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

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ABSTRACT

With buildings accounting for roughly 40 % of energy consumption in the US and Europe, energy efficiency upgrades will be central in meeting climate targets. Using a nationwide controlled field experiment, we find that the inclusion of property-specific energy cost labels within property advertisements increases energy efficiency premiums. We also show that more energy efficient properties sell faster and, for the first time, that energy cost labels shortened time-to-sell. While a major departure from existing property labelling policy, these results suggest that framing property energy efficiency according to their cost implications, rather than in energy units, increases the demand for energy efficiency.

1. Introduction

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. Achieving this ambitious target will require extensive technological investment in parallel with behavioural change, however, it is not guaranteed that these will move in tandem, since, 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' or the energy efficiency gap (Jaffe and Stavins, 1994b).

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 et al., 2018). Such an 'energy performance gap' (de Wilde, 2014) may be the result of 'prebound effects' (bias in forecasted energy savings) (Sunikka-Blank and Galvin, 2012) and/or rebound effects (an increase in energy service demand post installation) (Greening et al., 2000).

Energy efficiency labels – a cornerstone of environmental policy throughout the world – are motivated by the expectation that households are poorly informed about consumption differences across products resulting in under-investment and an energy efficiency gap. There is a considerable body of literature exploring the structural, behavioural and informational factors influencing the energy efficiency gap and estimating the magnitude of the gap, such as (Allcott and Greenstone, 2012; Gerarden et al., 2015; Gerarden et al., 2017; Houde and Wekhof, 2021; Andor and Fels, 2018). In the EU, the *imperfect information* rationale is explicitly included in labelling legislation, for example "… the provision of accurate, relevant and comparable information on the specific fuel consumption and CO_2 emissions of passenger cars may influence consumer choice in favor of those cars which use less fuel and thereby emit less CO_2" (EU Car Labelling Directive 1999/94/EC). 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 meter squared per year, for example).

However, even when clear and accurate comparative energy consumption information is available to adopters, there may be a

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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 based on size, kWh and energy prices over the lifetime of an investment are problematic for many households (Allcott, 2011; Heinzle, 2012). This situation is likely exacerbated by low levels of energy literacy (Turrentine and Kurani, 2007; Allcott, 2011; Heinzle, 2012; Brounen et al., 2013; Sovacool and Blyth, 2015; Levine et al., 2018).

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 gap/problem reduces the demand for more energy efficient properties. We test this using a nationwide year-long controlled field experiment, which provides information about energy costs in monetary terms in the context of residential property decisions, covering all property sales in Ireland. Specifically, 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 color-coded scale similar to existing labels in the EU and was automatically generated within the online platform when the property was first advertised. Although a limitation of our research design, given geographical constraints the Dublin region and surrounding commuter counties (Kildare, Meath and Wicklow) were grouped together and included in the treatment group, which was necessary to reflect property search patterns and commuting behaviour and thereby avoid treatment contagion.

While a large body of empirical evidence shows that households value property energy efficiency within labelled settings (Brounen and Kok, 2011; Fuerst et al., 2015; Fuerst et al., 2016; Jensen et al., 2016; Chegut et al., 2016; Cajias and Piazolo, 2013; Mudgal et al., 2013; Hyland et al., 2013; Stanley et al., 2016; Costa and Kahn, 2013; Frondel et al., 2020), to the best of our knowledge, this is the first paper to test the monetary framing of energy efficiency 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 energy uses, such as appliances and cars, the variation in property energy costs is significantly wider due to higher heterogeneity in energy efficiency and property size (Brounen et al., 2012; Eurostat, 2022).

Results show a large and significant relative increase in the energy efficiency premium in treatment counties of approximately 0.9 percentage points for each unit increase in the 15-point efficiency scale. This effect is seen in transaction prices only – listed (advertised) prices were unaffected, implying that the result is driven by changes in demand post-advertisement. In addition, we also show that (again, for the first time) treatment increased the speed at which more energy efficient properties sold, another valid proxy of increased demand.

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, using stated choice experiments, 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 et al. (2017) find similar effects for lightbulbs, in stated choice experiments, while Andor et al. (2017) shows that EU labels combined with annual operating cost information increases the demand for more energy efficient refrigerators, also in a stated choice setting.

Given the potential hypothetical bias in stated choice settings, revealed preferences, as evidenced through field experiments, have the potential to provide further robustness on the effectiveness of monetary energy labelling. For example, Boogen et al. (2022) show using a field experiment that many households are under-informed, or do not consider, energy costs in purchasing decisions for household appliances and lightbulbs. Many of the studies on closing the monetary information gap undertaken in field trial settings relate to appliance purchasing, for example, field studies on monetised energy costs labelling for large white goods such as refrigerators and tumble-dryers have been carried out in Norway (Kallbekken et al., 2013), the UK (DECC, 2014), Ireland (Carroll et al., 2016; Denny, 2022), Spain (del Mar Solà et al., 2021) and on lightbulb purchasing in the US (Allcott and Taubinsky, 2015), and Germany (Andor et al., 2019b). It is interesting to note that the results from these studies are mixed and it is not conclusively clear that monetary labelling for appliances increases willingness to pay for energy efficiency, some papers finding a positive effect, while others finding no effect. Thus, it is likely that contextual factors play an important role with differences in energy pricing and potential savings, labelling type and context differing across countries (Ceolotto and Denny, 2021). While vehicle labelling is examined in Allcott and Knittel (2019), field trials for larger products, beyond appliances and lightbulbs, are rare. Thus, a key contribution of this paper is that it is the first paper to test energy cost labelling in a field experiment for the property sector.

The duration of labelled 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 this type of duration effect, if present.

The rest of the paper is structured as follows. The next section presents the theoretical model used to describe the energy upgrade decision. Section III then describes the treatment and experimental design. Section IV and Section V present the datasets and results, respectively. Section VI discusses how these results impact this body of research and the policy sphere.

