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Do energy-efficient homes improve residents' health? Evidence from insurers' records

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Abstract

This paper provides quasi-experimental evidence of the health benefits of a large-scale, nationwide programme of home energy-efficiency retrofits in the Netherlands, exploiting individual medicine use from insurers' records. We demonstrate that these home upgrades improve children's health, as evidenced by a 4% reduction in the use of respiratory medication. We also find suggestive health improvements for other vulnerable groups, such as the poor.

JEL Codes: I10, Q40, R20

Keywords: Health, Energy efficiency, Renovation, Children, Public housing

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1 Introduction

Do energy-efficient homes improve residents' lives beyond reduced energy expenditure? This question is important because many countries around the world have started programmes to upgrade the quality of housing, particularly its energy efficiency, as about one-third of CO_2 emissions are building-related.[1] In this paper, we provide evidence that health outcomes improve, based on a large-scale quasi-experimental evaluation of energy-efficiency upgrades to homes (i.e. insulation and ventilation) that affected many low-income public housing tenants in the Netherlands.

The medical literature suggests that (energy-efficiency) home upgrades have positive and immediate health effects, but this literature suffers from important limitations.[2] Causal estimates are essentially absent. The only evidence we have is from Künn and Palacios (2024) who provide quasi-experimental results for East Germany after 1990. Using aggregate information on hospital visits and survey data, this study finds that energy-efficiency upgrades decreased hospital visits among older people only.

Our paper also uses a large, nationwide quasi-experiment. We significantly improve upon prior research by exploiting register-based microdata on housing upgrades combined with individual health measures based on detailed medicine use from insurers' records. This allows us to provide precisely estimated health effects. Confidence in our estimates is strengthened by demonstrating that the observed effects are specific to conditions likely to improve with energy-efficient upgrades, such as asthma and respiratory allergies.

We exploit a unique institutional setting. In the Netherlands, in 2012, a new policy was introduced that required almost all public housing associations to upgrade the insulation and ventilation of their old dwellings, more than one million in total. Due to the large number of houses qualifying for this retrofit, only a small share could be upgraded each year. The decision of which dwellings to upgrade, and when, was *not* based on (and not correlated to) tenant characteristics. By law, tenants could not opt out of the retrofit, preventing self-selection out of treatment. During our period of observation (up to 2021),

[1] E.g. Renovation wave in Europe, Weatherization Assistance Program in the US, or Warm Front Home Energy Efficiency Scheme in the UK.

[2] See Barton et al. (2007); Howden-Chapman et al. (2007, 2008); Osman et al. (2010); Woodfine et al. (2011); Heyman et al. (2011); Lajoie et al. (2015). These limitations include (i) treatment is not compulsory, so households may self-select into treatment; (ii) samples are small and non-representative; (iii) health outcomes are self-reported.

about 180,000 individuals received an upgrade, whereas our control group consists of 1.8 million individuals. As we follow individuals over time, we have about 12 million annual observations. This setting thus provides a unique large-scale quasi-experiment for identifying the causal effects of thermal quality upgrades on tenants' health.[3]

Home upgrades may affect the residents' health through various channels. Dutch houses qualifying for the retrofits were mostly built in 1960's under building standards that are no longer acceptable, particularly in terms of insulation and ventilation. These houses may expose residents to health hazards including extreme (low or high) temperatures, draught, damp and mould. Extreme outdoor temperatures have adverse effects on the cardiovascular system (Alahmad et al., 2023). Damp and mould have negative effects on the respiratory system, allergies and asthma (Fisk et al., 2020). Consequently, it is plausible that insulation and ventilation reduce these hazards and positively affect respiratory and cardiovascular health. This is particularly relevant for vulnerable groups such as children.

We use data from various administrative registers available at Statistics Netherlands, which can be linked at the individual level. Our analysis focuses on several types of dependent variables. The first is the use of medication, as recorded by health insurance companies. Specifically, we measure medication use related to the respiratory and cardiovascular systems, as these are the primary health issues through which home upgrades might affect healthcare demand. The second dependent variable is healthcare cost by expenditure type, including general practitioner, pharmacy, and specialty or hospital care costs.

We take advantage of the staggered treatment of retrofits (i.e. different tenants were treated in different years), and exploit this using the state-of-the-art econometric approach introduced by Sun and Abraham (2021).[4] Additionally, we investigate the possibility that the effects vary between different groups of residents, particularly those more at risk of health hazards: the elderly, poor households (Case et al., 2002), and children.

For children, respiratory issues are particularly common (Taussig et al., 2003). On an annual basis, one out of five children uses medication related to the respiratory system

[3]In a companion paper, we show that these upgrades resulted in a *large* upgrade in thermal quality (Roberdel et al., 2024).

[4]We also apply alternative approaches, including a standard two-way fixed effects approach and the approach introduced by Callaway and Sant'Anna (2021).

(e.g., asthma).[5] So, the reduced damp and mould in upgraded homes may have larger effects for children compared to adults. In line with this, our results indicate that home upgrades reduce children's respiratory medication use by about 0.7 percentage points, or about 4%.[6] The effect tends to increase with time. It is mainly driven by a reduction of medication use treating asthma and respiratory allergies, across children of all ages. These results are particularly important because, in terms of lifetime gains, the benefits are the largest for children. For other vulnerable groups, such as the poor, we also find health improvements, but these results are less robust to specification, so we interpret them as suggestive.

This paper relates to several streams of literature. First, we add to the literature on the benefits and costs of energy-efficient homes, see Metcalf and Hassett (1999) and follow-up studies (e.g. Fowlie et al., 2018; Roberdel et al., 2024) and references therein. We show that, next to direct energy savings, home upgrades may have important benefits in terms of health.

Second, our research connects to a substantial literature on the effects of improvements in subsidised housing, which generally do not enhance the lives of adult tenants in terms of higher wages or lower unemployment rates. However, such improvements turn to benefit children, who earn higher wages later in life (Chetty et al., 2016; Chyn, 2018; Pollakowski et al., 2022). We provide evidence that health may be the mechanism at work here.[7]

Third, we relate to the literature that emphasises the relationship between income and health, and the policy implications. The evidence that low income induces poor health outcomes (Ettner, 1996) has been used to argue for policies that increase the incomes of the poor through transfers, which are often politically contentious. Our study contributes by examining whether policies that upgrade the housing quality of the poor may offer an alternative approach that could be less politically contentious.

Fourth, our research engages with studies about upgrades in housing sanitary conditions on residents' health. A recent survey of the upgrades done at the end of the 19th century in various countries (Gallardo-Albarrán, 2024) highlights that the poor and chil-

[5] Respiratory drugs are among the most common medicines used by children, see `https://www.gipdatabank.nl`

[6] The implied reduction in annual pharmacy costs, if the reduction in this type of medicine were the only channel, is about 0.70 euro per child, consistent with our analysis of pharmacy costs.

[7] Case et al. (2002) finds positive effects of child health on income in later life.

dren in particular were affected by unhealthy houses, which motivated governments to upgrade housing sanitation.[8] Effect studies of sanitary upgrades in Bavaria in the 19th century (Brown and Guinnane, 2018) and Mexico in the 20th century (Cattaneo et al., 2009) found reductions in infant mortality and children parasites, respectively. Our research shows that also in a developed country with high quality housing on average, home upgrades can result in important health benefits.

The structure of the article is as follows. Section 2 introduces the institutional background. Section 3 describes the data, the empirical model and the identification. Section 4 presents the main results and heterogeneity. Section 5 concludes.

2 Institutional background

2.1 Dutch public housing: dwellings, residents and health

Our study focuses on the households living in Dutch public housing. The public housing sector in the Netherlands is large and includes 2.2 million dwellings (one third of the housing stock). These properties are owned and managed by non-profit entities called housing associations, and rented out at controlled rent levels to households with an income below the median.[9]

About two-thirds of the public housing stock was built before the 1990s, according to the low thermal quality standards of that time, and until recently had hardly any insulation or double glazing. In the Dutch temperate maritime climate with high humidity, a home's poor thermal quality exposes residents to draught, damp and mould throughout the year, which adversely affect the cardiovascular and respiratory systems, as well as allergies and asthma (Fisk et al., 2020; Alahmad et al., 2023).[10]

[8] For example, in 1901 in the Netherlands, for the first time, a law was introduced to prevent households from living in *unhealthy* houses.

[9] In 2016, households with yearly income below 40K were eligible for public housing. The income test is only done when a household enters a new dwelling. As a result, the public housing renters' population also includes households with higher incomes.

[10] Much of the country lies below sea level and there is much rain, so most of the year humidity is high. Average temperatures lie around 2-6°C in the winter and 17-20°C in the summer. Heatwaves in summers are not uncommon, however.

