Better energy cost information changes household property investment decisions: Evidence from a nationwide experiment*

James Carroll, Trinity College Dublin†

Eleanor Denny, Trinity College Dublin‡

Ronan C. Lyons, Trinity College Dublin#

With buildings accounting for roughly 40% of energy consumption in the US and Europe, energy efficiency upgrades will be central in meeting climate targets. Based on the hypothesis that there is imperfect information regarding the cost-saving implications of efficiency improvements, we add property-specific energy cost labels to sales advertisements in a randomized controlled trial covering the entire Irish housing market. This is the first energy framing field trial for property, the household's largest energy consuming investment and the household technology which likely has the highest variation in energy consumption due to heterogeneity in efficiency and size. Our analysis of over 31,000 transacted properties finds strong evidence that energy cost forecasts change homebuyer behaviour, with the energy efficiency premium increasing by 0.7 percentage points in treatment counties. We also find that more energy efficient properties sell faster and, for the first time, show that treatment further shortened this time-to-sell. While a major departure from existing property labelling policy, these results suggest that framing property energy efficiency according to cost implications rather than kilowatt-hours increases the demand for energy efficiency.

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† Department of Economics, Trinity College Dublin; jacarrol@tcd.ie

‡ Department of Economics, Trinity College Dublin; dennye@tcd.ie & research fellow of Payne Institute, Colorado School of Mines

Department of Economics, Trinity College Dublin & Spatial Economics Research Centre, LSE; ronan.lyons@tcd.ie

The UN Paris Agreement (United Nations 2015) reinforced global commitments to maintain average temperature to 'well below 2°C above pre-industrial levels'. In response, around sixty countries, including all countries in the European Union (European Council 2019), committed to reach climate neutrality by 2050. A range of deep policy reforms will be required to increase the rate of technological investment by the private sector, such as changes in relative prices (carbon pricing), regulations, quotas, and subsidies. However, private agents often miss cost-minimizing investment opportunities by failing to trade a higher upfront price for lower streams of energy expenditures over the life of the technology, a mis-optimization known as the 'energy paradox' (Jaffe and Stavins 1994b) with severe negative environmental externalities (Creutzig et al. 2018).

There is a long list of potential factors that may slow the rate of private sector investment into energy efficiency. For example, it is possible that "inattention" to energy efficiency (Allcott and Greenstone 2012, Sallee 2014) reflects the complexities of forming multi-period comparative energy cost expectations. In this regard, numerous studies find gaps in household energy literacy levels (Turrentine and Kurani 2007, Allcott 2011, Heinzle 2012, Brounen, Kok, and Quigley 2013, Sovacool and Blyth 2015) and such gaps likely impact investment decisions. There is also considerable debate in the literature as to whether the energy savings associated with such investments match technical *ex ante* consumption forecasts (Allcott and Greenstone 2012, Fowlie, Greenstone, and Wolfram 2018). Such an 'energy performance gap' (de Wilde 2014) may be the result of 'prebound effects' (biases in technical capabilities) (Sunikka-Blank and Galvin 2012) and/or rebound effects (post-adoption increase in energy services) (Greening, Greene, and Difiglio 2000).

Energy efficiency labels are a cornerstone of international informational policy to guide households towards technologies which consume less. In the EU, there is an explicit assumption regarding household imperfect information and a further assumption that removing this gap would increase the demand for efficiency. For example, the EU Car Labelling Directive 1999/94/EC states that "…the provision of accurate, relevant and comparable information on the specific fuel consumption and CO2 emissions of passenger cars may influence consumer choice in favor of those cars which use less fuel and thereby emit less CO2".

For property sales in the EU, categorical, color-coded energy efficiency labels display efficiency rankings ('A' through 'G', for example) and kilowatt-hour estimates (kWh per metre squared per year, for example) to aid decision-making. However, even when clear and accurate comparative energy consumption information is available to adopters, there may be a subsequent transaction cost associated with converting this information into the monetary forecast required to inform private cost-minimizing decision-making. For example, survey tests show that calculations which combine size, kWh and energy prices over the lifetime of an investment are problematic for households and can be significantly biased, although not necessarily implying an undervaluation of energy savings (Allcott 2011, Heinzle 2012).

This paper explores whether this specific monetary-related benefit of energy efficiency upgrades is missing or biased during the investment decision, and whether such an information problem reduces the demand for more energy efficient properties. We test this using a year-long randomized controlled trial (RCT) covering all property sales in Ireland. Online property advertisements in treated counties (14 of Ireland's 26 counties) received a new property-specific annual energy cost label based on the building's energy efficiency rating, floor area and average residential energy prices. This information was displayed on a comparative colour-coded scale similar to existing labels in the EU and was automatically generated within the online platform when the property was first advertised.

While a large body of empirical evidence shows that households value property energy efficiency within a labelled environment (Brounen and Kok 2011, Fuerst et al. 2015, Fuerst et al. 2016, Jensen, Hansen, and Kragh 2016, Chegut, Eichholtz, and Holtermans 2016, Cajias and Piazolo 2013, Mudgal et al. 2013, Hyland, Lyons, and Lyons 2013, Stanley, Lyons, and Lyons 2016), to the best of our knowledge, this is the first paper to test the effects of different efficiency framing formats within a revealed preference setting for this sector. This is an important contribution as the property is typically a household's highest energy-consuming "product". Furthermore, compared to other household technologies, such as appliances and cars, the variation in property energy costs is significantly wider due to higher heterogeneity in energy efficiency and property size.