2. Energy efficiency gap framework and research questions

We build upon the model proposed by Allcott and Greenstone (2012) to explain the 'energy efficiency gap', that is, the apparent failure of private agents to adopt technologies with positive returns. Within this framework, the financial trade-offs which underpin the general discourse on the size (or existence) of the energy efficiency gap are presented alongside a range of unobservable but likely influential costs and benefits. Our goal is to illustrate that apparent deviations from financial optimisation may mask a number of missing variables, and that while financial benefits are clearly important, they should be placed within the context of this broad, multifaceted decision. We believe this argument is particularly pertinent within the context of property investment decisions. For example, when an agent is comparing potential properties to purchase, there may be missing variables and biases relating to each property's energy efficiency that may result in the undervaluation of more energy efficient properties.

Failure to upgrade property energy efficiency when the lifetime benefits (energy savings and property value appreciation) exceed the initial investment costs is generally known as the 'energy paradox' (Jaffe and Stavins, 1994b) resulting in an 'energy efficiency gap' (Jaffe and Stavins, 1994a). In the context of this paper's property purchase setting, if a buyer was comparing multiple properties with similar non-energy attributes, but failed to invest in a more efficient property, even when the energy savings of this property outweighed its price premium, this would contribute to the energy efficiency gap.

It is important to consider that aside from the monetary energy savings benefits, the household decision is likely influenced by a

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[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.

numerous additional costs and benefits, most of which are unobservable. For example, there are likely transaction costs associated with the time and hassle of researching and estimating the relative energy savings benefits of each property, and adoption costs associated with changing household routines according to new installed technologies (new controls, for example) in the more efficient property.[2] There are also unobserved benefits associated with a more efficient property, such as potential improved health effects (Hamilton et al., 2015), convenience and comfort (Coyne et al., 2018) associated with, for example, more accurate and automated heating controls. In addition, given the positive externalities and intergenerational altruistic components associated with household emission reductions, there are likely altruistic or 'warm-glow' effects associated with investment in a more efficient property (Andreoni, 1990; Frederiks et al., 2015).

There are also a number of market failures which could explain an under-investment in more energy efficient properties. For example, imperfect information (or biased expectations) regarding energy savings, the lifetime of the installed energy efficiency technologies or the energy efficiency sales premium would clearly bias expectations of adoption benefits. Such knowledge gaps seem likely given that many households are unfamiliar with the energy sector, energy prices and the commonly employed units (kilowatt hours) of energy (Sovacool and Blyth, 2015; Brounen et al., 2013).

Downwardly biased energy price expectations would also reduce the benefits of adopting the more efficient property, both through the energy savings channel and the property value channel (assuming future energy savings 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 (or carbon taxes). Similarly, dwellings located further away from urban centres (with higher commuting costs) would be disproportionally impacted.

There might also be numerous behavioural biases at play. For example, irrationally high discount rates (Frederick et al., 2002) or short investment horizons clearly reduce the discounted benefits (observed and unobserved) of adoption of the more efficient property. In particular, the role of present bias, systematically overvaluing the present relative to the future, in relation to energy efficiency adoption has been found to result in underinvestment in residential energy improvements and thermostat temperature (Bradford et al., 2017; Schleich et al., 2019).

Status quo bias, where individuals have a preference for repeating previous choices, has also been shown to represent a potential barrier to increasing energy efficiency and thereby causing under-adoption, for example, when replacing a boiler, status quo bias would suggest a household would replace like with like, rather than invest in a new technology such as a heat pump (Blasch and Daminato, 2020). 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). The complexity associated with computing potential net benefits from investing in a more energy efficient property could suggest that limitations in cognitive skills and cognitive reflection may also play a role with consumers resorting to simple heuristics to simplify investment decisions (Gerarden et al., 2017; Andor et al., 2019a).

2.1. Research question and potential mechanisms

Our main hypothesis relates to imperfect information or bias regarding the specific monetary savings associated with more energy efficient properties. There are many reasons to believe that energy benefits are either missing from or biased in investment decisions. For example, surveys exploring household vehicle decisions (Turrentine and Kurani, 2007; Allcott, 2011) show that many households are simply inattentive to energy efficiency at the point-of-sale. The property, like the car, is a multi-attribute 'product', and prospective adopters may reduce the range of attributes to a smaller consideration set (Shocker et al., 1991), which may or may not include 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 et al., 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 suggesting an undervaluing of energy savings). The complexity of such calculations becomes even more challenging for investments with longer lifetimes, such as property.

In this paper, we explore this *specific energy saving information problem* by adding property-specific energy cost forecasts to advertisements on Ireland's largest property website, daft.ie and consider if this affects the demand for energy efficiency. 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 more efficient properties resulting in a higher energy efficiency premium. Thus, research question 1 considers:

$$H_0 : EE_M - EE_P = 0.$$

$$H_1 : EE_M - EE_P > 0$$

Where EE_M is the energy efficiency premium where property energy consumption is represented in monetary terms (€), and EE_P is the energy efficiency premium where property energy consumption is represented in physical terms (kWh). In other words, the alternative hypothesis is that monetary energy cost labelling increases the energy efficiency premium.

The second research question explores if adding property-specific energy cost forecasts to advertisements results in higher demand for more efficient properties through faster average time-to-sell.

$$H_0 : T_M - T_P = 0$$

$$H_1 : T_M - T_P < 0$$

Where T_M is the average time-to-sell when property energy consumption is represented in monetary terms (€), and T_P is the average time-to-sell where property energy consumption is represented in physical terms (kWh). In other words, the alternative hypothesis is that monetary energy cost labelling results in more energy efficient properties selling quicker, so the time-to-sell is reduced compared to the situation where energy costs are represented in physical units.

3. Research setting and experimental design

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

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[2] Allcott and Greenstone also include "net costs" in their equation.

