Do “Warm Glow” Feelings Affect Climate Mitigation Behaviors?
A Field Experiment on Consumer Electric Energy Choices

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Abstract

Addressing the climate crisis is going to require behavioral change in many aspects of daily life. A growing literature suggests the promise of “warm glow” messaging that features the pleasure and satisfaction one experiences from acting sustainably. Yet previous research has relied almost exclusively on self-reported behavioral intentions, leaving open whether such appeals meaningfully affect real-world, costly choices. We partnered with a local organization to conduct a field experiment on energy choices in Hanover, New Hampshire. Using a randomized roll-out design in early 2025, we systematically varied the timing of postcard mailers that contain a warm glow message encouraging customers to increase the renewable content of their energy option. Analyses of administrative data on consumer energy choices reveal that warm glow messages delivered in a field setting have a null effect on adoption of higher-renewable power choices, with a small number of customers changing to the greener but more costly option. Our study provides novel behavioral evidence on the real-world impact of intrinsic motivation appeals in the climate domain.

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Transparency Statement: The study was pre-registered (https://aspredicted.org/b2fy-pjbk.pdf). Administrative data on consumer energy choices were utilized for this study (i.e., there is no survey or questionnaire to disclose). We obtained approval for the study from town officials and the Institutional Review Board at the authors’ institution.

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> "The physical sciences have reached substantial consensus on best practices for managing natural resources and ecologies, and the main obstacle now is changing individual behavior and policy opinions. United States citizens recognize that environmental problems are serious...but there is a striking lack of voluntary behavior change" (Brick, Sherman, and Kim 2017, 236).

Human decision-making is at "the very core of the climate change problem" (van der Linden, Maibach, and Leiserowitz 2015, 758), which puts political psychologists in a unique position to offer meaningful solutions to this existential threat. In recent years, researchers have converged upon the idea that positive emotions—and feelings of "warm glow" (WG) in particular—are a "severely under-leveraged" strategy for motivating green behavior (van der Linden 2015, 612). In the environmental domain, warm glow refers to the pleasure and satisfaction that one experiences from acting sustainably. Compared to other kinds of climate communications, warm glow messages possess some advantages: they are more influential than appeals highlighting material rewards (Taufik, Bolderdijk, and Steg 2016) and they avoid the most polarizing aspects of the issue (Hart and Feldman 2018). Some even theorize that the effects of WG messaging may be self-reinforcing (Schneider and van der Linden 2023).

Our study investigates whether WG messaging influences green behavior in the domain of energy consumption. In partnership with local organizations in a New Hampshire town in the United States, we conducted a field experiment that randomized the delivery of postcards encouraging customers to increase the renewable content of their energy supply. We investigate the effect of the mailer with unique administrative data on consumer energy choices. The primary goal of the study was to explore whether a subtle warm glow message on the postcard could motivate customers to switch to a higher level of renewable energy.

Motivating Sustainable Behavior

In the political realm, affect has a powerful influence on decision making and behavior, largely through System 1 processes that operate quickly and effortlessly (Jerit and Kam 2023). There is a growing recognition of the importance of affective processes in the climate domain (e.g., Betzler, Kempen, and Mueller 2022; Iweala, Spiller, Meyerding 2019; Nilsson, Hansla, and Biel 2014). In a review of this literature, Brosch observes that "affect and emotions experienced toward climate change [are] consistently among the most important predictors of climate change-related judgements and behaviors" (2021, 15; also see Schneider, Zaval, and Markowitz 2021).

Both positive and negative emotions have been successfully leveraged in climate communications (Brosch 2021), but the former may generate more *durable* effects through a positive feedback mechanism (Schneider, Zaval, and Markowitz 2021, 115). Scholars theorize that "the anticipation of positive emotions from engaging in sustainable behavior [leads] to the subsequent experience of positive emotions, which in turn generates more positive anticipatory emotions" (Schneider and van der Linden 2023, 284; also see Chitraranjan and Botenne 2024 or Spielmann 2021). In contrast, negative emotions (e.g., guilt, anxiety) may initially promote pro-environmental behavior, but engaging in action "reduces the negative emotion and, as a consequence, eliminates the motivation for adopting subsequent [pro-environmental behaviors]" (Truelove et al. 2014, p. 132; also see Schneider, Zaval, Weber, and Markowitz 2017). From this perspective, positive emotions, especially the feelings one experiences in relation to the climate, have the twin advantages of working through fast System 1 processes (e.g., Spielmann 2021) while also generating effects that persist long after the initial exposure to a stimulus.

Within the broad category of positive emotions, an emerging literature highlights "warm glow" (WG) feelings as especially promising for climate communications. Warm glow refers to "the feel-good sense one gains from acting prosocially or sustainably" (Schneider, Zaval, and Markowitz 2021, 116). The concept of WG originated in economics, where scholars sought to account for behaviors, such as volunteerism and public goods provision, that were not well-explained by existing research (Evans and Ferguson 2014; Crumpler and Grossman 2008; Andreoni 1995). Individuals feel good when they do something for others, and these intrinsic motivations can be more powerful than extrinsic (e.g., financial) incentives or rewards. Translated to the climate domain, it is theorized that people help the planet because doing so gives them a feeling of satisfaction (e.g., Venhoeven, Bolderdijk, and Steg 2020; van der Linden 2015).