Results show a large and significant increase in the energy efficiency premium in treatment counties, from 1.7 (pre-trial) to 2.4 percentage points for each unit increase in the 15-point efficiency scale. This effect is seen in transaction prices only – listed prices were unaffected, implying that the result is driven by demand effects post-advertisement. Further, the ratio of the energy efficiency across control and treatment was stable in the years before the trial. In addition, this is the first paper to show that treatment increased the speed at which more energy efficient properties sold, another valid proxy of demand increases.

Our results build on a growing literature that finds higher demand for energy efficiency when savings are framed in monetary terms. In the US, Newell and Siikamäki (2014) find that the willingness-to-pay for efficient water heaters is highest when annual consumption costs are combined with more general informative aids. Min et al. (2014) and Blasch, Filippini, and Kumar (2017) find similar effects for lightbulbs, while Andor, Gerster, and Sommer (2017) shows that EU labels combined with annual operating cost information increases the demand for more energy efficient refrigerators. Kallbekken, Sælen, and Hermansen (2013) find that the combination of lifetime (ten-year) labels with staff training related to energy efficiency led to an increase in efficient tumble dryer sales in Norway. The duration of labeled cost forecasts may also be important – Heinzle (2012) shows that the demand for efficient televisions is considerably higher when ten-year costs are displayed relative to one-year, the 'reversed pennies-a-day' effect, following

Gourville (1998).[1] While we cannot corroborate this latter result, it is possible that the time-horizon of our cost labels (one year) was long enough to capture such a duration effect.

The rest of the paper is structured as follows. The next section presents the theoretical model used to describe the energy upgrade decision. Section II then describes the RCT: experimental and treatment design. Section III and Section IV present the datasets and results, respectively. Section V comments upon the impacts of these results.

I. Theoretical Model and Hypothesis

Motivated by the model proposed by Allcott and Greenstone (2012) to explain the 'energy efficiency gap', this section describes the energy investment decision using an intertemporal cost-benefit framework. While this model is presented within the context of an energy upgrade (of an existing property), it is equally applicable to the overall property decision which includes the energy efficiency attribute.

A. Household Energy Efficiency Upgrade Model

In Equation 1, the household uses m units of energy services (for example, a cubic metre of warm air) per year and the upgrade reduces the energy consumption per unit (for example, kilowatts of electricity) from e_0 to $e_1(e_0 > e_1)$ at energy price p. We assume that all other property attributes are unchanged.[2] The upgrade is loan financed with annual repayments of principal (l) plus interest (i) over term T_l (years). The life of the technology

[1] The 'pennies-a-day' effect states that prices appear lower and more attractive when they are framed into a series of smaller, daily expenses. For example, car dealers often only highlight the cost of monthly instalments (and suppress the total cost). The 'reversed pennies-a-day' effect therefore implies that multiple smaller costs will appear larger if aggregated over longer timeframes.

[2] We fully acknowledge that a property's condition is likely correlated with energy efficiency. However, in the context of a RCT, this bias will be present in both experimental groups.

is T_e and all future cash flows are discounted exponentially (governed by parameter r).[3] The upgrade also leads to an immediate once-off increase in dwelling value (v). Ignoring expectation operators and property/household-specific subscripts, the household will upgrade if:

(1) $$\sum_{t=1}^{T_l} \frac{l+i}{(1+r)^t} < \sum_{t=1}^{T_e} \frac{mp(e_0-e_1)}{(1+r)^t} + \Delta v$$

Failure to invest when energy savings and property value appreciation exceed the cost of investment is generally known as the 'energy paradox' (Jaffe and Stavins 1994b) resulting in an 'energy efficiency gap' (Jaffe and Stavins 1994a). However, the size and even existence (Allcott and Greenstone 2012) of a gap will depend on whether unobserved opportunity or utility costs are included. In Equation 2, we account for this by including transaction costs (θ), such as the time and effort associated with planning the upgrade and disruption during works (or the time an effort involved in moving to a more efficient home), and adoption costs (α), such as learning and potentially changing household routines according to new technologies.[4] Equation 2 also includes unobserved benefits associated with an upgrade, such as potentially improved health effects (δ) (Hamilton et al. 2015), the convenience and comfort (Coyne, Lyons, and McCoy 2018) associated with, for example, more accurate and automated heating controls (π), and, given the positive externalities and intergenerational altruistic components associated with household emission reductions, there are likely altruistic or 'warm-glow' effects, ω, at play, too (Andreoni 1990, Frederiks, Stenner, and Hobman 2015).

(2) $$\sum_{t=1}^{T_l} \frac{l+i}{(1+r)^t} + \theta + \alpha < \sum_{t=1}^{T_e} \frac{mp(e_0-e_1)}{(1+r)^t} + v + \delta + \pi + \omega$$

[3] We acknowledge that the discount rate for energy savings of the sophisticated investor will depend on the interest rate.
[4] Allcott and Greenstone also include "net costs" in their equation.

Within this framework, there are a number of market failures which would explain the under-adoption of energy efficiency. For example, imperfect information (or biased expectations) regarding energy savings ($e_0 - e_1$), the lifetime of the product (T_e) or the energy efficiency sales premium (v) will clearly affect the investment parameters and market outcomes. In addition, for many households, energy prices and the unit of electricity (the kilowatt-hour) are unfamiliar metrics (Sovacool and Blyth 2015, Brounen, Kok, and Quigley 2013).

There could also be market failures within the financial system. For example, when loan terms (T_l) are shorter than technology life (T_e), there is a higher probability that household non-energy consumption will be reduced up until year T_l (as energy savings will not cover loan repayments). If households cannot absorb this short-term energy efficiency shock, they will not invest, despite a positive returns over the full lifetime of the technology upgrade.