2.2 Homes' thermal quality upgrades; quasi-experiment

The thermal quality upgrades to the public housing, which we exploit in this paper, took place between 2012-2021.[11] After 2012, Dutch housing associations started enhancing the thermal efficiency of their homes, and by 2020, half a million houses were retrofitted. These upgrades were triggered by the 2012 Energy Efficiency Covenant that aimed at 20% CO_2 reductions by 2020 (Ministry of the Interior and Kingdom Relations, 2012). Increases in rent associated with these upgrades were small, so the treatment should be interpreted as a subsidy.[12]

The large-scale home upgrades consisted of (i) materials added to the walls, facades, and roofs of the old dwellings, to reduce heat loss; (ii) single glass replaced with double or high-efficiency glass; (iii) mechanical ventilation added to sustain healthy air circulation in the insulated structure. These retrofits drastically upgraded the heating efficiency of the houses and increased living comfort. A companion paper provides more details and reports a 20% reduction in gas consumption for heating after the upgrades (Roberdel et al., 2024).

Two characteristics of the home upgrades are important for our identification strategy and allow for a quasi-experimental approach. We highlight these here. First, the total number of dwellings that qualified for a retrofit was very large in 2012. These houses could not be upgraded all at once because of financial and logistical constraints. Therefore, a selection rule to prioritise certain houses was introduced. From discussions with responsible managers from four Dutch housing associations, it appears that, at least during the study period, targeting was largely based on observable building characteristics (e.g. construction period, energy label) and on synchronising the retrofit with scheduled maintenance activities (painting of exterior walls, replacement of lighting, pipes and tubes in the building).[13] Maintenance is a cyclical process where most maintenance activities

[11]Thermal quality is typically categorised based on the European energy label and is assigned to dwellings by trained professionals after a technical inspection. The label takes elements such as insulation quality, heating installation, (natural) ventilation and indoor air climate, solar systems, and built-in lighting into account. Labels 'A-B' are considered good, labels 'E-F-G' are considered bad.

[12]The 2012 agreement stipulated that the energy savings should exceed any rent increase due to the upgrade. We have investigated this further for upgrades in the year 2019, the only year for which we have rental data before and after the upgrade. We find that annual rents increased by about 60 euro (about 0.20% of household income), which is a small fraction of the costs associated with the upgrades.

[13]We are grateful for these discussions to the experts of Bazalt Wonen, Elan Wonen, PreWonen and

are scheduled years ahead (e.g. painting is scheduled every 6 years). It is performed by *complex* - a block of adjacent houses sharing the same building year and similar technical characteristics. The timing of regular maintenance can thus be assumed to be independent of, and in large samples, uncorrelated with, the potential outcomes of thermal quality upgrades.[14] As a result, the assignment of the houses to treatment can arguably be considered random, conditional on a few observable building characteristics such as construction year, energy efficiency and dwelling type.

The second useful feature of the home upgrades is that self-selection into or out of retrofit was impossible for tenants. By Dutch law, if more than 70% of the tenants of a complex agree with the retrofit plans (and this was almost always the case), individual tenants do not have a right to opt out. This means that the assignment to treatment of households can be seen as conditionally random, and the treated sample is representative of the public housing renters' population in the country.[15]

2.3 Medicine use and health expenditures as proxy for health outcomes

We will rely on individual medicine use and healthcare costs documented by insurers as proxies for the health status of an individual. For all residents of the Netherlands, it is mandatory to enrol at a private non-profit health insurer since the 2006 Health Insurance Act (Wammes et al., 2020; Artmann et al., 2022). Health insurers are obliged to provide a mandatory benefit package and cannot refuse applicants. The mandatory package is determined by the national government and includes, among other things, general practitioner care, specialty care, hospital care and prescription drugs. For children under 18, dental care and physiotherapy are also included in the mandatory benefit package. In addition to the mandatory coverage, about 84% of the population enrols in supplementary insurance that covers mostly dental care, physiotherapy, vision care, and drug copayments (Wammes et al., 2020).

The health insurance coverage is individual, and children under 18 are automatically covered. Every individual over age 18 with health insurance coverage has to pay an

Woonbedrijf.

[14]The replacement of the boiler - an intervention that does affect house energy efficiency - does not fall under regular maintenance and is typically dwelling-specific.

[15]We note that tenants could vote with their feet and relocate to another house if they did not agree with the home upgrade. We will show that this did not happen.

annual premium of about 1,500 euro (in 2021) and an annual deductible of about 385 euro. Individuals over age 18 pay the full price of healthcare up to the deductible. This deductible covers, among other things, hospital, pharmacy, and medical specialist care costs, but there is no deductible for general practitioner care or for children's healthcare expenses. The costs over the deductible are paid by the insurer.

We use information on the individual-based yearly expenses from the mandatory package claimed to insurers, including expenses below the deductible.[16] The expenditures are administered in three larger groups: (i) general practitioner, (ii) pharmacy, and (iii) specialty-and-hospital care.[17] We also have a dichotomous indicator of individual-level yearly prescription medicine usage, whereby medicines are categorised based on the Anatomical Therapeutic Chemical classification system (ATC) (e.g. the ATC code R03 contains a number of drugs used against asthma). These records document drug use prescribed under the mandatory coverage but exclude drugs provided during hospital stays.[18]

In the Dutch healthcare system, general practitioners (GP) act as gatekeepers. Except in case of accidents, a patient only gets access to the hospital and specialty care upon referral from the GP (Wammes et al., 2020). GP's are also authorised to prescribe a wide number of drugs. Therefore, one expects that any meaningful change in the overall health of the residents due to home upgrades will be reflected in the number of general practitioner visits and the medicines prescribed. Consequently, GP visits and medication used will be our main indicators of the changes in health. As discussed in Section 2.1, home upgrades may specifically affect the respiratory or cardiovascular system. We will therefore focus on changes to medication use related to these two systems.

[16] We do not include costs related to the supplementary insurance for two reasons. First, most of the cost types covered by supplementary insurance (e.g. dental care, physiotherapy, eyeglasses, lenses, and contraceptives), will not be affected by home upgrades. By including these costs, one would increase the measurement error in the dependent variable, making the econometric approach less efficient. Second, these data are not available to us.

[17] In the Netherlands, specialty care is provided in hospitals, so this makes comparison with other studies for countries where specialty care is distinctly administered, difficult.

[18] Unfortunately, we do not have information about the costs related to drug use in hospitals.

3 Data, model and identification

3.1 Data

We exploit information on thermal quality upgrades in homes performed by 128 (out of 300) Dutch public housing associations between 2012 and 2021. The housing associations in the sample collectively own about 1 million dwellings located in all regions of the country. Our sample covers 40% of the total public housing stock and is representative of the Dutch public housing sector.

We combine three datasets available for the years 2012-2021. The first dataset includes longitudinal dwelling-level data on building characteristics and the treatment year for all home upgrades.[19] We select dwellings that qualified for an insulation and ventilation upgrade. In essence, this entails two criteria: a building year before 1993 (before this year, the low thermal quality building standards applied) and low thermal quality, as reflected by the European energy label 'C' to 'G', in 2012, so at the beginning of our observation period. We drop dwellings upgraded in the first and the last year of our observation period since their pre- or post-treatment periods are not observed. We also exclude student housing and care homes. The resulting sample contains 669K dwellings. Of these, 87K dwellings received a thermal quality upgrade during our study period.

The second dataset contains restricted-access individual-level longitudinal microdata on outcome variables, collected by health insurers and made available through Statistics Netherlands. These include individual-level medicine prescriptions and healthcare costs (converted to 2015 euro) that are part of the mandatory insurance benefit package and were reimbursed by insurers. Medication prescription is categorised using ATC codes. This means, for example, that we know whether respiratory medicine has been used (code R), but also whether it was prescribed for chronic obstructive pulmonary disease (COPD) or asthma (code R03). Healthcare costs refer to specialty and hospital costs and pharmacy costs.[20] We also have information about general practitioner costs – converted to the equivalent number of regular GP visits.[21]

[19]We thank engineering bureau Atriensis for sharing with us their Energy Monitor data.

[20]We do not have information about drug usage *quantity*, only whether a drug was used. This is one reason to focus on pharmacy costs, as increased quantity is reflected in an increase in overall pharmacy costs.

[21]A regular GP visit costs about 12 euro. The cost of a GP visit at home is about twice the cost of a regular GP visit. A telephone consultation implies lower costs.

Finally, the third dataset, also made available by Statistics Netherlands, includes microdata on socio-economic characteristics of households, yearly energy consumption, and house energy efficiency.

The three datasets are merged at the individual level for the years 2012-2021. This yields, for about two million individuals, key information on (1) medicine use and health-care costs, (2) housing characteristics, including information about the thermal quality upgrade, and (3) individual and household characteristics.

3.2 Descriptive statistics

Table 1 shows descriptive statistics across all our sample years. The unit of observation is a person per year. In panel A, it is evident that about 7% of the observations received a home upgrade.[22] Home upgrades are not concentrated in specific years: the average treatment year is 2016.

