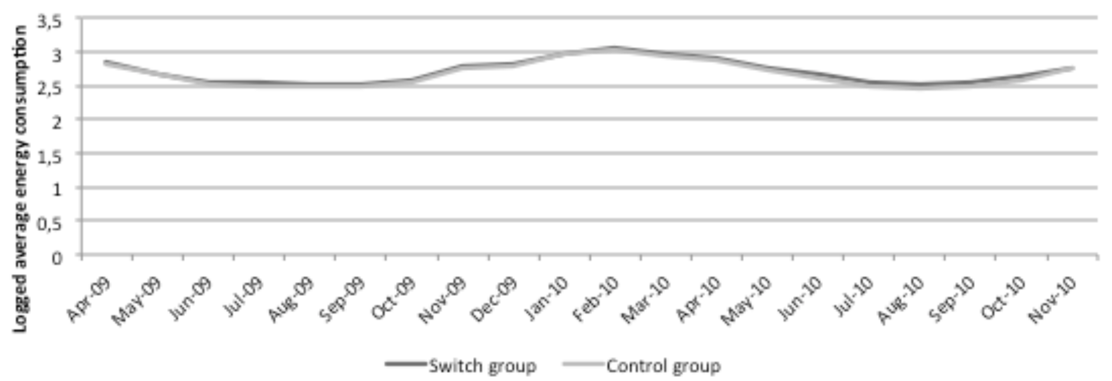
Graph 5.1b - Logged average energy consumption (Standard dataset)



Graph 5.2a - Average energy consumption (Early vs. late switchers dataset)



Graph 5.2b - Logged average energy consumption (Early vs. late switchers dataset)



6 Empirical Methodology

As discussed above, the literature suggests the existence of moral licensing effects. Applying this notion to the context of energy consumption, our model proposes that this notion could show up as an increase in energy consumption after the switch to a green energy contract. On the other hand, if moral licensing does not exist, we would expect the switch to a green energy contract to not affect energy consumption, except possibly through changes in prices. This leads us to the following hypotheses:

H0: The switch to green energy does not affect energy consumption

H1: The switch to green energy does affect energy consumption.

The null hypothesis will be rejected if the switch-parameter is statistically significant. The way in which we have formulated the alternative hypothesis implies a double-sided test, since we have not specified the direction of the change. There are pros and cons with this approach, compared to specifying the alternative hypothesis such that a single-sided test emerges. We have chosen the double–sided formulation in order to allow for changes in energy consumption in both directions. But note that only an increase in energy consumption (positive coefficient) would be evidence of a moral licensing effect.

6.1 Difference-in-differences Regressions

We use difference-in-differences (DID) methodology to test our main hypothesis, that switching to green energy will affect energy consumption. For the first level of differences we subtract energy consumption after the switch from energy consumption before the switch, giving us two sets of differences Δe_S for the switch group and Δe_C for the control group. By using DID rather than the regular difference estimator we are able to control for pre-switch differences in the dependent variable. By itself, Δe_S is not a good estimator of the effects from the switch since there might be other changes that affect at the same time as the switch is made. To control for this we use a second level of differences where we subtract Δe_C from Δe_S. Note that this is a measure of the average effect for the whole population.

6.2 Regression Model

One of the most important requirements for using DID here is that there are parallel trends in energy consumption (Stock & Watson 2007), meaning that the switch- and control group react identically to all internal and external shocks. The Graphs in the previous section suggest that this is not an unreasonable assumption. However, it is not appropriate to infer that trends are parallel simply by inspecting a graph. Even differences that are too small to be detected by the eye could indicate that the parallel trend assumption does not hold. This could be the case here if we had a selection problem i.e. if our switch group consists of a certain type of customers who systematically differ from the control group in how they consume energy. For instance, a problem could arise if these two groups have systematically different heating systems or if they systematically react differently to temperature changes. Say, for instance, that green consumers are more likely to have district heating or geothermal heating. Such differences would not impact the energy consumption during summer, since neither the control, nor the switch group, use heating during the summer. But when the winter arrives there would appear to be significant increases in energy consumption for the control group. Potential differences in heating system also means that the before and after periods have to include the same months to be comparable.