Downwardly-biased energy price expectations would also reduce the benefits of upgrading, both through the energy savings channel and the property value channel (assuming future energy costs are capitalised into dwelling values). There are also likely interactions between energy price expectations and property price (appreciation) expectations through size effects, in that larger dwellings would be disproportionally impacted by higher energy price growth. Similarly, dwellings located further away from urban centres (with higher commuting costs) would be disproportionally impacted.

It is also likely that behavioural biases reduce investment in energy efficiency. For example, irrationally high discount rates (Frederick, Loewenstein, and O'Donoghue 2002) or short investment horizons (less than T_e) clearly reduce the discounted benefits (observed and unobserved) of adoption. Furthermore, Prospect Theory (Kahneman and Tversky 1979) shows that our appraisal of uncertainty is heavily dependent on whether it is framed as a gain or a loss, relative to our certain reference point. For example, when prospective adopters compare an unfamiliar heating source to a familiar (for example, a standard boiler to a heat pump), they may psychologically inflate the disutility of a possible loss (a breakdown) and discount the benefits of potential gains (the energy savings) relative to the perceived less-risky reference point. In addition, Kahneman and Thaler (2006) question the ability of decision-makers to accurately forecast future utility, which may be particularly relevant in the case of benefits that have not been pre-experienced by the adopter (such as health, convenience and comfort).

B. Research Question and Potential Mechanisms

Our main hypothesis relates to imperfect information regarding the specific monetary savings associated with efficiency improvements in Equation 2 ($mp(e_0 - e_1)$). This hypothesis is indeed general and we acknowledge that there are potentially different mechanisms at play. For example, surveys exploring household vehicle decisions (Turrentine and Kurani 2007, Allcott 2011) show that many are simply inattentive to energy efficiency at the point-of-sale. As with vehicles, the property is multi-attribute 'product' and prospective adopters may reduce the set of target attributes to a smaller consideration set (Shocker et al. 1991), which may not include the energy efficiency. In addition, given the complex intertemporal trade-offs associated with the energy efficiency attribute, it is likely that many households will experience information overload (Jacoby, Speller, and Berning 1974) and a high cognitive load when combining technical energy units with energy price and energy service expectations to form energy costs forecasts (demonstrated by Heinzle (2012) and Allcott (2011), although not necessarily always downwardly biased). The complexity of such calculations become even more challenging for technologies with longer lifetimes, such as property.

We explore this specific monetary information problem by adding property-specific energy cost forecasts to advertisements on Ireland's largest property website, daft.ie. For energy-inattentive households, this new information may bring energy costs into their consideration set of property attributes. For attentive households with biased energy cost expectations, we would expect to see an increase in demand only if labelled energy savings in the trial are higher than their pre-trial expectations. In both cases, we would expect this to translate into higher demand for energy efficiency and a higher sales premium for low-consumption properties. The property sector is a very interesting case study to test imperfect energy cost information, mainly because the variation in energy costs is extremely wide due heterogeneous property size and efficiency.

II. Energy Efficiency Labelling RCT

Since 2013, all property advertisements in Ireland are required to include a *Building Energy Rating* (BER). This label displays the energy used for space and hot water heating, ventilation and lighting (Sustainable Energy Authority of Ireland 2013). The key metric with the BER is a property's kWh/m^2/year, which is displayed on a 15-grade colour-coded scale (Panel A of Figure 1). Advertisement regulations stipulate that a property's individual BER grade is required only (without the full colour-coded comparative scale) for all sale and rental advertisements (Panel B of Figure 1). The BER is calculated using technical thermal details from the building. It does not account for any behavioural effects which may follow an upgrade, such as rebound effects.

The new property-specific energy cost label was created using three components: property size, energy consumption per year (kWh/m^2/yr from the BER) and the price of energy (published quarterly by the Sustainable Energy Authority of Ireland). This method follows the Sustainable Energy Authority of Ireland's online energy cost tool "See what a difference a BER makes!" (see Appendix Figure A1). We provide an example of the energy cost calculations in Table 1.

Panel A. BER example

Panel B. BER advertisement examples

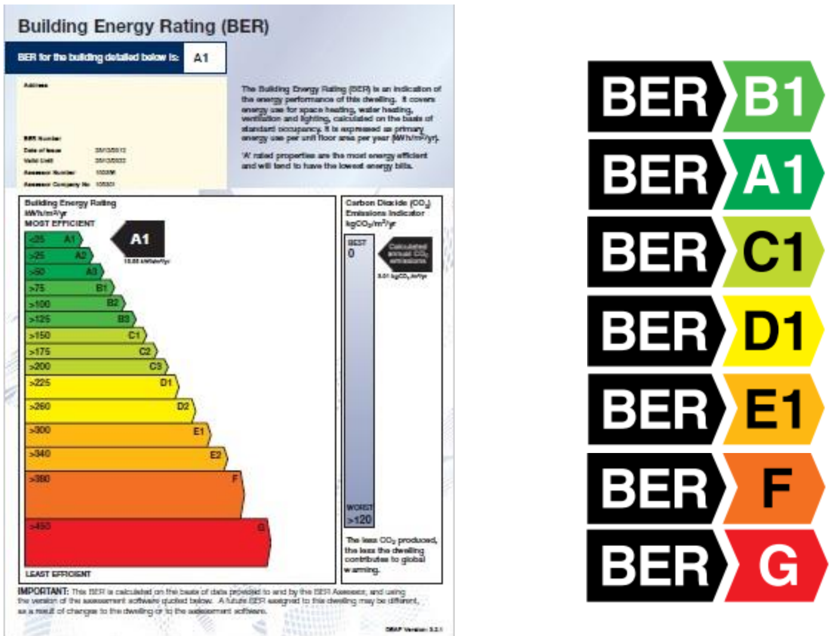


FIGURE 1. THE BUILDING ENERGY RATING (BER)