Panel B shows the health outcomes. These indicate that medicine use related to respiratory problems and cardiovascular problems is quite common. For example, 24% of individuals use respiratory medication in a specific year. For cardiovascular medicine, the use is even higher. Individuals visit their GP 5 to 6 times per year. The costs of specialty-and-hospital care are about 1400 euro, on average.

In panel C, we emphasise that a substantial number of individuals (slightly more than half of our sample) belong to vulnerable groups (i.e., they are below 18, above 65, or poor). Children and seniors together make up 40% of the sample, while every fourth person in the sample is classified as poor.[23,24]

According to Panel D, where we show household and housing characteristics, the average annual disposable household income is approximately 30,000 euro (about one third below the average household income for the whole population). The average size of a public housing unit is around 90 m^2, with 37% classified as apartments. Most houses

[22] We will use the change in the natural gas use after a thermal quality upgrade to show that the upgrade was indeed effective.

[23] Obviously, a substantial share of the sample combines two vulnerable characteristics: poor and child, or poor and senior.

[24] The poor are defined as households with a yearly income below 130% of the social minimum, i.e., the 'minimal amount to cover basic personal needs', which depends on household composition (Statistics Netherlands). In 2017, it amounted to a monthly income of 1,040 euro for single persons and 1,960 euro for couples with two children.

Table 1: Descriptives

	Mean	Std. deviation
Panel A: Treatment		
Home upgraded	0.07	0.26
Home upgrade year	2016.37	2.06
Panel B: Health outcomes		
Respiratory medication (R)	0.24	0.43
Asthma/COPD (R03)	0.12	0.32
Cough and cold preparations (R05)	0.04	0.20
Nasal preparations (R01)	0.08	0.28
Antihistamines (R06)	0.09	0.28
Cardiovascular medication (C)	0.42	0.49
GP visit count	5.55	8.40
Pharmacy costs (euro)	327.79	787.72
Hospital and specialist costs (euro)	1426.27	4536.19
Panel C: Vulnerable groups		
Child (age below 18)	0.19	0.39
Senior (age over 65)	0.20	0.40
Poor (income below 130% social minimum)	0.26	0.44
Vulnerable (child, senior or poor)	0.54	0.50
Panel D: Household and housing characteristics		
Household income (1,000 euro)	30.07	15.93
House size (m^2)	87.35	25.52
Apartment	0.37	0.48
Construction year	1967.13	15.66
Energy label EFG	0.29	0.45
Panel E: Sample size		
No. persons	2030176	
No. dwellings	668935	
No. years	10	
No. obs	12211838	

Notes: Mean and standard deviation calculated on our individual level sample across years 2012-2021. Capital letters with codes between brackets refer to ATC codes. Costs in 2015 prices. Income not inflation-corrected.

are relatively old: the average construction year is 1967 (most houses were built between 1945 and 1976). In panel E, we show the sample size of our data: 2 million individuals followed over 10 years for a total of 12 million observations.

One would only expect effects of home upgrades when the home upgrades non-negligibly enhance the quality of treated houses. We have therefore examined to what extent this is the case. In Appendix A, we show that natural gas use, the main indoor heating source, fell by about 20% after treatment. In addition, tenant turnover rates fell permanently, implying that the perceived house quality was higher after treatment.

3.3 Econometric approach

We are interested in the effect of upgrades on the health of individuals, specifically those who are treated during our period of observation. This effect can be analysed using a two-way fixed effects approach, where one set of effects pertains to individuals that live in a specific building, and the other set of fixed effects corresponds to time. This type of approach was previously dominant in the literature; however, more recent studies introduce refined methodologies that mitigate the risk of inconsistent results when treatments are staggered (de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021).

Instead of two-way fixed effects, our main approach follows Sun and Abraham (2021) to estimate the average treatment effect on the treated (ATT). For identification purposes, the never-treated serve as a control group, *i.e. individuals living in dwellings that qualify for an upgrade but did not receive treatment during our sample years.*

Our baseline econometric specification is:

$$y_{i,j,t} = \alpha R_{j,t} + \delta_{i,j} + \phi_{a,t} + u_{i,j,t}. \tag{1}$$

Here $y_{i,j,t}$ represents healthcare outcomes of individual i living in dwelling j in year t. Note that the age a of the individual in a certain year is defined by i and t. The binary treatment variable $R_{j,t}$ takes value 1 in the years following the retrofit and value 0 before; $\delta_{i,j}$ are individual-house fixed effects; $\phi_{a,t}$ are year fixed effects interacted with tenants' age fixed effects and $u_{i,j,t}$ is the idiosyncratic error term.[25] Standard errors are clustered

[25]In our main analysis, we do not include time-varying dwelling characteristics (i.e. solar panels, boilers) as they may be endogenous. In one of our sensitivity analyses (available upon request), we include these

at the housing complex level, as it is at this level that home upgrades take place. We are interested in the coefficient α, which can be interpreted as the ATT of a home upgrade on healthcare outcomes.

In equation (1), we deviate from standard specifications in terms of fixed effects. The most common specification entails that one includes separate individual fixed effects and/or building fixed effects, and time fixed effects, but no interactions between these fixed effects (Angrist and Pischke, 2008). We include individual-house and age-year interactions between these fixed effects for several reasons.

First, we use individual-house fixed effects, $\delta_{i,j}$, to estimate the treatment effect only from individuals residing in the same house before and after the upgrade. These fixed effects therefore control for (spatial) sorting of individuals into certain houses or neighbourhoods (Combes et al., 2008). To examine the importance of including these interactions, we have estimated models that include separate individual and building fixed effects rather than individual-house fixed effects. Results are very similar.

Second, which turns out to be more important, we also include year fixed effects interacted with age, $\phi_{a,t}$ (instead of standard year fixed effects). The reason for including these interaction effects is that omitting them violates the parallel trend assumption, which is the primary underlying assumption of approaches relying on two-way fixed effects (de Chaisemartin and D'Haultfœuille, 2020). In other words, parallel trends are (allowed to be) age-specific. This makes sense because health, approximated by medicine use and healthcare costs, depends on age in a *convex* way, meaning the parallel trends assumption must also be age-specific.[26] This is intuitive: the increase in healthcare costs associated with ageing is much higher for the elderly than for younger individuals.

We use individuals living in dwellings that qualify for an upgrade but did not receive treatment during our sample years as the control group. This choice of the control group

dwelling characteristics. The results are very similar. In another sensitivity analysis, we have included time-varying household characteristics (e.g., household income). Again, the results remain robust. To be more precise, the size of our main estimate, the respiratory health improvement among children, becomes (slightly) larger. The standard errors also become (slightly) larger, partly because these controls are not always observed, so we have a smaller dataset. As arguably controlling for time-varying household characteristics is unlikely to address any omitted variable bias, we prefer specifications without time-varying household characteristics, as these specifications are more efficient.

[26]This claim is supported by regression analysis of health outcomes on age. The results can be provided upon request.

seems very convincing. Nevertheless, the choice of this control group can be criticized if the treatment and control groups are very different from each other in terms of observed characteristics.

For that reason, Table A1 in the Appendix reports balancing tests by comparing the characteristics of the treated and the control groups in the first year of observation (2012), hence before any treatment occurs. We distinguish between several types of characteristics: (i) healthcare costs and cardiovascular and respiratory medication, our main outcomes of interest, (ii) detailed respiratory medication use - including medication related to conditions such as asthma/COPD, (iii) socio-economic variables, and (iv) dwelling characteristics.

It appears that health outcomes and socio-economic variables are well balanced between treatment and never-treated groups, while the dwelling characteristics are not, in line with our assumption that treatment assignment is based on dwelling characteristics.[27] We will therefore account for dwelling characteristics in all specifications of our empirical model by including individual fixed effects *that are dwelling-specific*. The treatment assignment can thus be interpreted as conditional random, i.e., conditional on (individual-specific) dwelling fixed effects (see Section 2.2). As discussed in Section 2.1, the concern of self-selection into or out of the treatment group is addressed in our setup. By law tenants could not opt out of the home upgrade program while living in the dwelling.[28]

A more important issue is that the approach introduced by Sun and Abraham (2021) estimates treatment effects by treatment year cohort and by calendar year, and then aggregates the estimates into a single average treatment effect *using the year before treatment as reference*. In our sensitivity analyses, we will also show two-way fixed effect estimates that take all the years before treatment as reference.

Finally, we obtain event study estimates by aggregating estimates by year-to-treatment.[29]

[27] One explanation for the latter result is that buildings that were easier to upgrade were treated earlier. Energy use seems not well-balanced using univariate statistics. However, it is balanced when controlling for dwelling characteristics, see our companion paper, Roberdel et al. (2024).

[28] In theory, tenants might have avoided the retrofit by moving out of the dwelling, but as this is very costly, and residential mobility rates within the public sector are very low, this is unlikely to create a selection bias. In line with that, we find that around the retrofit year, there is a suggestive temporary, small, increase in the tenant turnover rate, see Figure A1b. This increase implies that about 99% of the tenants were exogenously treated, so there is no concern of self-selection through residential moving.