If we have a selection bias of the type described above we can still differentiate the effect of the actual switch by using green customers who do not switch until after the observation period (late switchers), as control group. A control group with future green consumers is more similar to the switch group and still control for the time effect since they have not switched yet. Comparing Graphs 5.1a and 5.2a, and Graphs 5.1b and 5.2b in the previous section we see that the early and late switcher groups are in fact more similar in terms of energy consumption. Many factors suggest that the timing for the switch is random, but we cannot rule out the possibility that switches occur due to external factors omitted from the regression. If this were the case, it would lead to biased estimates of the switch effect in the early vs. late switchers regression, which is the reason why we keep the standard regression.

In order to reflect the considerations above, our basic regression model looks as follows:

$$\log(Y_{i,after}) - \log(Y_{i,before}) = \propto + \beta_1 Switch_i + \beta_2 Price_i + \beta_3 Mosaic_i + \beta_4 County_i + \varepsilon. \quad (7)$$

The variable Y_{it} represents energy consumption for individual i in period t. We only use two time periods in our estimations to eliminate biased standard error, as recommended by Bertrand et al. (2004). The dependent variable $\log(Y_{i,after}) - \log(Y_{i,before})$ is constructed as the difference in logarithms of aggregated energy consumption before and after the switch to green energy. The reason we differentiate the dependent variable immediately, rather than include a post switch and interaction dummy, is that by differentiating the dependent variable we can control for differences in trends, if they occur. This is preferred to controlling for volume differences, as would have been the case otherwise.

In our theoretical model we capture three things that affect energy consumption; moral cost, price, and the need. Using DID, the parameter β_1 captures the change is energy consumption due to the switch from a standard to a green energy contract. In a perfect DID setting we do not need any control variables but in our case we included some to adjust for conditional randomization (Stock & Watson 2007).

We use dummy variables to control for price (since there are only two prices in the data) and the level of need for energy. A dummy variable is simply a binary variable that takes the value of 1 if the dummy is true and 0 if the dummy is false. The price dummy $Price_i$ indicates whether consumer $\mathbf{i}$ faces a higher price as a consequence of switching from a standard to a green energy contract. The need for energy depends on both external factors (e.g. temperature etc.) and individual factors (e.g. house size, family size etc.). We use two vectors consisting of dummy variables to control for these effects. One is $County_i$, which controls for differences in demand that may arise due to differences in e.g. outdoor temperature. The other is $Mosaic_i$, capturing certain differences in consumer characteristics. Since we do not have individual data on morality we are unable to control for this effect in our standard regression. However, in the results section we will optimize our regression by restricting our dataset using education level as a proxy for morals.

6.3 Modifications of the Data Set

The data has been cleared from extreme outliers, errors in measuring and missing values. Outliers are considered to be households with more than 40.000 kWh of energy consumption during a 12-month-period. Missing values are blank readings due to broken electricity meters. Errors in measuring are negative values of energy consumption.

7 Results

We now turn to the results of our regressions. We divide these into a baseline regression on our two datasets, and into an extension where we optimize the sample of the two datasets in order to find an effect.

Tables 7.1 and 7.2 provide the results from our baseline regressions, where we consider the effect on energy consumption of switching to green energy contracts. Note that in our regressions the county of Blekinge and Mosaic group A are dropped to avoid multicollinearity, and they thus constitute the baselines for countries and Mosaic groups, respectively.

As can be observed in Table 7.1, the switch coefficient is not significant when employing our standard dataset (A), at the 5% confidence level. Furthermore, there is no increase in the significance level for our switch coefficient when we add the control variables[7], and the significance level is also in this case far too low to reject the null hypothesis. The results from using late switchers as a control group, seen in Table 7.2, display a very similar picture, even though the switch coefficient becomes slightly more significant when adding our control variables to the regression. We thus cannot reject the null hypothesis that the switch has no effect on energy consumption.