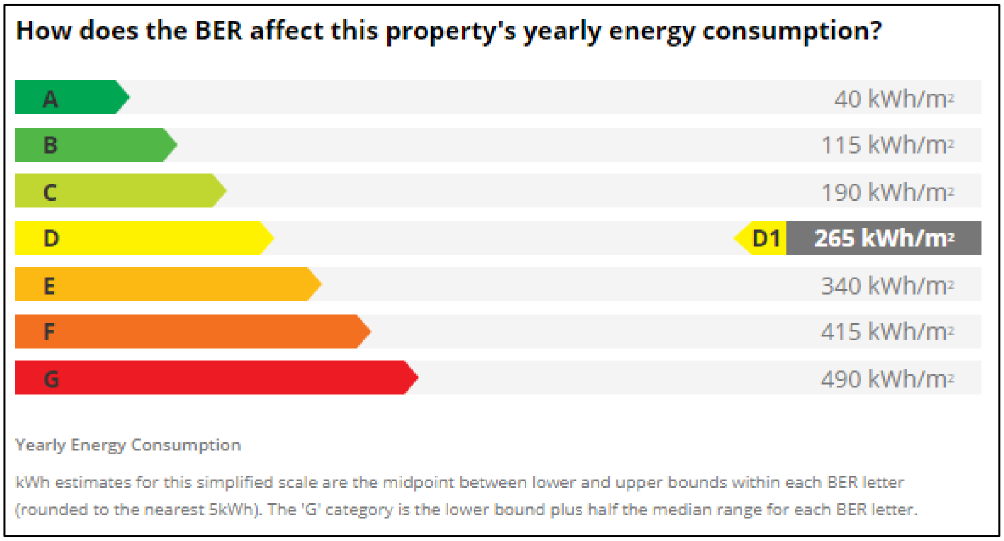
Source: Sustainable Energy Authority of Ireland

TABLE 1—EXAMPLE OF ENERGY COST CALCULATION FOR RCT

	Values	Code	Formula
BER (kWh/m²/yr)	350	a	
Size (m²)	100	b	
Cost of electricity (€/kWh)	0.1992	c	
Cost of Gas (€/kWh)	0.0678	d	
Cost of Oil (€/kWh)	0.0582	e	
Energy for light and pumps (kWh/m²/yr)	20	f	
Delivered energy for lights and pumps (kWh/m²/yr)	8	g	
Cost of lights and pumps (€/m²)	€1.59	h	g * c
Cost of heating (€/m²)	€20.79	i	(a - f) * ((d + e)/2)
Total annual energy cost	€2,238.36	j	(h + i) * b

Source: Calculations are based on the methodology used for the Sustainable Energy Authority of Ireland energy cost calculation tool "See what a difference a BER makes!" Energy prices are published quarterly by the Sustainable Energy Authority of Ireland

Panel A. Control Group Label



Panel B. Treatment Group Label

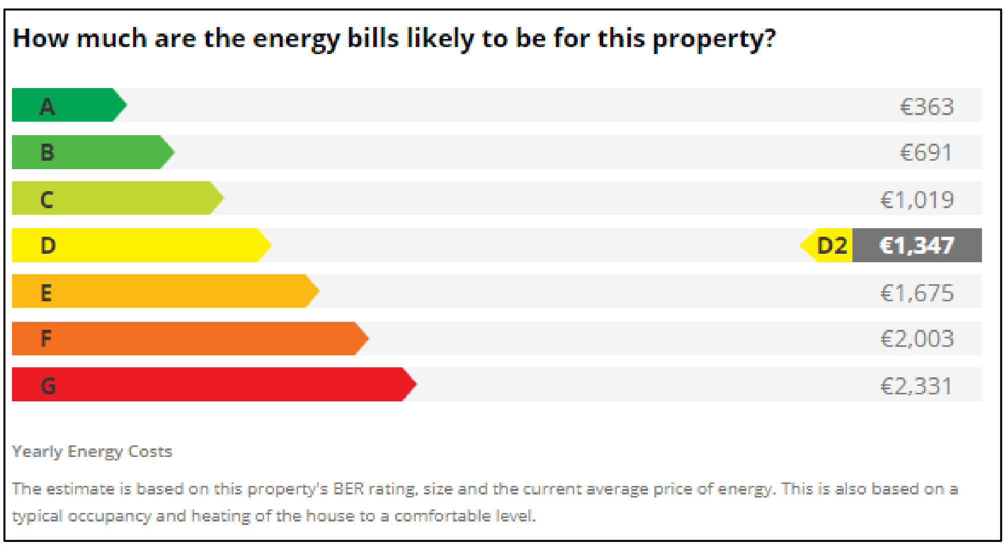


FIGURE 2. RCT LABEL EXAMPLES

Source: Designed by the authors and daft.ie

The monetary label is displayed in Panel B of Figure 2. Relative to the pre-trial advertisement format (Panel B of Figure 1 above), our new label contains two new and separate components: monetary framing of energy efficiency *and* a graphical comparative categorical scale. As both could change buyer behaviour, we isolate the independent effects of the monetary metric by including an identical categorical scale in the control group that is based on kWh/m²/annum information only (Panel A of Figure 2). The new labels were displayed between February 2018 and March 2019, in addition to existing BER advertising requirements at the bottom of the advertisement and were automatically generated within the advertising platform.

Treatment was randomly assigned across the 26 counties in Ireland. While this is a smaller than the optimal number of experimental units, our design was constrained by buyer search patterns, which are generally within county, and further disaggregation would have led to treatment contamination, that is, buyers learning about energy costs from a treatment area and applying this new knowledge to a control area.