[29] We show event study estimates for a time interval of up to five years before and after treatment

These event study estimates provide a visual confirmation of the causal effects we measure.

3.4 Alternative econometric approaches

3.4.1 Two-way fixed effects approach

Our main results refer to the Sun and Abraham (2021) approach, where, for identification purposes, the never-treated are used as a control group, i.e., individuals living in dwellings that qualify for an upgrade but did not receive treatment during our sample years. This approach is less efficient than a standard two-way fixed effects model, where the control group also contains other individuals (e.g. the treated later). Consequently, we will discuss sensitivity analysis in which we apply the standard two-way fixed effects approach.

One advantage of using a standard two-way fixed effects model is that it facilitates estimating models that include interactions between the treatment and specific tenant groups, g (e.g., children). Therefore, when we focus on the effects for specific groups and apply two-way fixed effects models, we will estimate these models with interactions for different groups using the full sample, where we additionally control for group-age-specific time trends:

$$y_{i,j,t} = \alpha_g R_{j,t} + \delta_{i,j} + \phi_{a,g,t} + u_{i,j,t}. \tag{2}$$

Note that g may vary over time for each individual and be, therefore, time-specific. Here, $\phi_{a,g,t}$ are year fixed effects interacted with age fixed effects and group fixed effects. Our main interest is in the coefficient α_g, which varies by group. In Appendix, Figures C1 and B2, we show that, on key outcomes, two-way fixed effects and Sun and Abraham event study estimates are almost identical, suggesting that any bias due to staggered treatment is limited.

3.4.2 Callaway and Sant'Anna (2021) approach

The main alternative discussed in the literature to the Sun and Abraham (2021) approach is an approach introduced by Callaway and Sant'Anna (2021), where the control group consists of individuals that are treated later on. In our application, the latter approach is extremely inefficient (as the number of individuals that are treated later on is almost

(estimated year-to-treatment effects outside this time interval typically exhibit large standard errors and provide less information).

a factor of 10 smaller than the number of individuals that are not treated), resulting in standard errors that are typically three to seven times larger than those obtained for the Sun and Abraham (2021) approach. Nevertheless, the signs of the point estimates obtained through this approach tend to support our results.[30]

4 Results

4.1 Full population

Table 2: Effects of home upgrade on health

	Mean	Estimate	SE	p-value
Respiratory medication (R)	0.25	-0.0012	0.0011	0.284
Cardiovascular medication (C)	0.43	0.0013	0.0012	0.294
GP visit count	5.43	0.02	0.04	0.512
Pharmacy costs (euro)	317.01	2.33	1.63	0.152
Hospital and specialist costs (euro)	1392.03	10.09	13.32	0.449

Notes: The table shows estimates of Equation (1) for 5 separate regressions. Mean is the sample mean at baseline (i.e. year before treatment). Number of observations (resp. number of unique treated individuals): 11,760K (177K). The control group comprises never-treated individuals (Sun and Abraham, 2021). Standard errors clustered at housing complex level.

We first report the average treatment effects for the full population for five different health outcomes; see Table 2. Clearly, at the level of the full population, there is no evidence that home upgrades improve health. In essence, all the effects are close to zero, with small standard errors. Only for respiratory medication is there a suggestive negative effect.

4.2 Vulnerable groups

The above analysis suggests a negative effect on respiratory medicine use and no improvement on other measures of health. This analysis, however, conceals important variation in the treatment effects for vulnerable groups. Vulnerable individuals most at risk of respiratory health hazards are more likely to experience beneficial effects from home quality

[30]Another complication is that allowing for age-specific time trends is not possible with the Callaway approach. Instead, we control for age in a quadratic form, so strictly speaking, the results are not directly comparable.

upgrades. For that reason, we focus on children and the elderly whose vulnerability is driven by age (either young or old), and poor households whose financial situation does not allow them to heat a dilapidated home properly (Taussig et al., 2003; Ettner, 1996; Gallardo-Albarrán, 2024).

Table 3 shows causal effects on the same health outcomes as above, for individuals belonging to the vulnerable group. In panel D, at the bottom of the table, it shows that home upgrades reduce medication use by 0.35 percentage points for vulnerable individuals, on average. This effect is not only statistically significant, but it is also economically meaningful, as it refers to a more than one percent reduction in respiratory medication use. Again, for other measures of health, there is no evidence of any effect of upgrades.

These results raise the question of whether certain subgroups are more likely to be more strongly affected by home upgrades. Panels A-C convincingly show that, in particular, children and the poor reduce their respiratory medication, but for the 65+ there is little evidence. In particular, for children, the reduction in respiratory medication is substantial and equal to about 4% of the mean. But also, for the poor, the reduction in respiratory medication is meaningful and equal to about 2% of the mean.

4.3 Types of respiratory medicine use

We have documented negative effects on respiratory medication use for two vulnerable groups: children and the poor. Here, we provide a more detailed analysis of this effect, distinguishing between three types of respiratory medication (on a three-digit ATC level): asthma/COPD, cough and cold remedies, and antihistamines used against respiratory allergies.[31] In panel A of Table 4, we show, for reasons of comparability, the estimates for the whole sample. Recall that we reported a negative effect on respiratory medication, but the effect was not statistically significant at the 5% level. If we focus, however, on the effects on the different types of respiratory medication, it appears now that there is a statistically significant reduction in antihistamines.

Interestingly, looking at the respiratory medicine use by the vulnerable individuals, see panel E, we find that the reduction in respiratory medicine use is mainly due to a

[31] We also examined the effect on nasal preparations (R01). It appears that the point estimate for this outcome is negative in all specifications, but never statistically significant at the 5% level. Consequently, this can be interpreted as suggestive evidence, although we are not able to show any effect. These results can be received upon request.

reduction in antihistamines and (-3%) cough and cold preparations (-5%). For children, though, this reduction occurs mainly through a drop in asthma/COPD medication (-5%) and antihistamines (-4%), whereas for the poor, the reduction in respiratory medicine comes mainly through a drop in antihistamines (-4%). In conclusion, it seems that the estimated reduction in respiratory medicine use is not identified through the reduction of one specific medication or one specific group, but is due to a combination of groups and different medications used.

One may wonder to what extent these results are supported by event study estimates. For that reason, in Figures B1 to B5 in the Appendix, we report the event study estimates for respiratory medication use.[32] It appears that point estimates prior to treatment are close to zero for each year observed, supporting our identification strategy. For the years after treatment, the event study estimates indicate improvements in respiratory health.

4.4 Children

The above results indicate that the effects on children are the most pronounced. We have investigated this further by distinguishing between three subgroups: up to 6 years old, 6 to 12 years, and 12 to 18 years. Table 5 shows estimates for these subgroups. Point estimates are mostly negative and each group shows at least one statistically significant effect at the 10% level, despite the lower precision due to smaller samples. Thus, it does not appear to be the case that the negative causal effects for children are mainly driven by one specific age group. The results suggest that all age groups benefit to some extent.

4.5 Robustness and placebo healthcare costs

In this section, we test the robustness of the main results to a number of specification changes. It appears that our results are extremely robust to specification.

First, so far we have discussed results based on the approach by Sun and Abraham (2021). Two disadvantages of this approach are that (i) it generates relatively large standard errors when focusing on subgroups and (ii) it estimates the treatment effects relative to the year before treatment. For these reasons, we also apply a two-way fixed effects estimator, which can be estimated for the full sample, with an interaction between vulnerable groups and the treatment indicator. Table C1 in the Appendix shows the

[32]In these graphs, we show results for a period of 5 years before and 5 years after the treatment, as the confidence estimates become large outside this interval.

Table 3: Effects of home upgrade on health, vulnerable groups

	Mean	Estimate	SE	p-value
Panel A: Child (age below 18)				
Respiratory medication (R)	0.18	-0.0069	0.0025	0.007
Cardiovascular medication (C)	0.19	-0.0012	0.0029	0.688
GP visit count	2.87	-0.02	0.03	0.545
Pharmacy costs (euro)	65.26	-1.17	2.06	0.571
Hospital and specialist costs (euro)	479.21	12.71	13.60	0.350
Panel B: Senior (age over 65)				
Respiratory medication (R)	0.31	-0.0017	0.0023	0.465
Cardiovascular medication (C)	0.80	0.0024	0.0020	0.234
GP visit count	8.37	0.00	0.09	0.963
Pharmacy costs (euro)	674.35	3.38	4.52	0.455
Hospital and specialist costs (euro)	2868.87	4.96	41.93	0.906
Panel C: poor (income below 130% social minimum)				
Respiratory medication (R)	0.29	-0.0057	0.0026	0.030
Cardiovascular medication (C)	0.50	0.0015	0.0027	0.581
GP visit count	6.61	-0.06	0.08	0.401
Pharmacy costs (euro)	431.72	-2.61	4.01	0.515
Hospital and specialist costs (euro)	1643.95	23.92	31.05	0.441
Panel D: vulnerable (child, senior or poor)				
Respiratory medication (R)	0.26	-0.0035	0.0016	0.028
Cardiovascular medication (C)	0.49	0.0005	0.0016	0.776
GP visit count	5.85	0.01	0.05	0.878
Pharmacy costs (euro)	383.58	0.73	2.39	0.761
Hospital and specialist costs (euro)	1647.55	10.97	20.56	0.594

Notes: The table shows estimates of Equation (1) for 20 separate regressions. Mean is the sample mean at baseline (i.e. year before treatment). Number of observations (resp. number of unique treated individuals): Panel A 2,210K (37K); Panel B 2,407K (36K); Panel C 2,598K (40K); Panel D 5,839K (91K). The control group comprises never-treated individuals (Sun and Abraham, 2021). Standard errors clustered at housing complex level.

estimates. We notice two things. One, the standard errors are indeed smaller, by about 10 to 50%. Two, the estimates also point to a statistically significant reduction in respiratory medication, in line with the Sun and Abraham (2021) Table 3 estimates. Typically, the point estimates are somewhat smaller, but still sizeable. For example, for children, the reduction in respiratory medicine use is about 3%, well within the confidence interval of our main estimate from Table 3.