Table 7.1 - Standard regression

Variable name	Coefficient	Robust Std. Err.	P-value	Coefficient	Robust Std. Err.	P-value
Switch	-0,0136	0,0147	0,355	0,0001	0,0149	0,994
Price	0,0105	0,0183	0,568	0,0106	0,0187	0,570
Constant	0,0265	0,0051	0	0,0272	0,0285	0,340
Control variables		No			Yes	
Observations		737			737	
R-squared		0,0013			0,0954	

Table 7.1 shows the results from our baseline regression on the standard dataset (A).

7 All of the results with the control variables included are found in the Appendix.

Table 7.2 – Early vs. late switchers regression

Variable name	Coefficient	Robust Std. Err.	P-value	Coefficient	Robust Std. Err.	P-value
Switch	0,0122	0,0136	0,371	0,0150	0,0141	0,286
Price	-0,0067	0,0153	0,662	0,0008	0,0158	0,959
Constant	0,0210	0,0168	0,211	-0,0249	0,0417	0,552
Control variables		No			Yes	
Observations		669			669	
R-squared		0,0022			0,0897	

Table 7.2 shows the results from our baseline regression on the early vs. late switchers dataset (B).

7.1 Extension

It does not seem implausible that the moral licensing effect is too small, or too concentrated to a specific group, to show up significantly in a regression using the whole population. In order to enhance the possibility of identifying a long-term moral licensing effect, we therefore use predictions from our theoretical model to restrict our sample to include only those people who are most likely to show this effect. To explain how, we need to take a closer look at the explanatory variables in our theoretical model.

Our theoretical model predicts that people with higher morals (higher m) will show a bigger moral licensing effect when switching from standard to green energy, all other things being equal.[8] Previous research has shown that an important factor for the awareness of climate change related issues is education (Pugliese & Ray 2009). We therefore use education as a proxy for morals in an extension, concentrating on the Mosaic groups where at least 40 percent of the households have tertiary education.

[8] To see how, note that the moral licensing effect of increased consumption when switching to green energy, is captured in equation (5) in the model. As can be seen in equation (2),

$$\frac{dx(m,z,p_d)}{dm} = \frac{1}{W''} < 0.$$

This implies that a larger m will lead to a larger moral licensing effect.

A special feature of the theoretical model is that it predicts that the magnitude of the need for energy (captured by z), does not affect the extent to which an individual household will increase its consumption when switching to a green energy contract.[9] However, there are several reasons to believe that in practice, a larger z would indeed lead to a greater moral licensing effect after a switch. For instance, our basic theoretical model does not take into account the differences imposed by living in an apartment and a house regarding the possibility to regulate ones energy consumption. Unlike people living in houses, consumers living in apartments cannot affect their energy consumption very much, since their heating and warm water consumption will not show up on their individual energy consumption. This leads to less sensitivity in energy consumption to the form of contract. We therefore only look at individuals that live in houses in the extended, optimized regression.

Table 7.3 – Standard dataset extension regression

Variable name	Coefficient	Robust Std. Err.	P-value	Coefficient	Robust Std. Err.	P-value
Switch	0,0365	0,0266	0,174	0,0591	0,0327	0,074
Price	-0,0811	0,0743	0,279	-0,0936	0,0791	0,241
Constant	0,0155	0,0116	0,186	-0,0284	0,0327	0,388
Control variables		No			Yes	
Observations		82			82	
R-squared		0,0307			0,1219	

Table 7.3 shows the results from our extension regression on the restricted standard dataset. The dataset only includes people who live in houses, and belong to a Mosaic group with an average tertiary education level above 40%.

Table 7.3 summarizes the regression results when focusing on households living in villas, and with a share of tertiary education above 40 percent. As can be observed, the regression using our standard dataset (A) gives us a switch coefficient of 0.0591, which is significant at the 10% level, but not at the 5% level. The evidence for rejecting the null hypothesis is therefore rather weak. However, let us anyway illustrate how estimates from the regressions could be used magnitude of the treatment effect in cases where the estimated parameters are more significant.

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[9] The moral licensing effect of increased consumption when switching to green energy is captured in equation (5) in the model. As can be seen in equation (4),

$$\frac{dx(m,z,p)}{dz} = 1.$$

This implies that the size of z does not affect the potential increase in consumption in absolute terms.