TABLE 2: CONTROL AND TREATMENT COUNTY ALLOCATION

Control	N	%	Treatment	N	%
Cork	29,778	11.25%	Carlow	2,772	1.05%
Galway	14,758	5.58%	Cavan	3,743	1.41%
Kerry	7,059	2.67%	Clare	5,352	2.02%
Kilkenny	3,628	1.37%	Donegal	6,267	2.37%
Laois	4,209	1.59%	Dublin	91,668	34.65%
Leitrim	2,343	0.89%	Kildare	10,918	4.13%
Limerick	10,034	3.79%	Louth	7,262	2.74%
Longford	2,458	0.93%	Mayo	7,422	2.81%
Roscommon	3,708	1.40%	Meath	8,366	3.16%
Tipperary	6,231	2.35%	Monaghan	1,555	0.59%
Westmeath	5,559	2.10%	Offaly	2,805	1.06%
Wexford	8,551	3.23%	Sligo	3,780	1.43%
			Waterford	8,217	3.11%
			Wicklow	6,148	2.32%

Notes: data are from January 1st 2017 to January 3rd 2019 and include rental and sales and are used to illustrate county shares only.
Source: own calculations based on daft.ie dataset

We applied simple randomization with one exception – Dublin County and city (the capital) were combined with neighbouring counties (Meath, Kildare and Wicklow) and imposed to the treatment group to account for the larger commuter radius surrounding the capital. The final county allocation is displayed in Table 2 with county numbers and shares (of total dataset). County shares generally range between 1% and 6% with two main exceptions – Cork (11%) and Dublin (34%).

III. Data and Methods

Irish properties are sold using a decentralized auction managed by the sales agent: following the advertisement of the initial list price (jointly agreed by seller and agent), there is a period of anonymous bidding rounds (no lower bound on initial bid), which ends at the seller's discretion. Therefore, if the new energy cost label increases the demand for the energy efficiency attribute, we would only expect to observe this in a higher transaction (final closing) price, and not in a higher list price.

Official transaction price data was sourced from the Irish Property Price Register (PPR) and merged to the advertising database using address and county. This merge was carried out after a large number of standardisation procedures for address strings in both datasets: removal of punctuation, spaces, counties and the standardization of common address terms (such as 'road' and 'street', for example).[5] The final merged dataset was based on exact matches in county and the first five characters of the address string and an 80% match for the remaining string of characters (known as a 'fuzzy merge'). Finally, we drop any properties where the closing sales date is not within a year after the advertising date to remove properties potentially sold multiple times during the period.

Table 3 displays descriptive statistics for the full sample (pre-merge) and the final sample (post-merge) for analysis. Differences in addresses (format, spelling and order) between daft.ie (added by the estate agent) and the PPR (added by the solicitor), unsold properties in the daft.ie dataset (and therefore no corresponding record in the PPR), and delays between sale date and PPR registration date, all lead to a significant reductions in sample size: 66% reduction in the control group (and 7.9% decrease in mean price), and 60% reduction in the treatment group (1.6% increase in price).

[5] A "fuzzy" merge was carried out in STATA 14 using the "reclink" command.

Table 3 shows that, within the final 'post-merge' sample, there are differences across experimental groups, largely driven by the inclusion of Dublin in the treatment group. For each variable except category "B" BER, all differences, while small in magnitude, are all statistically significant, particularly in relation to prices which is 58% higher in the treatment group. This large price difference declines considerably when Dublin is removed from the sample (not shown but tested in the robustness checks in Section 5).

TABLE 3: DESCRIPTIVE STATISTICS FOR REGRESSION SAMPLE

	Control Group		Treatment Group	
Full Sample (pre-merge):	N = 32,222		N = 53,065	
	Mean	Standard Deviation	Mean	Standard Deviation
Transaction Price (€)	230,365	137,186	330,465	220,906
Bedrooms (#)	3.4	0.9	3.1	1.0
Bathrooms (#)	2.2	1.0	2.0	1.0
Apartment (%)	9.5%	29.3%	20.1%	40.1%
BER: A (%)	2.0%	14.1%	2.6%	15.9%
BER: B (%)	10.7%	30.9%	9.5%	29.4%
BER: C (%)	41.3%	49.2%	38.1%	48.6%
BER: D (%)	23.5%	42.4%	24.9%	43.2%
BER: E (%)	9.7%	29.6%	12.4%	33.0%
BER: F (%)	5.0%	21.8%	5.9%	23.6%
BER: G (%)	7.7%	26.7%	6.5%	24.7%
	Control Group		Treatment Group	
Final Sample (post-merge):	N = 10,913		N = 21,388	
	Mean	Standard Deviation	Mean	Standard Deviation
Transaction Price (€)	212,243	118,766	335,593	216,920
Bedrooms (#)	3.3	0.9	3.0	0.9
Bathrooms (#)	2.1	0.9	1.9	0.9
Apartment (%)	10.2%	30.3%	20.2%	40.1%
BER: A (%)	0.5%	7.3%	1.1%	10.3%
BER: B (%)	9.5%	29.3%	9.1%	28.7%
BER: C (%)	43.5%	49.6%	38.4%	48.6%
BER: D (%)	24.4%	43.0%	25.8%	43.7%
BER: E (%)	9.7%	29.7%	13.2%	33.9%
BER: F (%)	4.8%	21.4%	6.5%	24.6%
BER: G (%)	7.5%	26.3%	6.0%	23.7%

Notes: data are from January 1st 2017 to February 28th 2019
Source: own calculations based on daft.ie and PPR data

We estimate the treatment effects using a hedonic difference-in-differences regression. Ignoring subscripts, constant, controls and the error term, the empirical specification is as follows:

$$(3) \qquad \log(Y) = \beta_1 E + \beta_2 P + \beta_3 T + \beta_4 (E * P) + \beta_5 (E * P * T)$$

where Y is price, E is energy efficiency, P is the trial period dummy and T is the treatment dummy. The key coefficients of interest are β_1 (the pre-trial relationship between efficiency and price in the control group), β_4 (the change in this relationship during the trial) and β_5 (how this change differed for the treatment group). This same approach is also used to explore the time-to-sell, which we define as the duration between the advertising date and the date the property was registered on the Irish Property Price Register (PPR).