Table 4: Effects of home upgrade on respiratory health

	Mean	Estimate	SE	p-value
Panel A: whole sample				
Respiratory medication (R)	0.25	-0.0012	0.0011	0.284
Asthma/COPD (R03)	0.12	0.0004	0.0007	0.603
Antihistamines (R06)	0.09	-0.0016	0.0007	0.029
Cough and cold preparations (R05)	0.04	-0.0002	0.0007	0.818
Panel B: children (age below 18)				
Respiratory medication (R)	0.18	-0.0069	0.0025	0.007
Asthma/COPD (R03)	0.09	-0.0042	0.0018	0.023
Antihistamines (R06)	0.08	-0.0030	0.0018	0.089
Cough and cold preparations (R05)	0.01	-0.0005	0.0006	0.422
Panel C: seniors (age over 65)				
Respiratory medication (R)	0.31	-0.0017	0.0023	0.465
Asthma/COPD (R03)	0.19	0.0001	0.0017	0.966
Antihistamines (R06)	0.06	-0.0002	0.0013	0.878
Cough and cold preparations (R05)	0.07	-0.0025	0.0017	0.144
Panel D: poor (income below 130% social minimum)				
Respiratory medication (R)	0.29	-0.0057	0.0026	0.030
Asthma/COPD (R03)	0.15	-0.0020	0.0018	0.256
Antihistamines (R06)	0.10	-0.0043	0.0017	0.010
Cough and cold preparations (R05)	0.05	-0.0020	0.0016	0.190
Panel E: vulnerable (child, senior or poor)				
Respiratory medication (R)	0.26	-0.0035	0.0016	0.028
Asthma/COPD (R03)	0.14	-0.0015	0.0011	0.168
Antihistamines (R06)	0.08	-0.0020	0.0010	0.042
Cough and cold preparations (R05)	0.04	-0.0018	0.0009	0.048

Notes: The table shows estimates of Equation (1) for 20 separate regressions. Mean is the sample mean at baseline (i.e. year before treatment). Number of observations (resp. number of unique treated individuals): Panel A 11,811K (177K); Panel B 2,217K (37K); Panel C 2,410K (36K); Panel D 2,601K (40K); Panel E 5,851K (91K). The control group comprises never-treated individuals (Sun and Abraham, 2021). Standard errors clustered at housing complex level.

Second, we have estimated models for the subsample of children that have used respiratory medication at least once during our period of observation. In percentage terms, we find identical reductions in respiratory medication. Our estimates (available upon request) suggest a sizeable 1% reduction in GP visits and a 4% reduction in pharmacy costs; however, these estimates are not statistically significant even at the 10% significance level.

Table 5: Effects on respiratory health, children

	Mean	Estimate	SE	p-value
Panel A: age below 6				
Respiratory medication (R)	0.18	-0.0084	0.0060	0.158
Asthma/COPD (R03)	0.12	-0.0146	0.0050	0.003
Antihistamines (R06)	0.06	0.0020	0.0038	0.587
Cough and cold preparations (R05)	0.00	0.0004	0.0004	0.286
Panel B: age 7-12				
Respiratory medication (R)	0.18	-0.0126	0.0052	0.016
Asthma/COPD (R03)	0.08	-0.0042	0.0033	0.200
Antihistamines (R06)	0.09	-0.0071	0.0037	0.054
Cough and cold preparations (R05)	0.00	0.0001	0.0010	0.931
Panel C: age 13-18				
Respiratory medication (R)	0.19	-0.0093	0.0049	0.059
Asthma/COPD (R03)	0.07	-0.0042	0.0029	0.156
Antihistamines (R06)	0.09	-0.0049	0.0037	0.179
Cough and cold preparations (R05)	0.02	-0.0018	0.0019	0.344

Notes: The table shows estimates of Equation (1) for 12 separate regressions. Mean is the sample mean at baseline (i.e. year before treatment). Number of observations (resp. number of unique treated individuals): Panel A 678K (12K); Panel B 702K (12K); Panel C 756K (13K). The control group comprises never-treated individuals (Sun and Abraham, 2021). Standard errors clustered at housing complex level.

Third, our control group consists of never-treated individuals. As an alternative, we use the group of not-yet-treated (i.e., individuals who did not yet receive a home upgrade) as a control by applying the approach by Callaway and Sant'Anna (2021), which also addresses staggered treatment bias. In Appendix D, Table D1 reports the average treatment effect on respiratory medication use for this approach. Again, we find negative effects, similar in size to the estimates reported above, but due to large standard errors, these results are statistically insignificant.

Fourth, we separately estimate short run and long run effects. Our results are in line with economic intuition: the long-run effects (after 5 years) are stronger than the short-run effects. See Appendix E

Finally, we have estimated the effect of home upgrades on a range of other healthcare costs that are unlikely to be affected by home thermal quality upgrades. These estimated effects can be interpreted as placebo effects. Two examples are dentistry costs, and other

GP costs, which are essentially administrative fees charged independently of visits to the GP. See Appendix F. No statistically significant effects are found despite small standard errors, as small as 0.4% of the mean for other GP costs.

5 Conclusion

A body of research suggests a relationship between poor housing conditions and adverse health outcomes. This relationship is especially important in the light of the recent numerous policy programs worldwide that aim at upgrading the energy-efficiency and thermal quality of old homes. This relationship suggests, but does not prove, that home upgrades improve health outcomes of individuals living in upgraded housing. Causal evidence is, however, scarce, and essentially non-existent for developed countries. In this paper, we perform a large-scale quasi-experimental evaluation of how thermal quality retrofits in deprived houses affect residents' health in the Netherlands.

We exploit a unique institutional setting, involving a conditionally random treatment assignment, to derive causal effects of home thermal quality upgrades. We make use of individual-level panel data of healthcare use outcomes from two million Dutch public housing residents, who are followed over a 10-year time interval to derive treatment effects on residents' healthcare use.

Our results indicate that home upgrades have an important impact on residents' healthcare use of vulnerable groups, defined here as individuals below 18 years, above 65 years, and poor individuals, which represent more than half of our sample. For individuals belonging to these vulnerable groups, we find clear negative effects. In particular, we document that the number of children taking respiratory medication falls by about 4%, following home upgrades. The effect tends to increase in the longer run.

From a policy perspective, our results suggest that health benefits, particularly for children, should be considered in cost-benefit analyses of housing renovation programmes. This is especially important given that energy savings alone might not compensate for the renovation costs (Fowlie et al., 2018; Christensen et al., 2023). Future research should investigate the long-term benefits of children's health improvements resulting from home upgrades.

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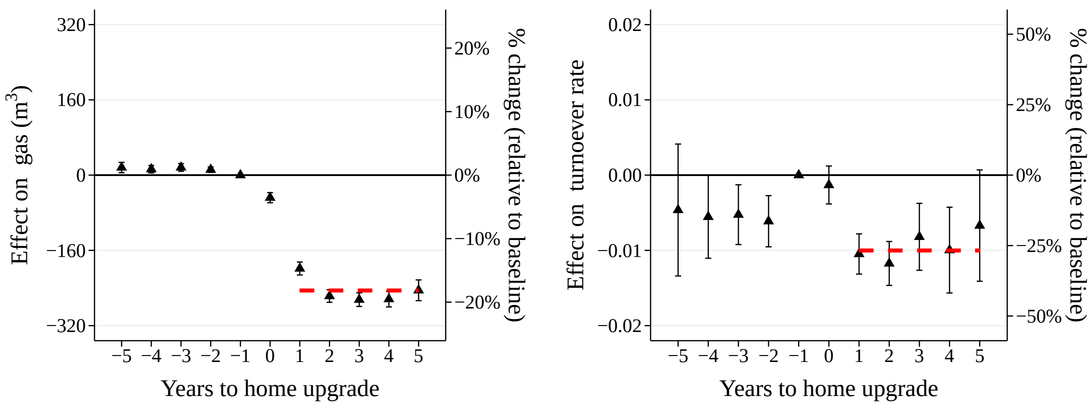
A Appendix Identification

A.1 Actual and perceived home quality upgrade

Here we show that the home upgrades in our data indeed led to substantial upgrades in the actual and perceived quality of the houses. Figure A1a shows that natural gas use, the only indoor heating source in our data, fell by about 20% after treatment. So, the thermal quality upgrade was indeed effective. Figure A1b demonstrates further that tenant turnover rates decreased. This implies that also the perceived house quality increased, tenants stayed longer in the dwelling than before.[33]

Figure A1: Effects of home upgrades on energy use and tenant turnover

(a) Energy use (b) Tenant turnover



Notes: Event study estimates. The control group comprises never-treated individuals (Sun and Abraham, 2021). The bars represent the 95% confidence interval. The percentage change is relative to the sample average of the dependent variable in the year before home upgrade. Standard errors are clustered at housing complex level. The dashed horizontal line represents the average treatment effect and the red color indicates a statistically significant effect at a 10% level.