BER is a property's kWh/m^2/year, which is displayed on a 15-grade color-coded scale (Panel A of Fig. 1). The rating is calculated by an independent and registered assessor using a standardized software application which considers all major components of the property including dimensions, orientation, insulation, and space and hot water system efficiencies. Ratings are based entirely on these technical aspects and do not account for any behavioural factors, such as rebound effects or the building's historical consumption data. Advertisement regulations stipulate that a property's individual BER grade is required only (without the full color-coded comparative scale) for all sale and rental advertisements (Panel B of Fig. 1).

The new property-specific energy cost label (Panel A of Fig. 2) 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 Fig. A6). We provide an example of the energy cost calculations in Table 1.

Treatment was randomly assigned across the 26 counties of Ireland. This small number of experimental units was necessary as prospective buyers generally search within a particular county, reflecting traditional preferences and reinforced by options for searching on the portal – any further disaggregation would have increased the probability of treatment contamination, that is, buyers learning about energy costs from a treatment area and applying this new knowledge to a control area. Only one adjustment is made to this strategy: Dublin county and city (the capital) were combined with neighboring counties Meath, Kildare and Wicklow. This county aggregation was necessary as Dublin, like other capital cities, has a high share of workers who commute from neighboring counties due to price pressures and such commuters are more likely to search across this wider geographic area. The final county allocation is displayed in Table 2. County shares generally range between 1 % and 6 % with two main exceptions – Cork (10 %) and Dublin (31 %).

4. 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. Given that our new energy cost label was automatically generated within the website (and not observed by the agent), we would mainly expect to observe demand-led changes in the transaction (final closing) price. Over time, however, it is possible that feedback loops (agents learning and harnessing this new information) would affect list prices.

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 standardization 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).[3] 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'). In addition, 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.[4]

Table 3 displays descriptive statistics for the full sample (pre-merge) and the final sample (post-merge) for analysis. A number of factors result in a reduction in the final sample size which stem from 1) differences in addresses (format, spelling and order) between daft.ie (added by the estate agent) and the PPR (added by the solicitor), 2) unsold properties in the daft.ie dataset (and therefore no corresponding record in the PPR), and 3) delays between the date of sale and the PPR registration date. These lead to a 64 % reduction in the sample size of the control group (and 7.5 % decrease in mean price), and 57 % reduction in the treatment group size (and 0.3 % decrease in price).

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 price 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).

We estimate the treatment effects using the following hedonic difference-in-differences regression:

$$\begin{aligned} log(Y) = & \beta_0 + \beta_1 E + \beta_2 P + \beta_3 T + \beta_4 (E^*P) + \beta_5 (E^*T) + \beta_6 (P^*T) \\ & + \beta_7 (E^*P^*T) + \boldsymbol{\beta}_C' \boldsymbol{C} + \varepsilon \end{aligned} \tag{3}$$

where Y is property price, E is energy efficiency, P is the trial period dummy, T is the treatment county dummy and C is a vector containing non-efficiency property attributes and area fixed effects. 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_7 (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 2018 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.

5. 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 Fig. 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

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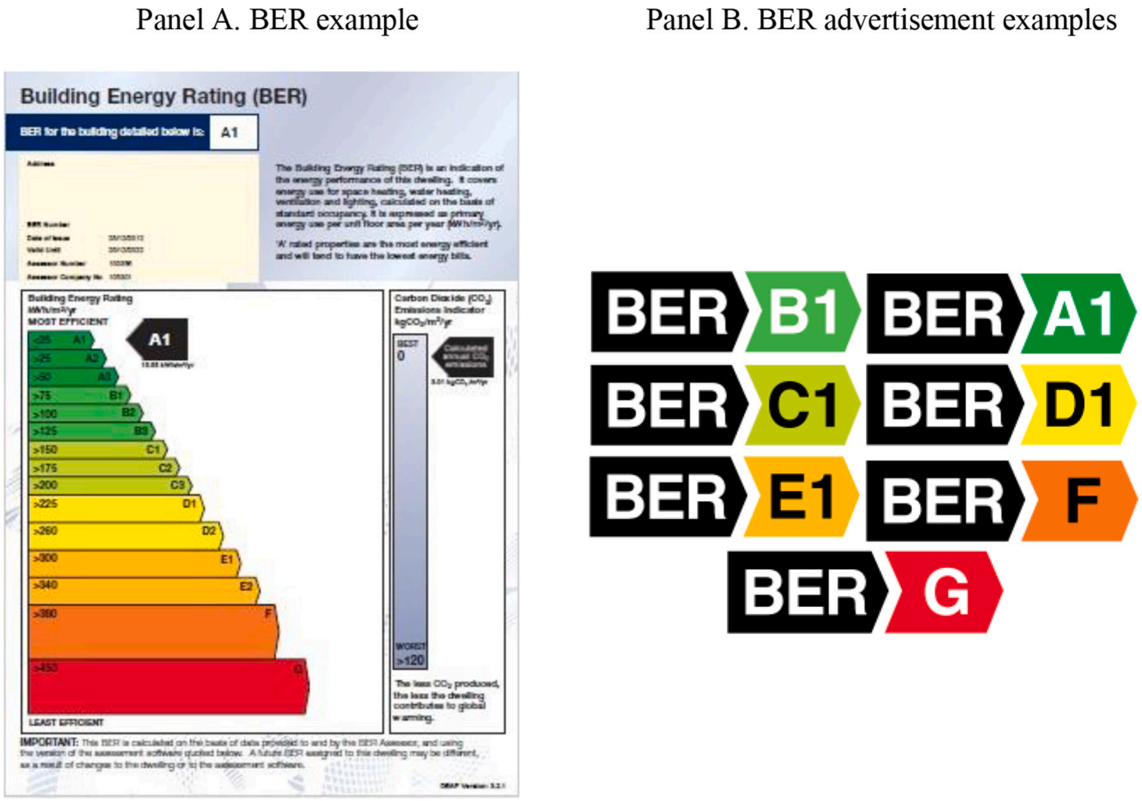
[3] A "fuzzy" merge was carried out in STATA 14 using the "reclink" command.