We assign a property to the pre-trial period if it was advertised from 1st January 2017 (earliest date in the data provided to us) and sold before 31st January 2018. Properties advertised between 1st March 2018 and 28th February 2019 are considered to be in the trial period (February 2019 excluded entirely due to implementation issues in the first month). In addition, we excluded properties with no energy efficiency information (usually protected structures) and newly built properties as they are all A-rated by regulation and they do not sell through the usual auction process, where treatment effects are expected to take hold. Finally, unusually large (more than six bedrooms or bathrooms) or expensive (more than €2 million) properties were excluded, as were land sales. Where listings with duplicate addresses occurred, only the latest was kept in the analysis.

IV. Results

The OLS results for transaction price and time-to-sell are displayed in Table 4. Energy efficiency is included as a continuous fifteen-grade BER scale from category 'G' to 'A1' (see Figure 1 above). In all models, we control for size (number of bedrooms and bathrooms), building type (dummy variables for apartment, bungalow, detached house, duplex house, end-of-terrace house, semi-detached house, terraced house and townhouse), market conditions (dummy variables for each month) and location (1,425 separate locations using daft.ie area codes). We also interact the time variables with a dummy for Dublin and surrounding counties as these counties have historically displayed differed price growth dynamics. Standard errors are clustered by the location variable.

The estimated energy efficiency premium from this regression (β_1) need not necessarily be the causal effect, as missing property attributes, such as internal property condition and age (discussed further below) may be correlated with energy efficiency. Similarly, changes in the energy efficiency premium over time (β_4) may reflect factors such as changes in buyer and seller market power. However, the three-way (efficiency-trial-treatment) coefficient (β_5) is causal due to randomization.

A large and statistically significant efficiency premium is evident from the results. For the control group pre-trial, each categorical increase on the 15-point BER scale is associated with a 4% higher transaction price. During the trial, this energy efficiency premium declined by 0.5 percentage points (PPs), which may be due to market stress as a result of severe supply shortages in many Irish urban areas during 2018. For the treatment group, the pre-trial energy efficiency premium was about half that of the control group in the same period (1.7%). However, the three-way efficiency-trial-treatment coefficient is positive and significant and shows that the energy efficiency premium increases by 0.7 PPs more during the trial period in treated counties. In other words, the energy efficiency premium in treated counties rose by two-fifths when property-specific energy cost forecasts were included.

TABLE 4: DIFFERENCE-IN-DIFFERENCES RESULTS FROM OLS HEDONIC REGRESSION

	Transaction Price	Time-to-Sell	List price
Efficiency	0.040	-1.914	0.043
	(0.003)	(0.504)	(0.003)
Efficiency * Trial	-0.005	0.942	-0.003
	(0.002)	(0.644)	(0.002)
Efficiency * Treatment	-0.023	0.781	-0.023
	(0.003)	(0.579)	(0.003)
Trial * Treatment	-0.064	8.725	-0.021
	(0.024)	(5.636)	(0.024)
Efficiency * Trial * Treatment	0.007	-1.521	0.003
	(0.003)	(0.736)	(0.003)
Area Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Number of Bedrooms	Yes	Yes	Yes
Number of Bathrooms	Yes	Yes	Yes
Dwelling Type Controls	Yes	Yes	Yes
Model Stats:			
N	31,822	31,822	31,822
Adjusted R-squared	0.521	0.058	0.610
F statistic	147.646	55.148	173.387
Prob. > F	0.000	0.000	0.000

Notes: Standard errors (in parenthesis) clustered by area controls. * indicates interaction. Remaining two-way interactions between efficiency, treatment and year are included in all regressions but none are statistically significant and excluded. Dwelling type dummies include apartment, bungalow, detached house, duplex house, end-of-terrace house, semi-detached house, terraced house and townhouse. Time fixed effects are month dummies interacted with a dummy for Dublin and surrounding counties (to allow for separate price trends)
Source: own calculations using daft.ie data and PPR data

The second model in Table 4 shows the results for time-to-sell (difference between closing date and advertised date). Unlike prices, there are no differences across experimental groups pre-trial (the efficiency-treatment interaction is not significant) and there is no change for the control group during the trial (efficiency-trial interaction not significant). However, we find that more efficient properties sell faster, and that treatment has reduced this time-to-sell even further. In the control group, each unit increase in efficiency reduces selling time by 1.9 days. After treatment, this effect increases to 3.4 days (1.9 days plus 1.5 days). Therefore, in treated counties, an A1-rated property sells 51 days faster than a G-rated property, compared to 28.5 days in the control group.

The final column in Table 4 presents results for list price: when closing prices are replaced with list prices the three-way interaction term capturing the treatment effect is not statistically significant. This is compelling evidence that transaction prices are not capturing otherwise unobserved factors, at either dwelling or market level, that only affect treated counties. It is also consistent with the modified label being a demand-led intervention targeted at buyers and we believe it is strong evidence that the three-way interaction term is not capturing otherwise unobserved market conditions at county level.

We test the robustness of these results in four further ways, outlined in Appendix Table A1.