A.2 Balancing test

[33] The figures highly improves on the analyses from Roberdel et al. (2024), as we use here a longer period of time.

Table A1: Balance between treatment and control groups

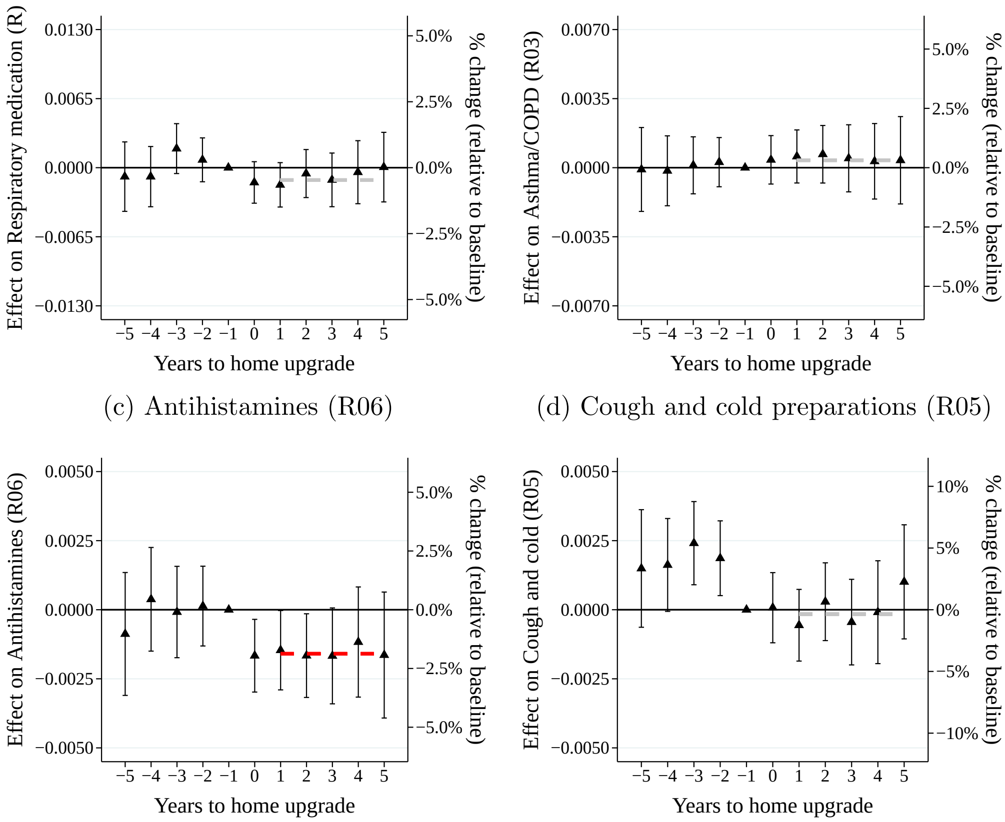
	Treatment	Control	p-value	SMD	VR
Panel A: Health care costs and medication use					
Respiratory medication (R)	0.25	0.25	0.65	0.00	1.00
Cardiovascular medication (C)	0.42	0.42	0.59	0.00	1.00
GP visit count	5.09	5.16	0.00	0.01	0.95
Pharmacy costs (euro)	336.45	351.83	0.00	0.02	0.91
Hospital and specialist costs (euro)	1368.51	1395.69	0.01	0.01	0.97
Panel B: Respiratory medication					
Asthma/COPD (R03)	0.12	0.12	0.40	0.00	1.00
Cough and cold preparations (R05)	0.04	0.04	0.02	0.01	1.03
Nasal preparations (R01)	0.08	0.08	0.00	0.01	0.97
Antihistamines (R06)	0.08	0.09	0.00	0.01	0.97
Panel C: Socio-economics					
Child (age below 18)	0.21	0.18	0.00	0.05	1.09
Senior (age over 65)	0.19	0.19	0.00	0.01	1.02
Female	0.52	0.52	0.32	0.00	1.00
Household income (1,000 euro)	27.91	27.67	0.00	0.02	0.74
Poor	0.23	0.23	0.03	0.01	0.99
Panel D: Dwelling characteristics					
House size (m^2)	89.96	87.11	0.00	0.12	0.48
Construction period 1906 - 1939	0.06	0.06	0.90	0.00	1.00
Construction period 1940 - 1965	0.45	0.30	0.00	0.31	1.19
Construction period 1966 - 1976	0.42	0.32	0.00	0.22	1.13
Construction period 1977 - 1992	0.07	0.32	0.00	0.68	0.29
Apartment	0.28	0.37	0.00	0.19	0.87
Energy label EFG	0.40	0.27	0.00	0.26	1.21
Electricity (1,000 KWh)	2.90	2.85	0.00	0.04	0.99
Gas (1,000 m^3)	1.54	1.34	0.00	0.33	0.92
No. persons	187173	1050673			

Notes: The table reports a balancing test between treatment and control groups for the sample year 2012. The columns *Treatment* and *Control* report the mean values of selected covariates in 2012. The column *p-value* reports the p-value of a mean equality test between treatment and control group. The column *SMD* reports the standardised mean difference between the treatment and the control group. The column *VR* reports the variance ratio. $SMD = |\bar{X}_{treated} - \bar{X}_{control}|/\sqrt{\left(S^2_{treated} + S^2_{control}\right)/2}$ and $VR = S^2_{treated}/S^2_{control}$, where $\bar{X}$ is the sample mean and S^2 is the sample variance. The balancing is considered good for *SMD* smaller than 0.25 *VR* between 0.5 and 2 (Rubin, 2001; Stuart, 2010).

B Appendix Event study

This appendix contains event study figures that correspond to the results of Table 4.

Figure B1: Effects on respiratory health, whole sample



(a) Respiratory medication (R)

(b) Asthma/COPD (R03)

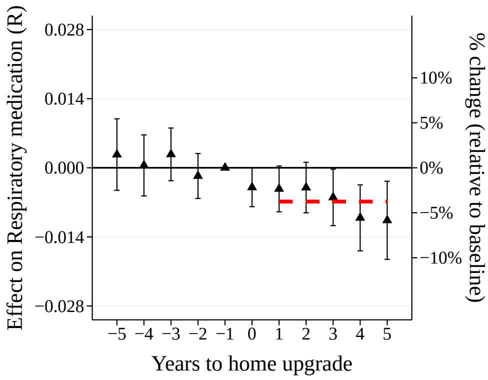
(c) Antihistamines (R06)

(d) Cough and cold preparations (R05)

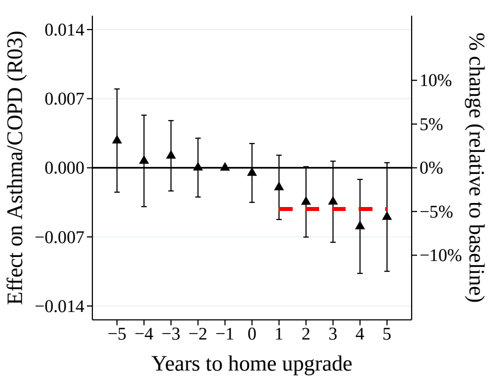
Notes: Event study estimates from 4 separate regressions. The control group comprises never-treated individuals (Sun and Abraham, 2021). The bars represent the 95% confidence interval. The percentage change is relative to the sample average of the dependent variable in the year before home upgrade. Standard errors are clustered at housing complex level. The dashed horizontal line represents the average treatment effect and the red color indicates a statistically significant effect at a 10% level.