Existing research suggests the potential of WG messaging for motivating green behavior, but several gaps remain in the evidentiary record. Observational studies affirm that warm glow feelings are associated with sustainable behavior (e.g., Jia and van der Linden 2020; van der Linden 2015; Hartmann et al. 2017; Taufik, Bolderdijk, and Steg 2015; Hartmann and Apaolaza-Ibáñez 2012), but the WG effect is not causally identified, and the outcomes in these studies consist exclusively of self-reports of respondent behavior or behavioral intentions. Answers to such items are prone to bias from overreporting or misremembering.

There also is uncertainty regarding the *scope* of warm glow effects—namely, whether the influence extends to high- as well as low-cost behavior. In a two-wave longitudinal study, van der Linden (2018) finds that anticipated warm glow predicts self-reports of low-cost actions (e.g., unplugging electronics), but not high-cost actions (e.g., buying carbon offsets). By contrast, Jerit, Shin, and Barabas (2024) manipulate WG in the context of a survey experiment and come to a different conclusion. Specifically, the authors report a statistically significant warm glow effect for more difficult green behaviors ($p < .01$) and a more modest effect for less difficult green behaviors ($p < .10$). The conflicting patterns across the two studies may reflect the specific types of behaviors that appeared in each or even the differing research designs. Both studies examine behavioral intentions, underscoring the lack of evidence from real-world decisions.

The small number of studies that employ actual behavioral outcomes—mostly in field experiments—also come to varying conclusions. For example, a WG message increased donations of a recycling credit to an environmental organization compared to a social norm or baseline message (Bergquist, Nyström, and Nilsson 2020). But a WG message placed on the menu of a Swedish fast-food restaurant had no effect on the purchase of vegetarian food options (Reinholdsson et al. 2023). Lohmann et al. (2024) report null effects for a WG treatment on a real-effort pro-environmental task, which consisted of a modified Stroop test completed in exchange for donations to an environmental organization. However, Lohmann et al. (2024) were only partially successful at inducing WG (based on a reported manipulation check), rendering the interpretation of the null effect ambiguous.

Taken together, there is evidence of a WG effect based on studies that employ self-reported items with some uncertainty as to the scope of those effects on low- versus high-cost actions, and a mixed picture of WG influence based on studies that employ behavioral outcomes. Our research fills these gaps by studying energy choices in a Community Choice Aggregation (CCA) program (also called "community power") in the U.S. state of New Hampshire—a setting in which customers faced a tradeoff between the renewable content of their energy plans and the price they pay for energy.

Our Case: Community Power in New Hampshire

Community power programs exist in roughly a dozen states representing 30 million people in the United States (Greenleaf et al. 2023). In these programs, municipal governments purchase power on behalf of residents and businesses. The policy gives communities greater control over their energy choices, which can include the selection of more renewable electricity sources, and oftentimes lower electricity prices (Hsu 2022; O'Shaughnessy et al. 2019). In New Hampshire (NH), community power was established in 2019 by bipartisan legislation. A crucial feature of the law was a provision that established municipal governments as the default supplier unless a customer leaves the program. In simple terms, once a municipality joins community power, customers are *automatically enrolled* at a "default" level of energy service. In this respect, NH's community power program is a classic example of a "nudge" policy (Thaler and Sunstein 2008). Customers have the option to change from the default to a level of service with varying amounts of renewable content. Customers also can leave the program for an alternative energy supplier or return to supply service from the private utility serving the area.

After community power was established in NH, the Community Power Coalition of New Hampshire (CPCNH) was formed to support participating jurisdictions, interact with the wholesale energy market, and create the rate structure for the state (Greenleaf et al. 2023). In New Hampshire, there are four rates (or "levels of service"): "Granite Basic," which has 25.2% renewable content, "Granite Plus" (33%), "Clean 50" (50%), and "Clean 100" (100%).[1] The rate

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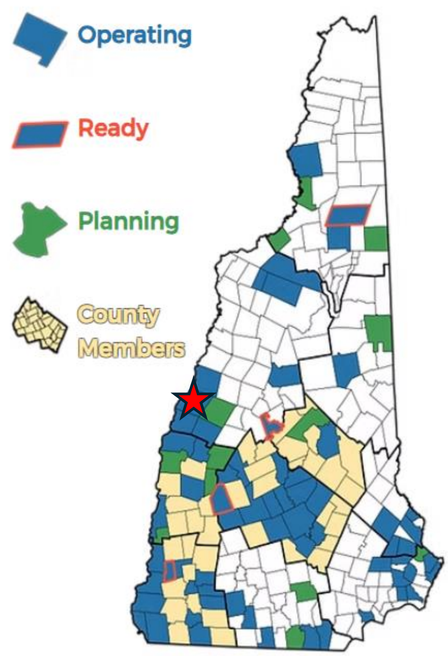
[1] The "Granite" label refers to New Hampshire's nickname as the Granite State. The renewable content for Granite Plus (25.5 %) corresponds to the state-mandated Renewable Portfolio Standard (RPS).