[4] Furthermore, for duplicate addresses with different dates, only the latest entry was kept. This choice (compared to dropping the later listing instead) was motivated by an expectation that, if a listing was subsequently updated, it was likely done to remove an error in the initial advertisement. In addition, in the case of a property selling twice (or more times), focusing on the latest listing (or equally, the earliest) increases the probability that the correct match with official sales price database occurs.


Panel A. BER example

Panel B. BER advertisement examples

Fig. 1. The Building Energy Rating (BER).
Source: Sustainable Energy Authority of Ireland.

townhouse), market conditions (dummy variables for each month) and location (1457 separate "micro-markets" using the website's area codes). We also interact the time variables with a dummy for Dublin and surrounding counties as these counties have historically displayed different price growth dynamics. Standard errors are clustered by the micro-market location variable (1457 clusters).

The estimated energy efficiency premium from this regression (β_1) may be biased in the absence of property attributes which are likely correlated with energy efficiency, such as internal property condition and age (discussed further below). 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) interaction effect (β_7) – the relative increase in the energy efficiency premium from switching to monetary information – is causal due to randomization (any limitations to which are discussed in the results). As per Nizalova and Murtazashvili (2016) this comes from the fact that treatment is assigned independently of both efficiency (BER) and potential other unobserved property characteristics (such as condition or age of the property).

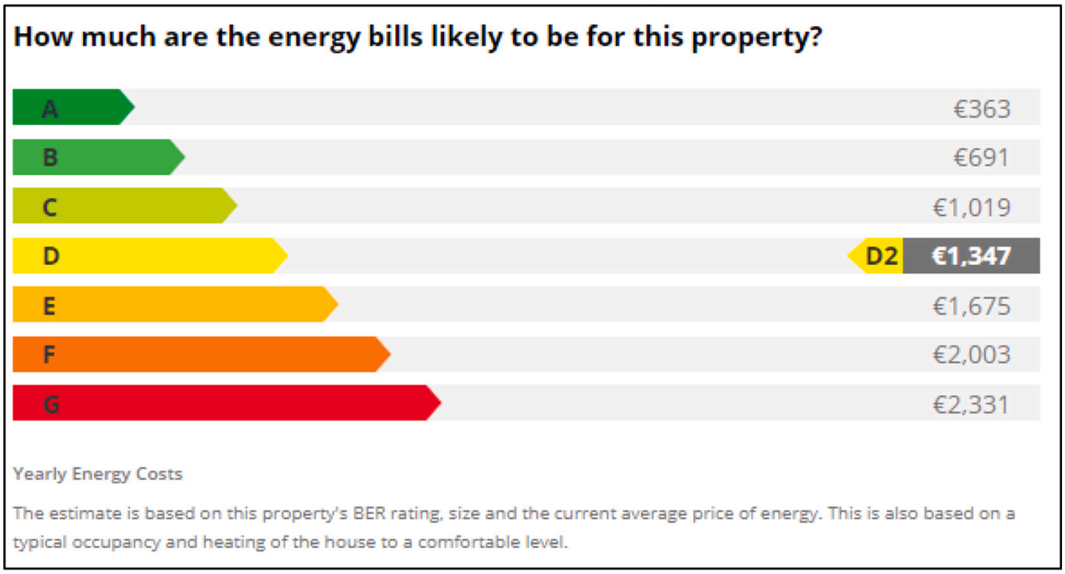
A large and statistically significant efficiency premium is evident from the results. For the control group pre-trial, each categorical increase on the 15-point BER scale is associated with a 4 % higher transaction price. During the trial period, this energy efficiency premium declined by 0.6 percentage 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 less than half that of the control group in the same period (1.6 %); a gap which is driven by the considerably higher property prices in the treatment group. However, the three-way efficiency-trial-treatment coefficient is positive and significant and shows that the energy efficiency premium increases by 0.9 PPs more during the trial period in treated counties (p-value from one sided test = 0.001) relative to control group counties. In other words, we observe a higher energy efficiency premium in treated counties (equating to approximately half of pre-trial treatment group premium) when energy efficiency was framed in monetary terms rather than in energy units (kWh).

The second model in Table 5 shows the time-to-sell results (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 approximately 1.8 days. In the treatment group, this effect increases to 3.3 days (1.8 days plus 1.5 days – p-value from one sided test = 0.019).

For list prices (final column in Table 4), treatment effects are no longer statistically significant. Such stark differences between listed and transacted prices are consistent with the modified label being a demand-led intervention targeted at buyers and 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 Table A1 (Appendix). 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 (we would not expect the treatment effect to decline due to fatigue as buyers do not buy several properties). This is done by estimating the model for the first six months of trial data, rather than the full twelve. We do not find evidence that the treatment effect declined over time: the effect is 0.9 percentage points (PPs) after six months and 0.9 PPs after twelve (no statistically significant difference is observed). Secondly, theory suggests that the negative effects of energy inefficiency increase with size, with larger spaces requiring proportionately more energy to heat. Therefore, we explore whether the effect is greater for larger dwellings (3–5 bedroom properties). We find that the exclusion of