Figure B2: Effects on respiratory health, children

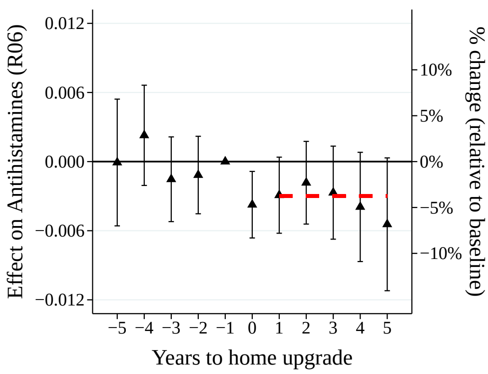
(a) Respiratory medication (R)



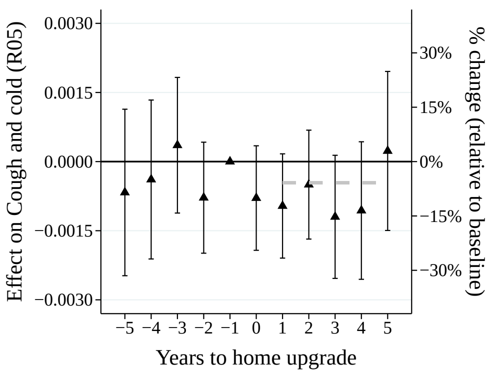
(b) Asthma/COPD (R03)



(c) Antihistamines (R06)



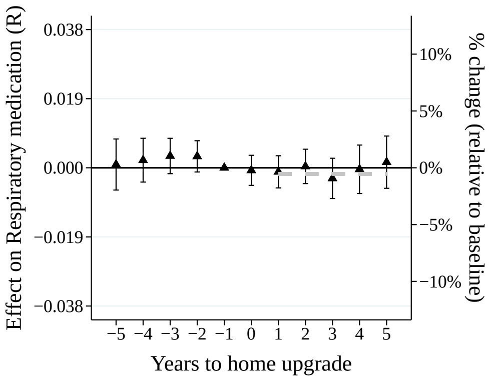
(d) Cough and cold preparations (R05)



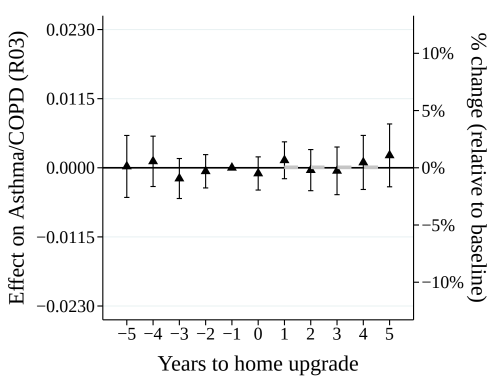
Notes: Event study estimates from 4 separate regressions. The control group comprises never-treated individuals (Sun and Abraham, 2021). The bars represent the 95% confidence interval. The percentage change is relative to the sample average of the dependent variable in the year before home upgrade. Standard errors are clustered at housing complex level. The dashed horizontal line represents the average treatment effect and the red color indicates a statistically significant effect at a 10% level.

Figure B3: Effects on respiratory health, seniors

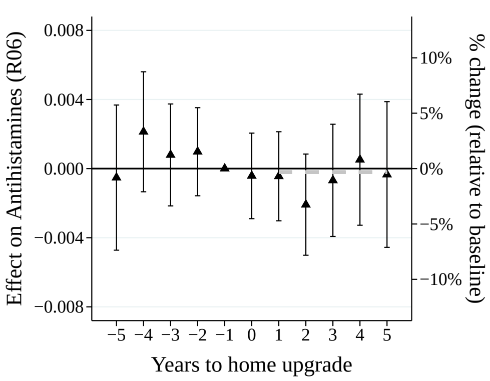
(a) Respiratory medication (R)



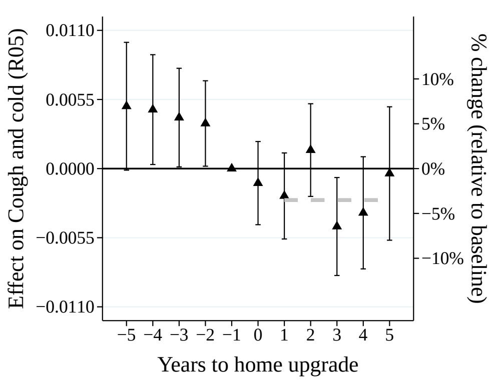
(b) Asthma/COPD (R03)



(c) Antihistamines (R06)



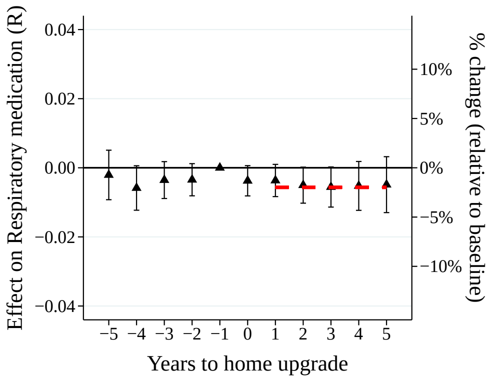
(d) Cough and cold preparations (R05)



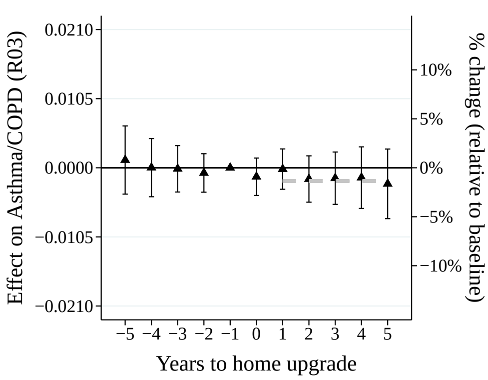
Notes: Event study estimates from 4 separate regressions. The control group comprises never-treated individuals (Sun and Abraham, 2021). The bars represent the 95% confidence interval. The percentage change is relative to the sample average of the dependent variable in the year before home upgrade. Standard errors are clustered at housing complex level. The dashed horizontal line represents the average treatment effect and the red color indicates a statistically significant effect at a 10% level.

Figure B4: Effects on respiratory health, poor

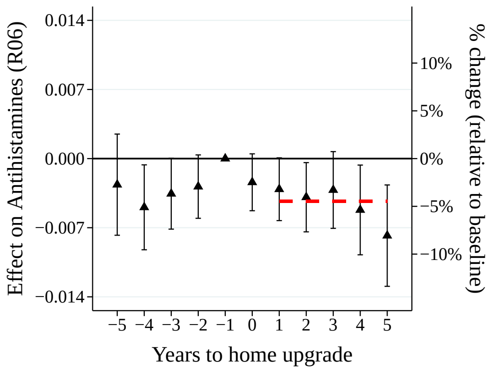
(a) Respiratory medication (R)



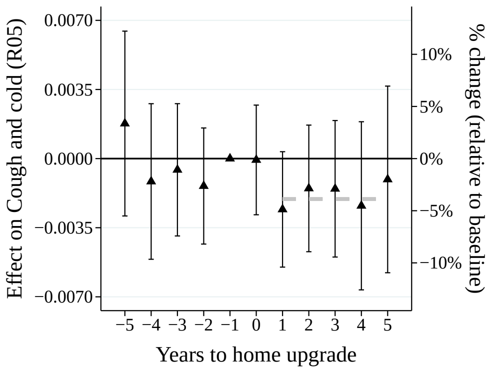
(b) Asthma/COPD (R03)



(c) Antihistamines (R06)

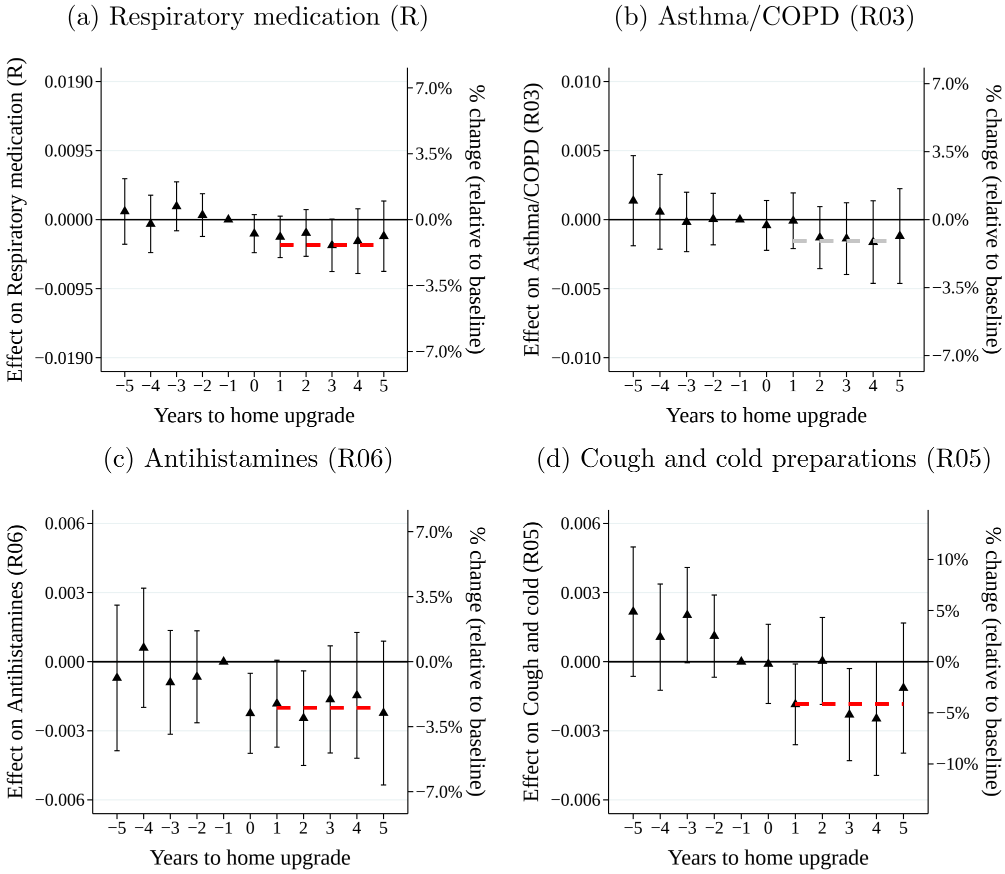


(d) Cough and cold preparations (R05)