(in ¢/kWh) increases along with the amount of renewable content. At the start of our study, the average bill for a typical customer (using 650 kWh per month) at each of the four levels was: $58, $62, $67, and $80.

Hanover was one of the first places to adopt community power in the state, and the town's plan established Granite Plus (33%, the second-lowest renewable option) as the default level of service. Outside of Hanover, it is more common to adopt the lowest level (Granite Basic, 25.2%) as the default level of service. In NH, community power enjoys strong public support and represents a growing portion of the state's electric customers (Caldwell 2025). Figure 1 shows where community power was operating (blue or yellow), planning to launch (blue with red outline), and not yet operational (white) in the spring of 2025, when our study took place.

Figure 1. Community Power in New Hampshire



Note: Hanover, NH indicated with a red star.
Source: https://www.cpcnh.org/ accessed in the spring of 2025.

Given the automatic enrollment feature of the community power program, our treatment leveraged warm glow to encourage customers to switch to a higher level of service, which would have the practical effect of increasing the renewable content of their energy choice. Some scholars speculate that the warm glow effect is strongest at the "buy in" stage (e.g., a municipality's decision to join community power at a particular default level of service), but customer decisions regarding the level of service are crucial for the actual impact of the policy (Ma and Burton 2016). Moreover, one of the challenges facing this model of energy procurement is customer turnover (O'Shaughnessy et al. 2019). It is important to explore a treatment like the one employed here because of the potential to increase the overall efficacy of the program.

Research Design

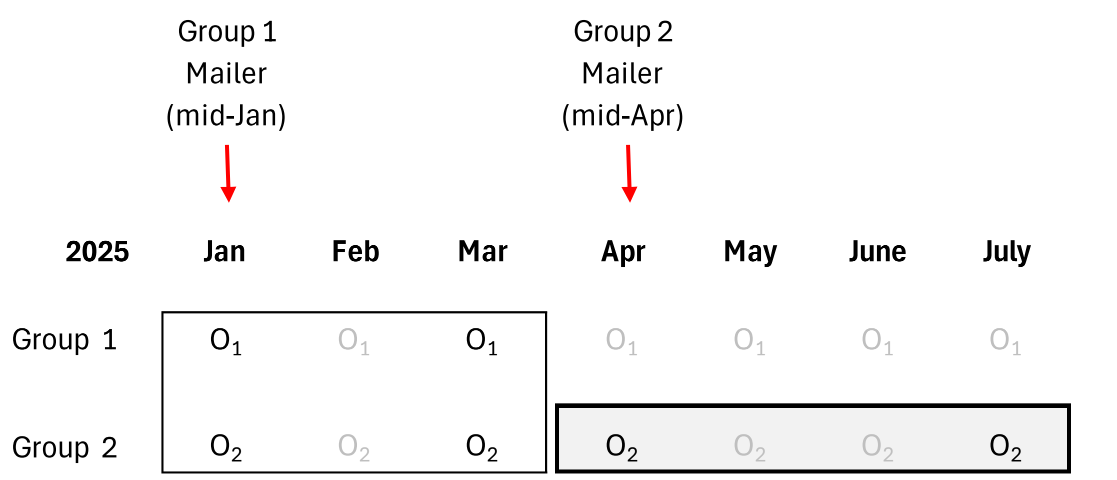
Our study employed a randomized roll-out design with two conditions that vary in the timing of treatment, which was a postcard sent through the USPS.[2] Historically, CPCNH sets rates two times a year: the beginning of January and the beginning of August. Our study was conducted during the initial rate period of 2025 (i.e., January to July). At this time, CPCNH rates were higher than public utility rates for the first time in the program's history, providing a difficult test for the impact of a warm glow message since customers had the option of switching to the less costly utility-provided power.

The field experiment included all eligible energy customers from Hanover, NH (*N* = 2,891) in January 2025. Most of these customers (87%) represent residential accounts, with the

[2] Approval for the study was obtained from town officials and the Institutional Review Board at [redacted]. The study was pre-registered (https://aspredicted.org/b2fy-pjbk.pdf).

remainder consisting of commercial or municipal accounts. In our main analyses we use the term "customer" to represent all account holders, though we distinguish between residential and other types of accounts in later analyses. A schematic of the research design is shown in Figure 2. Observations (denoted "O" in the figure) are power choices made by customers and reported at the monthly level by CPCNH.

Figure 2. Planned Research Design



Note: Customer energy choices ("O") recorded at monthly level by CPCNH. We test our hypothesis in (1) a between-group comparison in the January-to-March period (the change in O_1 versus O_2 in area shown with thin line, and (2) a within-group comparison in the April-to-July period (the change in O_2 before and after mailer in area shown with grey shading and dark line).