Panel A. Treatment Group Label



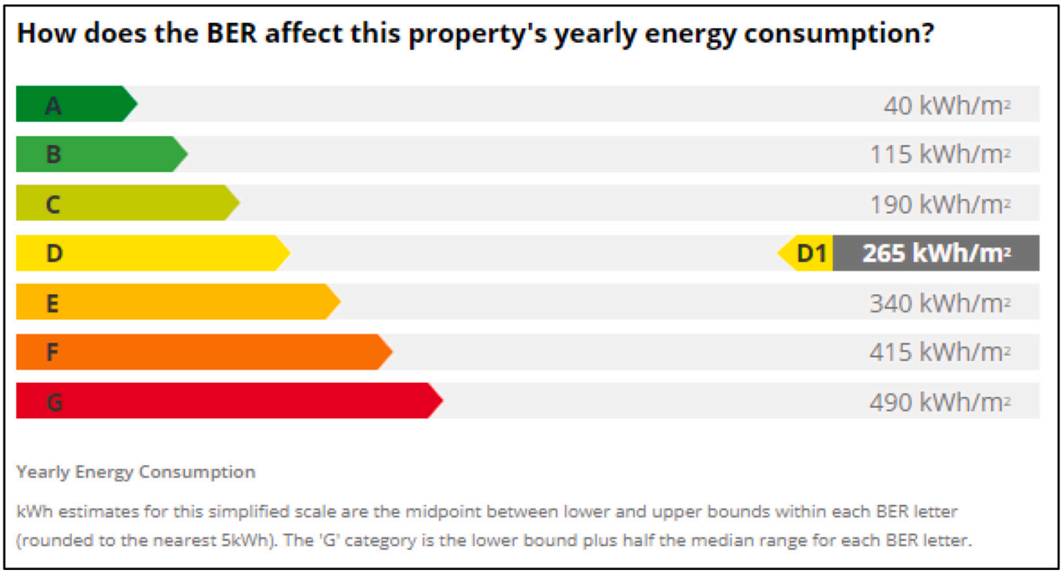
How much are the energy bills likely to be for this property?

A	€363
B	€691
C	€1,019
D	D2 €1,347
E	€1,675
F	€2,003
G	€2,331

Yearly Energy Costs

The estimate is based on this property's BER rating, size and the current average price of energy. This is also based on a typical occupancy and heating of the house to a comfortable level.

Panel B. Control Group Label



How does the BER affect this property's yearly energy consumption?

A	40 kWh/m2
B	115 kWh/m2
C	190 kWh/m2
D	D1 265 kWh/m2
E	340 kWh/m2
F	415 kWh/m2
G	490 kWh/m2

Yearly Energy Consumption

kWh estimates for this simplified scale are the midpoint between lower and upper bounds within each BER letter (rounded to the nearest 5kWh). The 'G' category is the lower bound plus half the median range for each BER letter.

Fig. 2. RCT Label Examples.
Source: Designed by the authors and daft.ie.

one- and two-bedroom properties increases the treatment effect slightly to 1.0 PPs (again however, this is difference is not statistically significant). Thirdly, we explore the impact of outliers by running four specifications, each of which removes the top/bottom 1 %, 2 %, 5 % and 10 % in transaction prices respectively. Across all four specifications, the coefficient size falls but is quite stable, at between 0.4 % and 0.7 %, and the *p*-values are generally higher than the baseline: 0.004 (for the 1 % outlier drop), 0.013 (for the 2 % outlier drop), 0.057 (for the 5 % outlier drop) and 0.005 (for the 10 % outlier drop).[5]

Finally, we find that the treatment effect is broadly robust to the exclusion of Dublin, although the effect declines from 0.9 PPs to 0.8 PPs, with the associated p-value being approximately 0.01. Statistical significance declines further with the exclusion of Dublin's surrounding counties (associated p-value of 0.06). However, these declines in precision are likely part-driven by sample size which declines by 38 % with the exclusion of Dublin and by 51 % with the additional exclusion of these surrounding counties. In the case of both excluding the Dublin region and shortening the analysis to 6 months we find that the effect size is still stable at 0.9 PPs with an associated one-sided p-value of 0.017. It is also similar when only excluding Dublin and small properties (effect size 0.9 PPs, one-sided p-value 0.016). When dropping Dublin, commuter counties and in a six-month trial the effect is 0.9 PPs with an associated p-value of 0.056, while dropping Dublin, commuter counties and small properties reduces the effect size to 0.8 PPs and increases the associated p-value to 0.082. In this instance however we would be excluding 20,501 observations or 59 % of the entire sample (80 % from the treatment group and 18 % from the control group). Given that the exclusion of Dublin places almost all remaining cities in the control group however this may not be an appropriate comparison.

For the time-to-sell model, similar robustness checks are applied (Appendix Table A2). Unlike with the transaction price checks, the

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[5] 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.

Table 1
Example of energy cost calculation for the treatment group in the RCT.

	Values	Code	Formula
BER (kWh/m^2/yr)	350	a	
Size (m^2)	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/m^2/yr)	20	f	
Delivered energy for lights and pumps (kWh/m^2/yr)	8	g	
Cost of lights and pumps (€/m^2)	€1.59	h	g * c
Cost of heating (€/m^2)	€20.79	i	(a - f) * ((d + e)/ 2)
Total annual energy cost	€2238.36	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.

Table 2
Control and treatment county allocation – pre-trial county shares.

Control	N	%	Treatment	N	%
Cork	3213	10.29 %	Carlow	399	1.28 %
Galway	1713	5.49 %	Cavan	358	1.15 %
Kerry	851	2.73 %	Clare	789	2.53 %
Kilkenny	518	1.66 %	Donegal	629	2.01 %
Laois	419	1.34 %	Dublin	9567	30.64 %
Leitrim	287	0.92 %	Kildare	1675	5.37 %
Limerick	1165	3.73 %	Louth	1010	3.24 %
Longford	306	0.98 %	Mayo	838	2.68 %
Roscommon	400	1.28 %	Meath	1285	4.12 %
Tipperary	898	2.88 %	Monaghan	182	0.58 %
Westmeath	690	2.21 %	Offaly	298	0.95 %
Wexford	1262	4.04 %	Sligo	479	1.53 %
			Waterford	1127	3.61 %
			Wicklow	862	2.76 %
	11,722	37.55 %		19,498	62.45 %

Notes: data are from 2017 (pre-trial) and include sales advertisements only.
Source: own calculations based on daft.ie dataset.