Let us write equation (7) as:

$$\log(Y_{i,after}) - \log(Y_{i,before}) \;=\; k + \;\beta_1 Switch_i$$

where $k = \propto + \beta_2 Price_i + \;\beta_3 Mosaic_i + \;\beta_4 County_i + \;\varepsilon.$

Rewriting the expression we get

$$\log \frac{(Y_{i,after})}{(Y_{i,before})} = \;k + \;\beta_1 Switch_i,$$

or

$$\exp\left(\log \frac{(Y_{i,after})}{(Y_{i,before})}\right) = \exp(k + \;\beta_1 Switch_i\;).$$

Rewriting the expression we get

$$\frac{(Y_{i,after})}{(Y_{i,before})} = 10^{\,k+\;\beta_1 Switch_i}.$$

When the switch dummy is 1, this gives us

$$\frac{(Y_{i,after})}{(Y_{i,before})} = 10^{\,\beta_1}$$

$$= 10^{\,0{,}591} = 1{,}146.$$

This indicates that the moral licensing effect would lead to a 15% higher change in energy consumption between the two periods, when switching to a green energy contract compared to the control group that do not switch (provided we believed in the estimate of β_1).

As mentioned above, we also use an alternative approach where we consider the difference between early and late switchers (dataset B). However, as can be seen from Table 7.4, results are still not significant at the 5 % level results for the switch coefficient, as can be observed in Table 7.4.

Table 7.4 – Early- vs. late switchers dataset extension regression

Variable name	Coefficient	Robust Std. Err.	P-value	Coefficient	Robust Std. Err.	P-value
Switch	-0,0339	0,0583	0,562	-0,0269	0,0734	0,715
Price	-0,0542	0,0523	0,304	-0,0448	0,0669	0,506
Constant	0,0770	0,0479	0,144	0,0669	0,0669	0,322
Control variables		No			Yes	
Observations		63			63	
R-squared		0,0229			0,0819	

Table 7.4 shows the results from our extension regression on the restricted early- vs. late switcher dataset. The dataset only includes people who live in houses, and belong to a Mosaic group with an average tertiary education level above 40%.

8 Concluding Discussion

This thesis aims to investigate whether a switch to green-energy contracts causes a long-term moral licensing effect. To this end, the paper first constructs a formal theoretical model describing moral licensing, in order to give this normally rather vague notion a more precise meaning. The paper then employs two datasets based on data from the energy supplier Fortum, Sweden, to examine empirically whether such an effect can be detected.

As shown above, the two baseline regressions did not give us any statistically significant support for the existence of a moral licensing effect. The highest level of significance was obtained in a regression using late switchers to green energy contracts as a control group. The switch dummy was significant at a 10% significance level, indicating that the change in energy consumption between the two periods being examined is approximately 15% higher for the treatment group than for the non-treated ones. It should be emphasized that this estimation uses a very small sample (only 82 observations), and that we therefore have to be cautious when interpreting the result. After all, with a 90 percent level of confidence, we will get significant results in ten percent of the cases even if there is no effect (a type-I error – see Stock & Watson 2007). There is therefore considerable risk that this particular result could be significant just by chance.

A possible reason why rather insignificant results were found is of course that there simply does not exist any long-term moral licensing effect from switching from standard to green energy contracts in the market under study. However, there are several shortcomings in the method and design of our study that could hinder us from finding a moral licensing effect even if it did exist. In what remains of this Section we will discuss some of these weaknesses, and point to issues that seem worthy of future study.

First, in keeping with the standard approach in modern econometrics, we have employed a two-sided test in the above. A two-sided test can also be defended on the basis that the theory does not clearly predict the direction of change in energy consumption, since one of the predictions from our theoretical model predicted was that the price variable could reduce consumption. The consequence of the two-sided approach is that it becomes harder to reject the null hypothesis for a given level of significance. It can be noted that the estimated β_1 parameter would have been significant at a 5 % significance level, had we used a one sided test.