As shown in Figure 2, a postcard was mailed to a randomly selected half of energy customers (Group 1; n = 1,444) in mid-January 2025. The remaining half (Group 2; n = 1,447) received treatment in mid-April 2025. Our planned analysis consists of two comparisons. First, we compare the power choices of units in Group 1 to the power choices of units in Group 2 over the January–March period (At this stage, Group 1 represents the treatment condition and Group 2 represents the control condition). Our second comparison is the pre-post contrast among customers in Group 2 during the April–July period. Here the quantity of interest is the change in a customer's power choice across time. Within Group 2, we randomized one subset to get the same postcard twice (i.e., increasing the dose in intervals one week apart). The decision to add this randomization was made after pre-registration but before anyone in Group 2 was treated. [3]

Community power customers in Hanover can remain at the default, *increase* their renewable content (e.g., change from Granite Plus to Clean 100; colloquially referred to as "opting-up"), or *decrease* their renewable content (e.g., change from Granite Plus to Granite Basic; "opting-down"). Based on the existing literature, we hypothesized that customers who had been assigned to receive a mailer with the WG message would be more likely to increase the renewable content of their energy choice compared to customers who did not receive the mailer.

The dependent variable is an ordinal measure of changes in the renewable content of a customer's electricity plan. Positive values indicate that a customer increased the renewable content ("opt-up"), 0 indicates no change, and negative values indicate a decrease ("opt-down"). The theoretical range on this variable is –3 to 3, but the observed range in our data is –1 to 2 because most customers began on the default plan, the second-lowest level of renewable content. A secondary outcome is a dichotomous measure indicating whether a customer increased their renewable content or not (1= opt-up; 0=stay the same or opt-down). These outcomes represent

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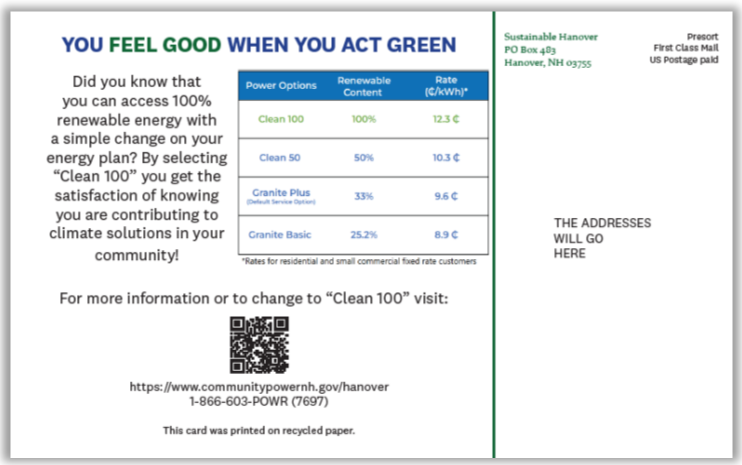
[3] Random numbers were generated with the "`runiform`" command in Stata 18. We transmitted the list of random numbers to CPCNH so the organization could sort the electricity customer files into the Group 1/Group 2 conditions (as well as the additional randomization within Group 2). CPCNH sent the identifying information directly to the printing and mailing office to maintain anonymity of the customer accounts.

the behavior of interest, as opposed to a self-report in which people describe their willingness to pay for green electricity (Hansla et al. 2008). In our main analysis, we treat customers who left the program (n = 22) as missing. The decision to leave the program was unrelated to group assignment ($|t|$ = 0.42; p = .67; df = 2889) and the number of customers doing so was in line with historical data collected by CPCNH. We include and control for this subset in later exploratory analyses.

Our study includes the population of eligible customers as of January 2025. Anyone moving into the community after random assignment was excluded from the study. Attrition occurs when an account becomes inactive (i.e., there is no product choice on record), typically because a customer moves out of the community. The total number of inactive accounts throughout the course of the study (n = 358) was balanced across conditions ($n_{\text{Group 1}}$=182, n_{Group2} = 176; $|t|$ = 0.36; p = .72; df = 2,889).

The Treatment. Following canonical field experiments in political science (e.g., Hopkins, Meredith, Chainani, Olin, and Tse 2021; Gerber and Green 2000), we treated subjects with a postcard. In designing the mailer, we drew upon existing WG research to devise the exact language. We also included an image, which has been shown to enhance text in studies that seek to increase giving (e.g., Leon and Schobin 2023). Once we identified potential images and phrases, we conducted pre-study focus groups with residents of neighboring towns (22 people across 3 sessions). The focus groups helped us refine the text, ensure readability, and select a final image. The logo and background color (right hand side) match promotional material from CPCNH. The front and back of the mailer are shown in Figure 3.