- Firstly, given the market-wide scale of the intervention, we explore the potential for contamination effects – that is, treatment effects spilling over from treatment counties to control counties over time. This is done by estimating the model for the first six months of trial data, rather than the full twelve. There is some evidence that the treatment effect declined slightly over time: 0.8 percentage point (PPs) after six months but 0.7 PPs after twelve.
- Secondly, theory suggests that the negative effects of energy inefficiency increase with size. Therefore, we explore whether the effect is greater for larger dwellings (3-5 bedroom properties). We find that the exclusion of one- and two-bedroom properties increases the treatment effect to 0.9 PPs.
- Thirdly, we explore the impact of outliers by running four specifications, each of which removes the top/bottom 1%, 2%, 5% and 10% in selling price respectively. Across all four specifications, the coefficient size is quite stable, at between 0.4%

and 0.5%. Nonetheless, the p-values are generally higher than the baseline: 0.062 (for the 1% outlier drop), 0.136 (for the 2% outlier drop), 0.082 (for the 5% outlier drop) and 0.024 (for the 10% outlier drop). The treatment effect remains statistically significant at conventional thresholds, however, when, in addition to the removal of these outliers, one- and two-bedroom properties are also removed from the sample.

- Finally, we find that the treatment effect is largely robust to the exclusion of Dublin, although the effect declines from 0.7 PPs to 0.6 PPs, with the associated p-value rising just above 0.1. (Conventional levels of statistical significance are reached, however, if either the trial is shortened to six months or if smaller properties are excluded, as described above.)

For the time-to-sell model, similar robustness checks are applied (Appendix Table A2). Unlike with the transaction price checks, the treatment effect is not robust to the exclusion of smaller properties, but otherwise the results are similar, including the importance of the Dublin market in estimating the effect, and, on balance, the robustness of the results to the exclusion of the outliers at either end of the price distribution.

As a further robustness check, Table 5 explores the variation in our key variables of interest (the energy efficiency premium, the difference between experimental groups, and how this difference changes through time) in the three years prior to our experiment (2014-2016). In this regard, we explore how the energy efficiency premium changed historically year-on-year, a duration which is consistent with our trial length. Results show that the pre-experiment energy efficiency premium is almost identical to the trial period. Furthermore, the underlying control-treatment gap is also evident and almost identical in magnitude. Importantly, the three-way efficiency-treatment-year interactions are not statistically significant. This demonstrates that the control-treatment efficiency premium gap was stable prior to the trial, alleviating any concern that the markets randomly chosen for treatment had been in some way subject to different pressures in relation to the energy efficiency premium.

TABLE 5: OLS MODEL RESULTS – PRE-TRIAL DATA (2014-2016)

	No Interactions	Treatment Interaction	Treatment and Time Interactions
Efficiency	0.029	0.042	0.042
	(0.002)	(0.002)	(0.004)
Efficiency * Treatment		-0.018	-0.019
		(0.003)	(0.004)
Efficiency * Treatment * Year (2015)			0.001
			(0.004)
Efficiency * Treatment * Year (2016)			-0.000
			(0.004)
Area Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Number of Bedrooms	Yes	Yes	Yes
Number of Bathrooms	Yes	Yes	Yes
Dwelling Type Controls	Yes	Yes	Yes
Model Stats:			
N	38,022	38,022	38,022
Adjusted R-squared	0.566	0.569	0.569
F statistic	151.661	158.119	152.317

Notes: Standard errors (in parenthesis) clustered by area controls. * indicates interaction. Remaining two-way interactions between efficiency, treatment and year are included in all regressions but none are statistically significant and excluded. Dwelling type dummies include apartment, bungalow, detached house, duplex house, end-of-terrace house, semi-detached house, terraced house and townhouse. Time fixed effects are month dummies interacted with a dummy for Dublin and surrounding counties (to allow for separate price trends)
Source: own calculations using daft.ie data and PPR data

V. Conclusion

Achieving carbon neutrality in the coming decades will require significant changes in behaviour and technological investment by the private sector. This is particularly relevant for buildings, which account for 40% of energy consumption. A debate exists in the research literature on whether an energy efficiency gap exist – that is, whether the current technological equilibrium embodies many missed profitable energy efficiency investments.

Our trial results suggest that house buyers are missing an important piece of information during the investment decision – the future energy saving implications – and that providing such information increases the demand for energy efficiency. We also document a time-to-sell effect for the first time in the literature: more efficient properties sell faster in general and treatment significantly increases the speed of sale. The overall effects are in most cases robust to a number alternative specifications. Most notable of these is the non-significance of treatment when analyzing list prices instead of transaction prices. This implies that treatment effects are driven by demand only; that they occurred after the initial advertisement.

The magnitude of the treatment effect is large – the relative change in treatment counties versus control counties is almost 0.7 percentage points which is a relative rise in the energy efficiency premium of approximately 40% in treated counties. Whether our labelling brought households closer to economic rationality is unknown and depends on the researcher's assumptions regarding what is "optimal": within the theoretical framework, many costs and benefits of energy efficiency are unobservable and the classification of economic rationality becomes significantly more blurred. Within this theoretical model, our framing experiment would have changed the elasticity of demand with respect to just one benefit of an energy efficiency upgrade – energy savings – and any appraisal of rationality must also account for the many non-price and unobservable costs and benefits. For example, given the significant rise in climate change awareness and concern in recent years (Ballew et al. 2019, European Commission 2019), it is possible that other factors, such as the 'warm glow' resulting from reducing household carbon impacts on future generations, will likely increase in importance. In short, there is still much we do not know about the relative importance of these factors – how they are changing, and how they interact with one another.