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 outliers at either end of the price distribution.

5.1. Parallel trends

Table 5 explores the effects of some of the experimental limitations of our study (small number of experimental units and the part-manual treatment allocation) on the key variable of interest – the energy efficiency premium – using data from the four years prior to the trial (statistical modelling and data manipulations identical to that employed in the core analysis outlined previously). Specifically, we explored the annual variation (same duration as the trial) in energy efficiency premiums across our experimental groups. While the overall energy efficiency premium is stable over time (efficiency by year interactions), it is consistently lower in treatment counties (efficiency by treatment interaction). However, and importantly, the energy efficiency premium gap between experimental (three-way interaction terms) is stable on a year-to-year basis. Thus, while there are differences across groups, such differences appear to be very stable before the trial introduction.

In addition, we present the energy efficiency price premiums for monetary information in an augmented regression including all available data in Fig. 3, which illustrates the coefficient on the three-way interaction term (Efficiency * Treatment * Year) over time dating back to 2013 with a base year set as 2017 (the year prior to the trial introduction). We do not observe a significant effect of treatment in any of the

Table 3
Descriptive statistics for regression sample.

Full Sample (pre-merge):	Control Group *N* = 32,222		Treatment Group *N* = 53,065	
	Mean	Standard Deviation	Mean	Standard Deviation
List 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 %
Final Sample (post-merge):	**Control Group *N* = 11,630**		**Treatment Group *N* = 22,925**	
	Mean	Standard Deviation	Mean	Standard Deviation
List Price (€)	213,144	118,055	329,216	207,033
Transaction Price (€)	212,742	117,481	337,473	217,202
Bedrooms (#)	3.3	0.9	3.0	1.0
Bathrooms (#)	2.1	0.9	1.9	0.9
Apartment (%)	10.3 %	30.5 %	20.2 %	40.2 %
BER: A (%)	0.5 %	7.2 %	1.1 %	10.5 %
BER: B (%)	9.5 %	29.3 %	9.1 %	28.7 %
BER: C (%)	43.4 %	49.6 %	38.4 %	48.6 %
BER: D (%)	24.5 %	43.0 %	25.6 %	43.6 %
BER: E (%)	10.0 %	30.0 %	13.2 %	33.9 %
BER: F (%)	4.8 %	21.4 %	6.6 %	24.9 %
BER: G (%)	7.3 %	26.0 %	6.0 %	23.8 %

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

Table 4
Difference-in-differences results from OLS Hedonic regression.

	Transaction Price	Time-to-Sell	List price
Efficiency	0.040	−1.844	0.043
	(0.003)	(0.521)	(0.003)
Efficiency * Trial	−0.006	0.379	−0.004
	(0.003)	(0.651)	(0.002)
Efficiency * Treatment	−0.024	0.661	−0.023
	(0.003)	(0.597)	(0.003)
Trial * Treatment	−0.077	9.256	−0.021
	(0.023)	(5.713)	(0.023)
Efficiency * Trial * Treatment	0.009	−1.537	0.004
	(0.003)	(0.740)	(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	34,539	34,539	34,539
Adjusted R-squared	0.520	0.024	0.614
F statistic	147.483	15.401	179.670
Prob. > F	0.000	0.000	0.000

Notes: Standard errors (in parenthesis) clustered by 1457 micro-markets. * indicates interaction. *Efficiency* refers to the 15-point BER grade rating of the property, *Trial* is an indicator variable equal to 1 if a property is listed in the trial period (Mar 2018 – Feb 2019), and *Treatment* is an indicator variable equal to 1 if a property is part of the treatment group. 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 5
Difference-in-differences results from OLS Hedonic regression – transaction price pre-trial (2013–2016).

	No Interactions	Treatment Interaction	Treatment and Time Interactions
Efficiency	0.029	0.041	0.038
	(0.001)	(0.002)	(0.003)
Efficiency * Treatment		−0.017	−0.018
		(0.003)	(0.003)
Efficiency * Year (2013)			0.006
			(0.004)
Efficiency * Year (2014)			0.003
			(0.003)
Efficiency * Year (2015)			0.004
			(0.003)
Efficiency * Treatment * Year (2013)			0.004
			(0.005)
Efficiency * Treatment * Year (2014)			−0.000
			(0.004)
Efficiency * Treatment * Year (2015)			−0.000
			(0.003)
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	46,709	46,709	46,709
Adjusted R-squared	0.571	0.573	0.573
F statistic	186.225	194.338	189.955

Notes: Standard errors (in parenthesis) clustered by area controls (1478 clusters). * indicates interaction. *Efficiency* refers to the 15-point BER grade rating of the property, *Year* indicates pseudo treatment year (March to February in subsequent year), and *Treatment* is an indicator variable equal to 1 if a property is part of the treatment group. 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, semi-detached house and terraced house. 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 matched daft.ie data and PPR data.

preceding years on either transaction prices or list prices, nor do we observe any differences in energy efficiency premium between transaction and list prices. Following treatment introduction in 2018, we observe a significant interaction effect on transaction prices only as discussed previously.

In 2013 we observe a comparatively higher premium for energy efficiency in treatment counties (one-sided test p-value equal to 0.037 for transaction prices and 0.028 for list prices). It is important to note that the legislation which requires properties to display a BER when advertised was enacted in early 2013 (European Union (Energy Peformance of Buildings) Regulation, 2012), and therefore the requirement to display a BER when selling a property was still in its infancy. Given that our treatment group includes the capital region, it is likely that initial compliance/rollout of the ratings may have led to higher initial premiums for efficiency. Confidence intervals are also wider in 2013 relative to 2018 since we have fewer observations dating back to this time period.