Figure 3. Mailer (Front and Back)



The main message ("YOU FEEL GOOD WHEN YOU ACT GREEN") resembles the exemplars we described earlier (Reinholdsson et al. 2023; Bergquist, Nyström, and Nilsson 2020).[4] Specific words, such as "satisfaction" and "contributing," were based upon existing WG studies (Jerit, Shin, and Barabas 2024; Hartmann and Apaolaza-Ibáñez 2012) as well as focus

[4] In Bergquist, Nyström, and Nilsson (2020), the WG message ("It feels good to donate the recycling credit.") appeared on a sticker. In Reinholdsson et al. (2023), the WG message ("The green option feels good!") appeared next to food items on an interactive digital menu.

group feedback. The card also instructs people how to change their power choice: via QR code, URL, or phone. The rate table (backside of mailer) matches one that appeared on the CPCNH website at the time of our study. We included the table so we could pair the WG message with a specific request—namely, changing to a higher level of renewable content. We also wanted to be transparent about the financial cost of making a change given low levels of knowledge about the program. For the typical customer (using 650 kWh per month), the change from Granite Plus to Clean 100 (the highest level of service) would result in a $22 increase on a customer's monthly bill.

Unplanned Events. Field studies are valuable for investigating how people behave in a "real choice environment" (Schneider, Zaval, and Markowitz 2021, 115), but the real world can unfold in unpredictable ways. In 2025, there were two developments that had implications for our study. First, in an unprecedented move, CPCNH announced new, *higher* rates on January 31, 2025 (an .8¢ increase for each level of service). The rate increase occurred after one of CPCNH's energy contracts fell through and the organization had to negotiate with a new supplier during an unusually cold winter. The new rates would not become effective until March 3, and the difference amounted to a $5 increase for each level of service. The rate increase was featured in news stories throughout the state during the first week of February.[5]

[5] The $5 estimate was calculated by multiplying the new rate by monthly energy usage for the typical customer (650 kWh) at each level of service. Thus, the average monthly bill was projected to increase from $58 to $63 for Granite Basic, from $62 to $67 for Granite Plus, from $67 to $72 for Clean 50, and from $80 to $85 for Clean 100.

The second unplanned event also occurred at the beginning of 2025. At this time, CPCNH allowed forty municipal customers to lock in Granite Basic contracts for the entire year, making these units effectively untreatable. The forty accounts represent about a quarter of the customers in the Granite Basic category and are balanced across experimental conditions ($|t|$ = 0.95; p = .34; df = 2,889). Given that we examine the *change* in power choices, annual contract customers who locked in Granite Basic pricing are excluded from the main analyses (though including them does not offer the results we report below). In exploratory analyses, we estimate the effect of the mailer with controls for annual contract customers. Both the unexpected rate increase and the annual contracts work at cross purposes with our treatment and might conceivably offset a WG effect.

Empirical Results

We begin by showing that there is balance on our key outcome (energy choice) across experimental conditions at the start of the study. For our baseline, we utilize data on electricity choices from December 2024, the month before our study began. As shown in Table 1, the distribution of customer choices is nearly identical across conditions and there is no significant difference in energy choices across conditions, either in a t-test ($|t|$ = 0.51; p = .61; df = 2884) or a Kolmogorov-Smirnov test (D(2886) = .0042; p = 1.00).

Table 1. Electric Power Choices at Baseline before Field Experiment

	December 2024			
	Group Treated January '25 (Group 1)		Group *Not* Treated January '25 (Group 2)	
Power Choice	N	%	N	%
Granite Basic (25% Renewable)	72	5.0	75	5.2
Granite Plus (33% Renewable [Default])	1,280	88.8	1,284	88.9
Clean 50 (50% Renewable)	29	2.0	28	1.9
Clean 100 (100% Renewable)	61	4.2	57	3.9
Total	1,442	100	1,444	100

Another pattern that is apparent from Table 1 is the large number of customers (almost 90%) in the default category of Granite Plus. In that regard, NH's community power program illustrates the effectiveness of nudge policies (Thaler and Sunstein 2008). Once enrolled in the program, few customers change from the default option. A potential disadvantage of this nudge feature, revealed by conversations with local partners, is a lack of awareness about community power. It was common for customers to be unaware that they were "in" the program.

Using administrative data from CPCNH, Table 2 shows the number of customers who changed to a power choice with higher renewable content ("Opt-Up"), did not alter their power choice ("Stayed the Same"), or changed to a power choice with a lower renewable content ("Opt-Down") during the January-to-March period.

Table 2. Change in Power Choices (January to March)

	Δ January to March	
Power Choice	Group Treated January (Group 1)	Group *Not* Treated January (Group 2)
Opt-Up	2	0
Stayed the Same	1,377	1,385
Opt-Down	4	1

Notes: Cell entries are counts of customers in category.

Overall, behavioral change is rare, with only seven customers changing across both groups. In the treatment group (i.e., Group 1 from Figure 2), two customers increased the renewable content in their energy, with one going from Granite Plus to Clean 50 and the other changing from Granite Plus to Clean 100. However, four people in the same group moved in the opposite direction, changing from Granite Plus to Granite Basic. The net amount of change is small and the difference across groups is not statistically significant in a two-tailed test ($|t| = .00$; $p = 1.00$; $df = 2{,}767$).[6]

In Table 2, among those treated in January, more customers opted down than opted up. The timing of these changes, combined with a content analysis of regional newspapers, suggests that this pattern may be related to unexpected rate increase in late January. Although the local paper serving Hanover (*Valley News*) did not run a story on the rate increase, several regional

[6] There is a similar pattern when we utilize the dichotomous dependent variable (1=opt-up; 0=stay the same or opt down): $|t| = 1.42$; $p = .16$; $df = 2{,}767$.

outlets did cover the topic. It is possible, then, that some treated customers became aware of their enrollment in Granite Plus (and the rate they were paying for energy) at a time when there was news coverage about a rate increase. Given the environment surrounding the rate increase, a conservative interpretation of Table 2 is that most customers did not change their plans, despite an external shock that made higher-renewable options less attractive.