Second, there are shortcomings with regard to our data. First, even if a moral licensing effect exists, the short observation period could prevent us from finding a true long-term moral licensing effect. A longer observation period would also give us the opportunity to follow the energy consumption for a longer time, making it easier to establish if the trends are truly parallel between the consumers who use green energy and the consumer that decide to stay with the standard option. Secondly, due to lack in the data of consumers who actively choose to change to a green energy alternative, we have had to aggregate several months in the switch periods. More observations would have been useful since it would have allowed us to establish if even a short-term moral licensing effect can be found in the Swedish green energy market. This would have been useful to compare to the potential long-term effect. Thirdly, it would have been desirable to use more consumer specific data, i.e., better control variables. The level of consumption, and the possibilities to vary consumption, is highly dependent on whether a consumer lives in a house or an apartment, the size of that house or apartment, the size of the family, the household's income, and the total price per kWh that the consumer faces. These are all factors that have an effect on energy consumption but that we are merely able to control for to some extent using our Mosaic control variables.

But even if our regressions had shown a positive and significant switch dummy, there would still be reasons to be careful when interpreting the results. As always in natural field experiments, there is a question of causality. In our theoretical model, we have assumed that each individual decides whether or not to switch to a green energy contract on the basis of his or her current consumption of energy. However, it is possible that an individual chooses to switch to green energy because they know that they will increase their consumption in the future. One example would be if an individual plans to build a sauna that would increase the individual's future energy consumption. In our regression, the increase after the switch to green energy would then seem as an effect of moral licensing, while in reality, it could be an effect of a planned increase in consumption and the switch to green energy would simply be an act of moral cleansing.

Another problem arises with our choice of method. DID perform at its best when an exogenous shock affects a random sample of the population and that this sample can be compared to another sample not affected by the shock. However, using DID to test if there is a moral licensing effect that leads to higher energy consumption when switching to a green energy contract, brings on a new series of problems. In reality, the people who switch to green energy are not necessarily randomly assigned because they have actively chosen to switch to green energy. As discussed earlier the reason why certain people switch to green energy is unclear, and there is a risk that there is an omitted variable that also might affect the consumption levels when the switch has taken place.

There are also other possible reasons for the lack of more significant results. For instance, it could in principle be the case that there is no long-term moral licensing phenomenon in this particular market, although the effect exists more generally. We are not convinced that this is case, however. As mentioned in Section 3 in paper, people are increasingly aware of how energy consumption affects the environment, and a green alternative should lead to a lower moral burden for the energy consumers. However, there is the possibility that the potential long-term moral licensing effect is directed somewhere else, as discussed below. It is also possible that the moral licensing effect is simply a short-term phenomenon.

It should also be noted that the moral licensing effect might take different forms from the one expounded here. As seen in previous experimental studies in Section 2, the initial moral act does not have to be related to the immoral act that follows as a result of moral licensing. As opposed to what is the case in experimental studies, we are not able to force the consumers to choose between two specific choices right after they have performed the moral act. It is therefore plausible that a consumer that switches to green energy commits long-term immoral acts that we cannot measure e.g. drives further distances with his or her car, instead of increasing their households' energy consumption. This is a general problem with natural field experiments when one aims to capture a behavioral effect.

To conclude, we are unable to verify any stronger evidence of a long-term moral licensing effect on the energy market. However, we cannot fully rule out the possibility that it might exist either since our result indicate that there is a possibility that only a certain group of people express a moral licensing behavior. As discussed earlier in this section, we cannot rule out that our insignificant results are due to inadequate data, a deficient model or a mix of the two. We would therefore like to see more field studies on moral licensing in a context where subjects are randomly assigned. It is of importance to further study this phenomena, since if a long term moral licensing effect truly exists, we might have to reevaluate the way we perceive environmentally and socially responsible products and services.