We illustrate the effect of treatment on time-to-sell in a comparable format in Fig. 4. We do not observe significant differences in the effect of efficiency on time-to-sell between treatment and control in any of the preceding periods. Following treatment introduction in 2018, we observe a decline in time-to-sell of approximately 1.5 days for each BER grade increase in treatment counties (associated p-value from one sided test = 0.023).

As additional evidence for parallel trends, we present graphs of the outcome variables (prices and time-to-sell) and efficiency (BER) disaggregated by treatment and control group in Appendix Figs. A1, A2 and A3. Visually, there do not appear to be any differences in trends of either price levels or efficiency between treatment and control counties. There does however appear to be a convergence in time-to-sell between treatment and control countries which occurs in 2014. Subsequent to this, mean time-to-sell moves in parallel between treatment and control groups, even exhibiting a similar pattern of decline in 2017. Finally, we also present the distribution of BER ratings by treatment and control group, pre and post treatment (Appendix Figs. A4 and A5). We observe an improvement in efficiency in both treatment and control groups (lower proportion of inefficient categories G - D1 and a higher proportion of efficient categories C2 – A3). The pattern of ratings also appears to be relatively stable pre-and post-treatment between treated and control groups.

5.2. *Limitations and future research*

The results indicate that providing energy cost information in relation to residential properties increases the willingness to pay for energy efficiency, however, it is possible that the findings could be different in other jurisdictions and contexts (Ceolotto and Denny, 2021). For example, the benefits of energy efficiency would be lower in jurisdictions with lower energy prices (residential energy prices are relatively high in Ireland) and lower energy demand due to warmer climates. Similarly, alternative jurisdictions are likely to have disparate contexts in terms of demographics, average property size, urban density, ownership rates and feasible energy efficiency interventions, as well as other behavioural differences, such as, political attitudes that may influence baseline willingness to pay for energy efficiency. Thus, it is recommended that future work explore expanding this research in other countries to capture context specific limitations of this work as well as addressing issues relating to sample size and power.

Although this is a nationwide field trial, one of the key limitations of the study is the relatively small size of Ireland causing the experimental design to be limited by the fixed number of counties across which randomization could be done, which limits the possible power achievable in such a study (Ioannidis, 2005). This is evident in Ioannidis et al. (2017), who show that over 90 % of papers they examined are underpowered. While increasing sample size is not possible in the current context, higher precision would be achieved through studying a much larger geographical area or across multiple countries.

An ex-post power analysis was conducted following the methodology in Gelman and Carlin (2014), which involves a “design analysis” to represent the outcome of a hypothetical replication which focuses on estimates and uncertainty rather than statistical power. The Gelman and Carlin (2014) methodology requires an estimation of the true effect size, however, given that this is the first paper to examine monetary energy labelling in the context of sales prices of residential properties, it is not possible to base this on existing literature. Thus, a range of possible true effect sizes are considered. Based on the literature on the impact of monetary labelling for appliances through field trials, those with statistically significant impacts range from 3.16 % (del Mar Solà et al., 2021) to 8 % for lightbulbs (Boogen et al., 2022) to 12 % for tumble-dryers (Kallbekken et al., 2013) to 45 % for lightbulbs (Andor et al., 2019b). It should be noted that these are very different experiments in different settings under different contexts and different jurisdictions (Ceolotto and Denny, 2021).

In the housing market, there are no similar studies on the impact of monetized energy labels on residential property prices; however, research on the impact the introduction of the Energy Performance Certificate (EPC) had on price premiums ranges from a 3.6 % increase in sales price in the Netherlands upon the introduction of the EPC (Brounen

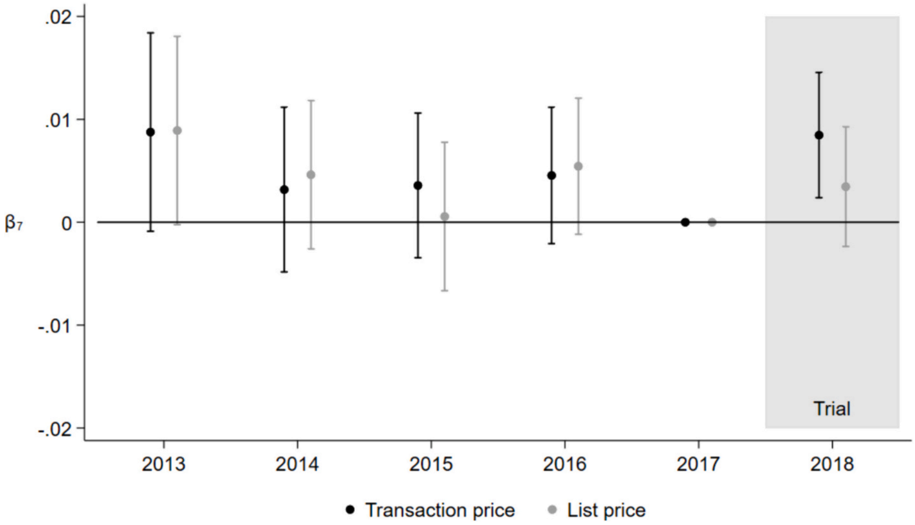

Fig. 3. Parallel trends – price premiums.
Notes: Dependent variable = log of transaction price or list price. Coefficient on interaction of Efficiency * Trial * Treatment and associated 95 % CI displayed for each year, with a base year set as the year prior to treatment introduction (2017). Year indicates pseudo treatment year with treatment switching on the first of March in each period until the end of February in the subsequent year. Observed trial results highlighted in 2018.