These external factors had largely dissipated by the time our mailer was sent to Group 2 in mid-April, providing a more favorable context for detecting any effect of the WG message. We expected that the pre-post comparison of Group 2 would show an increase in power choices with a higher renewable energy content. Table 3 presents the April-to-July comparison for customers treated with a mailer in April, alongside the same comparison for customers who received the mailer in January.

Table 3. Change in Power Choices (April to July)

	Δ April to July	
Power Choice	Group Treated in April (Group 2)	Group Treated in January (Group 1)
Opt-Up	6	2
Stayed the Same	1,227	1,232
Opt-Down	1	2

Notes: Cell entries are counts of customers in category.

As the table reveals, the April mailer had the expected effect on Group 2, with a greater number of customers opting up (n = 6) than opting down (n = 1). However, the corresponding difference-in-differences estimate (i.e., the comparison in how the group treated in April changed over time versus the group treated earlier in the year) is insignificant ($|t| = 1.37$; $p = .16$; $df = 2{,}468$).[7] Even under more favorable conditions, the amount of behavioral change induced by the mailer is small and not detectable using conventional cutoffs for statistical significance (i.e., $p < .05$ in a two-tailed test).

Across the entirety of the study, 10 customers opted up—with 9 out of 10 customers changing to Clean 100 (one remaining customer changed to Clean 50). As of November 2025, those 10 customers remained at this higher level, indicating that once customers opt up, the changes tend to persist. On the flip side, 8 customers opted down, all from Granite Plus to Granite Basic, and those changes also persisted through November 2025. The amount of behavioral change was minimal, but the effect of the treatment was *net positive*—not only in terms of the number of customers opting-up versus opting-down, but also in terms of the change in renewable content (i.e., the change from Granite Plus to Clean 100 is a larger gain in renewable content—33% to 100%—than the corresponding drop from Granite Plus to Granite Basic—33% to 25.2%).

To put the overall level of change into context, we calculated analogous figures for: (1) the neighboring town of Lebanon and Lyme, NH, which have community power programs administered by CPCNH but were not treated with postcards, and (2) Hanover during the January-to-July period of the previous year. Both comparisons convey information about the typical amount of change in energy choices, absent outside intervention.

Changes in energy choices at the town-level are presented in the top part of Table 4. Data from Lebanon and Lyme are informative because the two towns border Hanover. Lyme has a

[7] We observe a similar result with the dichotomous outcome ($|t| = 1.42$; $p = .16$; $df = 2{,}468$).

smaller population than Hanover but is comparable to Hanover in terms of socioeconomic characteristics. Lebanon has a comparable (slightly larger) population than Hanover, but has a lower median income than Hanover, and is served by the same private utility supplier (Liberty Utilities).

Table 4. Aggregate Changes in Power Choices at Town Level

Town	Default Level of Service	Time Period	Opt-Up	Opt-Down
Hanover	Granite Plus	Jan–July 2025	10	8
Lebanon	Granite Basic	Jan–July 2025	1	4
Lyme	Granite Basic	Jan–July 2025	1	1
Hanover	Granite Plus	Jan–July 2024	4	4

Note: Hanover figures were calculated from customer-level data in the field experiment. Lebanon and Lyme figures are based on monthly data provided by CPCNH. Grey shading indicates town that received treatment.

During the first six months of 2025, fewer customers in Lebanon and Lyme opted up compared to Hanover. This difference is notable given that Lebanon and Lyme have a lower default level of service than Hanover and thus have more "room" to increase their level of renewable content. In the bottom part of Table 4, we contrast the pattern observed in our study to the behavior of Hanover customers one year before (i.e., 2025 versus 2024). As with the comparison to untreated Lebanon and Lyme, there are fewer opt-ups in Hanover in 2024 than in 2025. While the number of opt-ups in Tables 2 and 3 is modest, the baseline rate of change (in similar communities and in Hanover itself the year before) is even smaller.

Exploratory Analyses. To further investigate the effect of the mailer, we conducted additional exploratory analyses. After the study began, we randomized Group 2 units into two subgroups: one that would receive a single mailer and another that would receive two (identical) mailers, one week apart. We were interested in whether there was a dosage effect (e.g., McAfee et al. 2017), with a greater number of opt-ups for the subgroup receiving two mailers. Given the small number of customers changing their behavior (Table 3), there is no difference between the groups receiving one versus two mailers ($|t| = .38$; $p = .71$; $df = 1{,}232$).