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Appendix

Table 7.1b - Standard regression (A)

	No control variables (A1)			Control variables included (A2)		
Variable name	Coefficient	Robust Std. Err.	P-value	Coefficient	Robust Std. Err.	P-value
Switch	-0,0136	0,0147	0,355	0,0001	0,0149	0,994
Price	0,0105	0,0183	0,568	0,0106	0,0187	0,570
Dalarna	-	-	-	-0,0248	0,0426	0,561
Gävleborg	-	-	-	-0,0474	0,0290	0,102
Halland	-	-	-	-0,0076	0,0284	0,790
Jämtland	-	-	-	0,0539	0,0461	0,243
Jönköping	-	-	-	0,1046	0,0496	0,035
Kalmar	-	-	-	0,0348	0,0415	0,402
Kronoberg	-	-	-	-0,0041	0,0445	0,927
Norrbotten	-	-	-	0,0718	0,0299	0,017
Skåne	-	-	-	0,0557	0,0323	0,085
Stockholm	-	-	-	-0,0315	0,0261	0,227
Södermanland	-	-	-	-0,1706	0,0644	0,008
Uppsala	-	-	-	0,0672	0,0466	0,150
Värmland	-	-	-	-0,0239	0,0254	0,348
Västernorrland	-	-	-	-0,0044	0,0541	0,935
Västmanland	-	-	-	-0,0746	0,0253	0,003
Västra Götaland	-	-	-	-0,0198	0,0246	0,421
Örebro	-	-	-	0,0290	0,0272	0,286
Östergötland	-	-	-	-0,0106	0,0485	0,826
Mosaic Group B	-	-	-	0,0036	0,0198	0,854
Mosaic Group C	-	-	-	-0,0491	0,0421	0,244
Mosaic Group D	-	-	-	-0,0030	0,0166	0,857
Mosaic Group E	-	-	-	0,0170	0,0251	0,499
Mosaic Group F	-	-	-	0,0162	0,0302	0,593
Mosaic Group G	-	-	-	0,0064	0,0209	0,760
Mosaic Group H	-	-	-	0,0520	0,0193	0,007
Mosaic Group I	-	-	-	0,0099	0,0185	0,593
Mosaic Group J	-	-	-	0,0246	0,0238	0,301
Mosaic Group K	-	-	-	0,0165	0,0206	0,425
Mosaic Group L	-	-	-	0,0232	0,0194	0,232
Constant	0,0265	0,0051	0	0,0272	0,0285	0,340
Observations		737			737	
R-squared		0,0013			0,0954	

Table 7.1b shows the results from our baseline regression on the standard dataset (A) with all control variables included.

Table 7.2b – Early vs. late switchers regression (B)

	No control variables (B1)			Control variables included (B2)		
Variable name	Coefficient	Robust Std. Err.	P-value	Coefficient	Robust Std. Err.	P-value
Switch	0,0122	0,0136	0,371	0,0150	0,0141	0,286
Price	-0,0067	0,0153	0,662	0,0008	0,0158	0,959
Dalarna	-	-	-	0,0044	0,0319	0,890
Gävleborg	-	-	-	0,0056	0,0337	0,869
Halland	-	-	-	0,0413	0,0318	0,194
Jämtland	-	-	-	-0,0051	0,0356	0,887
Jönköping	-	-	-	0,1251	0,0461	0,007
Kalmar	-	-	-	0,0765	0,0747	0,306
Norrbotten	-	-	-	-0,1141	0,0339	0,001
Skåne	-	-	-	0,0850	0,0586	0,147
Stockholm	-	-	-	0,0172	0,0367	0,640
Södermanland	-	-	-	-0,2355	0,0644	0,000
Uppsala	-	-	-	-0,0569	0,0378	0,133
Värmland	-	-	-	0,0164	0,0323	0,611
Västernorrland	-	-	-	0,2606	0,0648	0,000
Västmanland	-	-	-	-0,2020	0,0148	0,000
Västra Götaland	-	-	-	0,0447	0,0272	0,101
Örebro	-	-	-	0,1072	0,0374	0,004
Östergötland	-	-	-	0,2497	0,0417	0,000
Mosaic Group B	-	-	-	0,0003	0,0242	0,990
Mosaic Group C	-	-	-	-0,0783	0,0394	0,048
Mosaic Group D	-	-	-	-0,0029	0,0260	0,910
Mosaic Group E	-	-	-	0,0258	0,0333	0,438
Mosaic Group F	-	-	-	-0,0237	0,0369	0,520
Mosaic Group G	-	-	-	0,0119	0,0274	0,665
Mosaic Group H	-	-	-	0,0462	0,0393	0,240
Mosaic Group I	-	-	-	0,0120	0,0313	0,701
Mosaic Group J	-	-	-	0,0507	0,0346	0,144
Mosaic Group K	-	-	-	0,0283	0,0325	0,385
Mosaic Group L	-	-	-	0,0386	0,0333	0,247
Constant	0,0210	0,0168	0,211	-0,0249	0,0417	0,552
Observations		669			669	
R-squared		0,0022			0,0897	

Table 7.2b shows the results from our baseline regression on the early vs. late switchers dataset (B) with all control variables included.