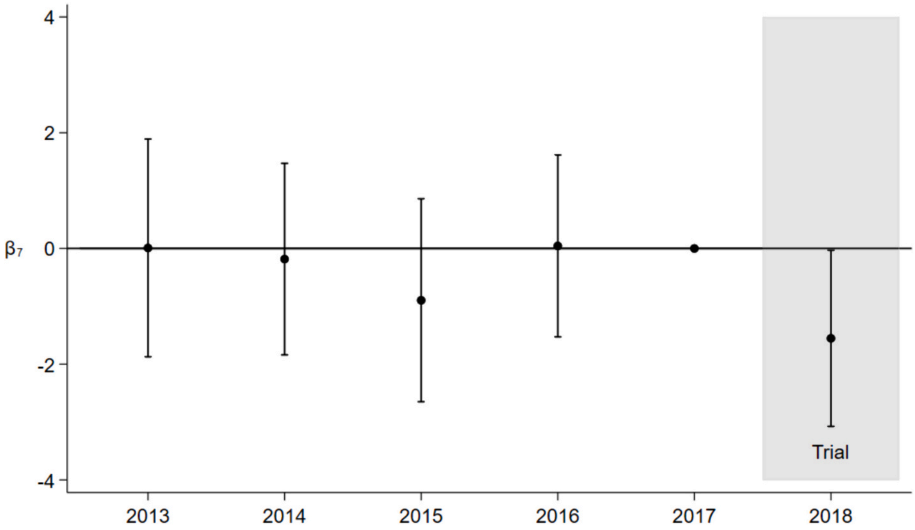

Fig. 4. Effect of monetary information on Time-to-sell.
Notes: Dependent variable = log of transaction price or list price. Coefficient on interaction of Efficiency * Trial * Treatment and associated 95 % CI displayed for each year, with a base year set as the year prior to treatment introduction (2017). Year indicates pseudo treatment year with treatment switching on the first of March in each period until the end of February in the subsequent year. Observed trial results highlighted in 2018.

and Kok, 2011) to between 19 and 28 % for sales premia for office buildings in the US (Eichholtz et al. (2010) and Fuerst and McAllister (2011) respectively). Carroll et al. (2022) examine the impact of monetary energy bill information in the context of the energy efficiency premiums for rental properties in the Dublin area using a discrete choice experiment and find that providing estimates of annual energy bills increases willingness to pay for more efficient rental properties from €28 to €82 per month per improvement in energy efficiency letter grade, a 293 % increase. In this context, the relative increase in energy efficiency premium found here, of 0.9 % (equivalent to 56 % of the pre-trial treatment group premium of 1.6 %), appears small. However, it should be noted that Carroll et al. (2022) considered rental premia, not sales premia, and it was an online stated choice experiment which is likely inflated by hypothetical bias.

Based on the broad findings in the literature as discussed above, and recognizing the limitations of comparisons given the very different

Table 6
Ex-post power analysis.

True effect size (*D*)	alpha	Standard error (*s*)	Power	Type S error	Type M error
0.016	0.05	0.003	0.999	1.51E-13	1.001
0.012	0.05	0.003	0.979	1.29E-09	1.012
0.008	0.05	0.003	0.760	2.44E-06	1.156
0.004	0.05	0.003	0.266	0.0019	1.924
0.001	0.05	0.003	0.063	0.1737	7.093

Notes: based on methodology in Gelman and Carlin (2014) using STATA 14 command *rdesigni* with estimated effect size (*d)* of 0.009 for transaction price with standard error (*s*) of 0.003 (as illustrated in Table 5). True effect size (*D)* ranges are based on external literature of effect sizes found in similar experiments in alternative contexts.

contexts, true effect sizes (D) of 1.6 %, 1.2 %, 0.8 %, 0.4 % and 0.1 % are considered in Table 6. The Gelman and Carlin (2014) methodology uses these true effect sizes based on external information, together with the observed effect (in this paper, a 0.9 % increase in the energy efficiency premium as per Table 5), the standard error of the observed effect (0.3 %) and the *p*-value to estimate the ex-post power, the Type S error rate (the probability that the replicated estimate has the incorrect sign) and the exaggeration rate (Type M error, which is the expectation of the absolute value of the estimate divided by the effect size) assuming a confidence level of 95 %.

The analysis in this paper finds a statistically significant effect size of +0.9 % in the energy efficiency premium. In the case where the true effect size is 0.4 % there is a 0.19 % chance that the estimate will have the wrong sign (Type S error) and the estimated effect will be 1.92 times too high (the exaggeration ratio). However, if the true effect size is smaller again, at 0.1 %, while the probability that the estimate is of the wrong size remains small (17 %), the exaggeration ratio is much higher at over 7 times.

Another limitation of the paper, which was driven by the geographical constraints of the case study country, is the non-random assignment of the Dublin region to the treatment group. As described above, the grouping of four counties into one region was necessary due to the size of the Dublin metropolitan region, and the patterns of search for individuals in commuter areas around the capital. Further, these were assigned to the treatment group as collectively the four counties represented roughly 43 % of listings.

Missing variables could also bias our findings. In this paper, we are controlling for a large number of characteristics, including the location, size and type of the dwelling, however, anything not directly controlled for and which is correlated with energy efficiency may bias the direct estimates – although it is unclear whether and how that would be correlated with treatment (rather than with efficiency). A suggestion for future research would be to include other property features, such as age, if such information is available.

6. 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, while not passing all robustness checks and limited by the low number of treatment counties and non-random assignment of the capital region, still provide broad evidence that house buyers are likely 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, given that we only observe an effect post initial advertisement.

The magnitude of the treatment effect is large – the relative increase in the energy efficiency premium is approximately 0.9 % per BER grade, which equates to roughly 56 % of the pre-trial premium 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.

CRediT authorship contribution statement

James Carroll: Writing – review & editing, Writing – original draft, Validation, Methodology, Investigation, Data curation. **Eleanor Denny:** Writing – review & editing, Writing – original draft, Validation, Supervision, Resources, Project administration, Investigation, Funding acquisition, Conceptualization. **Ronan C. Lyons:** Writing – review & editing, Validation, Investigation, Formal analysis. **Ivan Petrov:** Writing – review & editing, Validation, Formal analysis, Data curation.

Declaration of competing interest

None.

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Appendix. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.eneco.2024.107909.

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