We also estimated a series of random effects regressions that utilize the panel structure of the data (see Appendix Table 1 for model coefficients). Here the dependent variable is a customer's power choice in a particular month (e.g., January, February, … August). The main independent variable is a dummy term indicating the month of treatment. Because the treatment was introduced at different times across groups (January for Group 1 and April for Group 2), the specification leverages the staggered timing of the intervention in the panel data. The coefficient on this indicator ("Immediate Effect") represents the change in a customer's power choice in the month when the treatment occurs, relative to other months, while accounting for both within-group and between-group variation in the panel.[8] The models include controls for customer type (residential versus commercial), municipal customers with an annual contract at Granite Basic, and customers who left the program. Working with CPCNH, we also identified individuals (n=16) who were aware of the study—either through participation in the focus groups or communication with the researchers—and include an indicator for customers with this prior knowledge.

[8] To allow for a delayed effect we also include a term representing one month after treatment.

In the random effects analysis, the immediate effect of the mailer on electricity choice was not statistically significant. This pattern may have been expected, given the small number of customers opting up to a higher level throughout the period of our study. As for the control variables in the random effects models, expected patterns emerged. Annual contract customers had a significantly lower level of renewable content in their energy choices compared to other customers ($p < .01$, two-tailed). Residential customers and those with prior knowledge of the study had significantly higher levels of renewable content (both *p*s < .01, two-tailed) compared to others.[9] Finally, allowing for delayed treatment effects does not change the substantive conclusion; we find no evidence for an effect of the mailer one month later.

At this juncture, several limitations of the study design deserve mention. First, we estimate an intent-to-treat effect (ITT) and therefore are unable to detect where behavioral drop-off occurs. The null effects we observe could be the result of: (a) customers not seeing the mailer at all; (b) processing the mailer only superficially (e.g., one side but not the other); (c) or logistical hurdles associated with changing the level of service on a person's electricity account. Future scholars could adapt the current design to gain a better understanding of the obstacles that influence treatment take-up.[10] Second, the current study cannot isolate the effect of warm glow

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[9] Customers with prior knowledge of the study were not more receptive to the treatment (e.g., in interactive specifications). As with the larger dataset, the dominant pattern among these customers is one of stability (i.e., most maintained their product choice throughout the study period).

[10] We attempted to collect returned cards, but town officials did not report receiving any. Address information came from the electric utility's billing records and therefore was highly accurate.

apart from the rate information. We employed a randomized roll-out design so we could treat all power customers in the community, but this design choice made it difficult to include a placebo mailer that featured a rate table (without the WG message). Third, and finally, the message urged customers to switch to the *highest* level of renewable energy, Clean 100, as opposed to merely increasing the renewable content of their energy plan (by for example changing from Granite Plus to Clean 50). Given the small amount of text we could include on a post card, we wanted a clear “ask” with the potential for maximal impact. It is possible, however, that our message was off-putting to customers who could not afford to switch to the most expensive plan.

Notwithstanding those limitations, the analyses show that a WG message appearing on a postcard motivated a small number of customers to increase the renewable content of their energy choice, and these changes, once made, persisted for at least four months. This is important because small changes can have a large impact when they are enduring (Goldberg, Gustafson, and van der Linden 2020). Future scholars can build upon our results to further refine warm glow interventions. Although we did not observe a dosage effect with two mailers, a warm glow message may work effectively in conjunction with other messaging strategies. Meta-analyses highlight the power of social norms (Berquist et al. 2023), and prior work on dynamic norms (Sparkman and Walton 2017) suggests that information about other people’s behaviors may amplify WG feelings. In particular, learning that a growing number of community members are increasing the renewable content of their home energy choice might augment the satisfaction one feels from taking that same action. While a subtle warm glow message was insufficient to offset the perceived financial burden of changing to a higher level of renewable energy for most of the customers in our study, it would be fruitful to investigate whether a combined intervention could motivate high-cost sustainable behaviors.

Conclusion

The findings indicate that a warm glow message prompt durable changes in energy choices for a small number of customers, but that the mailer's aggregate impact in realistic, high-cost settings is not statistically significant. From this standpoint, our study is consistent with survey-based research showing that the WG effect is less likely for high-cost, high-effort behaviors (van der Linden 2018).

Several features of the study location deserve mention. We selected Hanover, NH because we could establish a relationship with local partners, and that rapport was essential to gain access to administrative data on energy choices. Yet our site was a challenging location for observing treatment effects. Unlike other participating communities, Hanover has a higher default (Granite Plus instead of Granite Basic). Not only is there less room to opt-up to a higher level of renewable content, but Hanover residents may be reluctant to pay more if they feel like they are already making a substantial investment in green power. The unexpected late-January rate increase, along with the municipal customers locked into annual contracts at the Granite Basic rate, may have further limited the range for observable behavioral change.