There are two possible explanations for this change in demand and the results could reflect some combination of both: either the cost savings associated with improved energy efficiency are higher than adopters expected, or it is possible that a more familiar metric (money) increased the salience of energy consumption and switched some buyers from inattentive to attentive buyers. Furthermore, while not tested experimentally, it is also possible that the timeframe of our energy forecast (one year) was important, with previous studies showing that framing energy costs over longer durations increases the willingness-to-pay for energy efficiency (Heinzle 2012).

The evidence that energy cost labelling increased the demand for energy efficiency has implications for existing labelling policy. This result is particularly important as it relates to property, a household's largest energy consumer. There are other benefits to monetary labelling more generally: if applied across all household appliances and technologies, households may be better equipped to identify which technologies consume the most, and could therefore focus their energy/money-conservation efforts where savings are highest.

A switch in the labelling metric from kWh/CO2 to annual or even ten-year costs would represent a major philosophical departure from existing informational policy in the EU, which is currently more in line with environmental and societal motivators of upgrading energy efficiency (in particular mitigating climate change). However, a final word of warning: over longer time periods, and, in particular, when climate change damages become more visible to technology adopters, the demand for energy efficiency in the current informational setting (with a stronger carbon component) would likely increase. However, in the short-run, cost labelling is a solution for increasing the demand for energy efficiency and could be presented in conjunction with existing labels.

APPENDIX A

TABLE A1: OLS MODEL RESULTS – SELLING PRICE ROBUSTNESS CHECKS

	Baseline	6-Month Trial	Exclude Small	Percentile drop - 1%	Percentile drop - 2%	Percentile drop - 5%	Percentile drop - 10%	Exclude Dublin	Exclude GDA
Efficiency	0.040	0.040	0.036	0.037	0.035	0.028	0.023	0.040	0.040
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
Efficiency * Trial	-0.005	-0.006	-0.003	-0.003	-0.003	-0.002	-0.003	-0.005	-0.005
	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Efficiency * Treatment	-0.023	-0.023	-0.024	-0.021	-0.019	-0.014	-0.012	-0.009	-0.004
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)	(0.004)	(0.005)
Trial * Treatment	-0.064	-0.071	-0.083	-0.038	-0.030	-0.039	-0.046	-0.060	-0.061
	(0.024)	(0.025)	(0.028)	(0.021)	(0.020)	(0.019)	(0.017)	(0.029)	(0.038)
Efficiency * Trial * Treatment	0.007	0.008	0.009	0.005	0.004	0.004	0.005	0.006	0.006
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.005)
Area Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Bedrooms	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Bathrooms	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dwelling Type Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Model Stats:									
N	31822	31822	19676	23642	25614	31245	30620	28724	25654
Adjusted R-squared	0.521	0.525	0.393	0.554	0.555	0.539	0.500	0.504	0.478
F statistic	147.646	141.370	102.549	169.558	166.332	174.693	140.79 4	115.344	121.945
Prob. > F	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: Standard errors (in parenthesis) clustered by area controls. * indicates interaction. Remaining two-way interactions between efficiency, treatment and year are included in all regressions but none are statistically significant and excluded. Dwelling type dummies include apartment, bungalow, detached house, duplex house, end-of-terrace house, semi-detached house, terraced house and townhouse. Time fixed effects are month dummies interacted with a dummy for Dublin and surrounding counties (to allow for separate price trends)

Source: own calculations using daft.ie data and PPR data

TABLE A2: OLS MODEL RESULTS – TIME-TO-SELL ROBUSTNESS CHECKS

	Baseline	6-Month Trial	Exclude Small	Percentile drop - 1%	Percentile drop - 2%	Percentile drop - 5%	Percentile drop - 10%	Exclude Dublin	Exclude GDA
Efficiency	-1.914	-1.435	-1.840	-1.895	-1.889	-2.288	-2.652	-2.016	-2.054
	(0.504)	(0.544)	(0.515)	(0.495)	(0.503)	(0.498)	(0.545)	(0.510)	(0.513)
Efficiency * Trial	0.942	0.000	0.546	0.770	0.722	1.068	1.151	0.954	0.943
	(0.644)	(0.688)	(0.701)	(0.653)	(0.676)	(0.678)	(0.740)	(0.643)	(0.645)
Efficiency * Treatment	0.781	0.383	0.652	0.691	0.620	0.979	1.379	0.728	0.350
	(0.579)	(0.641)	(0.589)	(0.571)	(0.577)	(0.587)	(0.632)	(0.690)	(0.830)
Trial * Treatment	8.725	3.514	6.592	7.043	6.822	8.413	8.103	5.075	0.612
	(5.636)	(5.886)	(6.395)	(5.691)	(5.819)	(5.861)	(6.348)	(6.656)	(7.581)
Efficiency * Trial * Treatment	-1.521	-0.869	-1.446	-1.268	-1.220	-1.520	-1.729	-0.947	-0.243
	(0.736)	(0.800)	(0.818)	(0.745)	(0.765)	(0.773)	(0.833)	(0.885)	(1.020)
Area Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Bedrooms	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Bathrooms	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dwelling Type Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Model Stats:									
N	31822	23642	25614	31245	30620	28724	25654	19676	15670
Adjusted R-squared	0.058	0.061	0.025	0.057	0.056	0.055	0.055	0.058	0.061
F statistic	55.148	41.607	11.937	54.639	53.882	50.652	48.561	40.023	41.030
Prob. > F	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: Standard errors (in parenthesis) clustered by area controls. * indicates interaction. Remaining two-way interactions between efficiency, treatment and year are included in all regressions but none are statistically significant and excluded. Dwelling type dummies include apartment, bungalow, detached house, duplex house, end-of-terrace house, semi-detached house, terraced house and townhouse. Time fixed effects are month dummies interacted with a dummy for Dublin and surrounding counties (to allow for separate price trends)

Source: own calculations using daft.ie data and PPR data

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