Table 7.3b – Standard dataset extension regression

	No control variables			Control variables included		
Variable name	Coefficient	Robust Std. Err.	P-value	Coefficient	Robust Std. Err.	P-value
Switch	0,0365	0,0266	0,174	0,0591	0,0327	0,074
Price	-0,0811	0,0743	0,279	-0,0936	0,0791	0,241
Dalarna	-	-	-	0,2393	0,0327	0,000
Gävleborg	-	-	-	0,0369	0,0327	0,263
Halland	-	-	-	0,0228	0,0440	0,606
Jämtland	-	-	-	(dropped)	-	-
Jönköping	-	-	-	0,0751	0,0327	0,024
Kalmar	-	-	-	(dropped)	-	-
Kronoberg	-	-	-	(dropped)	-	-
Norrbotten	-	-	-	(dropped)	-	-
Skåne	-	-	-	(dropped)	-	-
Stockholm	-	-	-	0,0235	0,0269	0,386
Södermanland	-	-	-	(dropped)	-	-
Uppsala	-	-	-	(dropped)	-	-
Värmland	-	-	-	0,1465	0,0593	0,016
Västernorrland	-	-	-	(dropped)	-	-
Västmanland	-	-	-	(dropped)	-	-
Västra Götaland	-	-	-	0,0822	0,0373	0,031
Örebro	-	-	-	0,1218	0,0327	0,000
Östergötland	-	-	-	(dropped)	-	-
Constant	0,0155	0,0116	0,186	-0,0284	0,0327	0,388
Observations	82			82		
R-squared	0,0307			0,1219		

Table 7.3b shows the results from our extension regression on the restricted standard dataset with all control variables included. The dataset only includes people who live in houses, and belong to a Mosaic group with an average tertiary education level above 40%.

Table 7.4b – Early vs. late switchers dataset extension regression

	No control variables			Control variables included		
Variable name	Coefficient	Robust Std. Err.	P-value	Coefficient	Robust Std. Err.	P-value
Switch	-0,0339	0,0583	0,562	-0,0269	0,0734	0,715
Price	-0,0542	0,0523	0,304	-0,0448	0,0669	0,506
Dalarna	-	-	-	(dropped)	-	-
Gävleborg	-	-	-	-0,0331	0,0000	0,000
Halland	-	-	-	-0,0180	0,0590	0,762
Jämtland	-	-	-	(dropped)	-	-
Jönköping	-	-	-	(dropped)	-	-
Kalmar	-	-	-	(dropped)	-	-
Norrbotten	-	-	-	(dropped)	-	-
Skåne	-	-	-	0,0484	0,0555	0,387
Stockholm	-	-	-	-0,0165	0,0267	0,539
Södermanland	-	-	-	(dropped)	-	-
Uppsala	-	-	-	(dropped)	-	-
Värmland	-	-	-	0,1496	0,1264	0,242
Västernorrland	-	-	-	0,1286	0,0669	0,060
Västmanland	-	-	-	(dropped)	-	-
Västra Götaland	-	-	-	0,0534	0,0483	0,274
Örebro	-	-	-	-0,0376	0,0000	0,000
Östergötland	-	-	-	(dropped)	-	-
Constant	0,0770	0,0479	0,144	0,0669	0,0669	0,322
Observations		63			63	
R-squared		0,0229			0,0819	

Table 7.4b shows the results from our extension regression on the restricted early- vs. late switcher dataset with all control variables included. The dataset only includes people who live in houses, and belong to a Mosaic group with an average tertiary education level above 40%.