Additionally, the location we chose to study—Hanover, NH—is strongly Democratic. This characteristic is noteworthy because past work suggests that WG messages might have a greater impact among audiences who are climate skeptical (Jerit, Shin, and Barabas 2024). As with other behavioral interventions in the climate domain (e.g., Allcott 2015), WG effects are likely to be context dependent. Continued use of randomized interventions and close attention to the moderating characteristics of specific study sites is essential (Bryan et al. 2021, 981). Our study provides valuable empirical evidence regarding the causal effect of a WG treatment in a specific place and time. Logical extensions involve versions of the design in other geographic areas with community power, particularly those that are more politically heterogeneous.

This study adds to the empirical record regarding the psychological processes that undergird green behavior (also see Levine and Kline 2017 or Bolsen, Druckman and Cook 2014), but it does so by clarifying the limits of one prominent approach. Our results suggest that appealing to warm glow in isolation is unlikely to generate large changes for costly behaviors. Such evidence is important given the wide variety of research designs and empirical findings in the literature. For the field of political psychology, the implication is not that emotions are irrelevant, but that emotional appeals must be embedded in broader informational or institutional strategies if they are to result in meaningful changes in sustainable behavior.

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Appendix

Appendix Table 1 presents random effects model output. Administrative data on customer energy choices was recorded each month by the Community Power Coalition of New Hampshire.

Appendix Table 1. Participation in Clean Energy Program by Treatment Status

	Energy Choice (DV: Clean Energy Choice Level from Low to High)			
	Model 1	Model 1 w/ Controls	Model 2	Model 2 w/ Controls
Mailer: Immediate Effect	-.001	-.001	.001	.000
	(.003)	(.003)	(.002)	(.002)
Mailer: 1-Month Delayed Effect	-.002	-.002	.002	.002
	(.003)	(.003)	(.002)	(.002)
Fixed Effects				
Month 2	-.000	.000	-.001	-.001
	(.003)	(.003)	(.002)	(.002)
Month 3	-.005 *	-.005 *	.000	.000
	(.003)	(.003)	(.002)	(.002)
Month 4	-.002	-.002	.002	.002
	(.003)	(.003)	(.002)	(.002)
Month 5	-.003	-.003	.003	.003
	(.003)	(.003)	(.002)	(.002)
Month 6	-.007 **	-.007 **	.004 **	.004 **
	(.003)	(.003)	(.002)	(.002)
Month 7	-.015 ***	-.015 ***	.003	.003
	(.003)	(.003)	(.002)	(.002)
Other Controls				
Residential Customer	--	.076 ***	--	.073 ***
		(.028)		(.028)
Annual Contract	--	-.996 ***	--	-.998 ***
		(.079)		(.079)
Prior Knowledge	--	.725 ***	--	.877 ***
		(.119)		(.118)
Left Program	--	-.450 ***	--	-.029
		(.079)		(.079)
Constant (Intercept)	2.051 ***	2.001 ***	2.050 ***	1.996 ***
	(.009)	(.026)	(.009)	(.026)
Model X^2	37.41***	311.61***	17.35**	284.29***
Rho	.96	.96	.99	.99
Number of Observations	19,154	19,154	19,102	19,102
Number of Groups	2,876	2,876	2,876	2,876

Note: The entries are random effects models with panel data from a 2025 field experiment in Hanover, NH. In the first two columns for model 1, the dependent variable contains values of 4=Clean 100 (100% clean power), 3=Clean 50 (50%), 2=Granite Plus, (33%), 1=Granite Basic (25%), or 0=opted-out of program. In model 2, opt-out months are treated as missing so that only months in which customers could actively choose an electricity product are included in the estimation. See text for additional details. The entries in parenthese are standards errors. *** $p < .01$, ** $p < .05$, * $p < .10$ (two-tailed).

In Appendix Table 1, the dependent variable is a customer's power choice in a particular month (i.e., there are multiple observations for each customer). The main specification (Model 1) uses a 5-point dependent variable, where 0=left the program, 1=Granite Basic, 2=Granite Plus, 3=Clean 50, and 4=Clean 100. In these models, customers who ultimately left the program were given a numerical value representing their power choice (1 to 4) while they were in the program and coded to 0 after they left. A second version of the dependent variable (Model 2) ranges from 1 to 4, where 1=Granite Basic (i.e., 25% clean), 2=Granite Plus ("33%), 3=Clean 50 ("50%"), and 4=Clean 100 ("100%"). Here customers have a numerical value representing their power choice (1 to 4) while they were in the program and coded to missing after they left. Given the exploratory nature of the analysis, statistical significance is based on two-tailed tests.

The key independent variable is an indicator representing the month of treatment (e.g., January for Group 1; April for Group 2, which is designated as "Mailer: Immediate"). We also include a variant of that term to allow for a effect (one month later, "Mailer: 1-Month Delay"), which permits a delayed time period between treatment with the postcard mailer and subsequent power choices (i.e., February for Group 1; May for Group 2).

Columns two and four contain output for augmented versions of Models 1 and 2 that include control terms for different types of customers ("Residential Customer" or "Annual Contract") as well as those who had prior knowledge of study (i.e., residents of the local area who were involved in the study or who were local partners, "Prior Knowledge"), and customers who ultimately left the program ("Left Program").