Notes: Event study estimates from 4 separate regressions. The control group comprises never-treated individuals (Sun and Abraham, 2021). The bars represent the 95% confidence interval. The percentage change is relative to the sample average of the dependent variable in the year before home upgrade. Standard errors are clustered at housing complex level. The dashed horizontal line represents the average treatment effect and the red color indicates a statistically significant effect at a 10% level.

Figure B5: Effects on respiratory health, vulnerable



Notes: Event study estimates from 4 separate regressions. The control group comprises never-treated individuals (Sun and Abraham, 2021). The bars represent the 95% confidence interval. The percentage change is relative to the sample average of the dependent variable in the year before home upgrade. Standard errors are clustered at housing complex level. The dashed horizontal line represents the average treatment effect and the red color indicates a statistically significant effect at a 10% level.

C Appendix TWFE

This appendix presents the alternative two-way fixed effects estimates corresponding to our main findings in Table 4. We also show event study estimates corresponding to Figure B2.

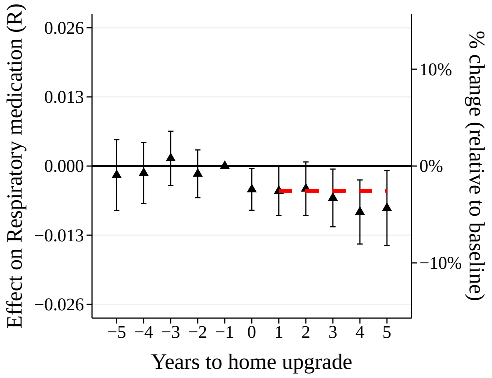
Table C1: Effects on respiratory health, TWFE

	Mean	Estimate	SE	p-value
Panel A: whole sample				
Respiratory medication (R)	0.25	-0.0017	0.0009	0.055
Asthma/COPD (R03)	0.12	0.0003	0.0006	0.606
Antihistamines (R06)	0.09	-0.0019	0.0006	0.004
Cough and cold preparations (R05)	0.04	-0.0014	0.0005	0.006
Panel B: children (age below 18)				
Respiratory medication (R)	0.18	-0.0046	0.0019	0.017
Asthma/COPD (R03)	0.09	-0.0025	0.0014	0.065
Antihistamines (R06)	0.08	-0.0027	0.0014	0.065
Cough and cold preparations (R05)	0.01	-0.0004	0.0004	0.300
Panel C: seniors (age over 65)				
Respiratory medication (R)	0.31	-0.0008	0.0016	0.612
Asthma/COPD (R03)	0.19	0.0008	0.0013	0.508
Antihistamines (R06)	0.06	-0.0018	0.0009	0.054
Cough and cold preparations (R05)	0.07	-0.0038	0.0011	0.000
Panel D: poor (income below 130% social minimum)				
Respiratory medication (R)	0.29	-0.0023	0.0015	0.143
Asthma/COPD (R03)	0.15	-0.0010	0.0011	0.348
Antihistamines (R06)	0.10	-0.0019	0.0011	0.073
Cough and cold preparations (R05)	0.05	-0.0011	0.0009	0.197
Panel E: vulnerable (child, senior or poor)				
Respiratory medication (R)	0.26	-0.0021	0.0011	0.064
Asthma/COPD (R03)	0.14	-0.0006	0.0008	0.450
Antihistamines (R06)	0.08	-0.0021	0.0008	0.006
Cough and cold preparations (R05)	0.04	-0.0019	0.0006	0.003

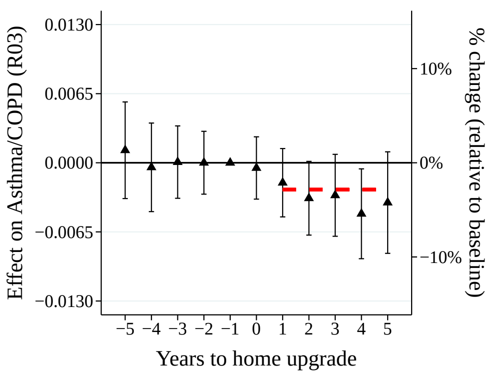
Notes: The table shows estimates of Equation (2) for 16 separate regressions. Mean is the sample mean at baseline (i.e. year before treatment). Number of observations (resp. number of unique treated individuals): 11,627K (177K). Two-way fixed effects estimates. Standard errors clustered at housing complex level.

Figure C1: Effects on respiratory health, children

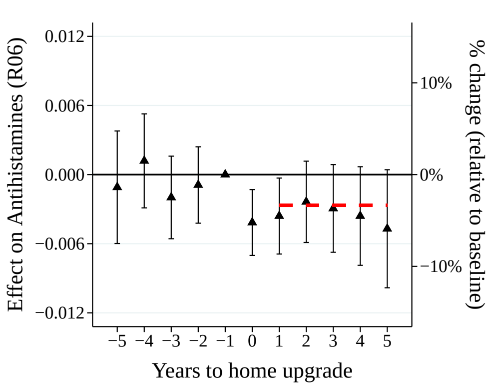
(a) Respiratory medication (R)



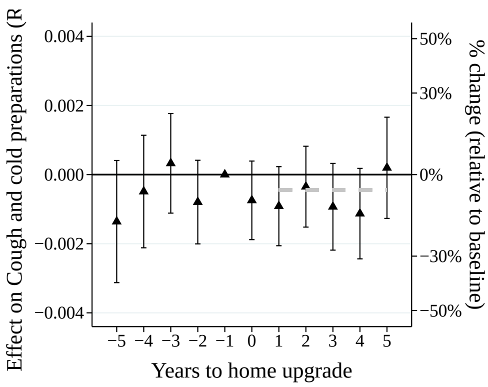
(b) Asthma/COPD (R03)



(c) Antihistamines (R06)



(d) Cough and cold preparations (R05)



Notes: Two-way fixed effect event study estimates from 4 separate regressions. The bars represent the 95% confidence interval. The percentage change is relative to the sample average of the dependent variable in the year before home upgrade. Standard errors are clustered at housing complex level. The dashed horizontal line represents the average treatment effect and the red color indicates a statistically significant effect at a 10% level.

D Appendix Callaway and Sant'Anna (2021)

This appendix presents the robustness check estimates from Callaway and Sant'Anna (2021) using the not-yet-treated as control group, providing additional support for our main findings in Table 4.

Table D1: Effects on respiratory health, not-yet-treated

	Mean	Estimate	SE	p-value
Panel A: whole sample				
Respiratory medication (R)	0.25	-0.0009	0.0076	0.905
Asthma/COPD (R03)	0.12	-0.0012	0.0057	0.830
Antihistamines (R06)	0.08	0.0008	0.0054	0.884
Cough and cold preparations (R05)	0.04	0.0004	0.0044	0.929
Panel B: children (age below 18)				
Respiratory medication (R)	0.18	-0.0050	0.0153	0.745
Asthma/COPD (R03)	0.09	-0.0032	0.0114	0.779
Antihistamines (R06)	0.08	-0.0001	0.0107	0.994
Cough and cold preparations (R05)	0.01	-0.0000	0.0041	0.992
Panel C: seniors (age over 65)				
Respiratory medication (R)	0.31	-0.0069	0.0130	0.596
Asthma/COPD (R03)	0.19	-0.0042	0.0108	0.699
Antihistamines (R06)	0.06	-0.0012	0.0072	0.871
Cough and cold preparations (R05)	0.07	-0.0027	0.0080	0.733
Panel D: poor (income below 130% social minimum)				
Respiratory medication (R)	0.28	-0.0013	0.0162	0.937
Asthma/COPD (R03)	0.15	-0.0022	0.0115	0.849
Antihistamines (R06)	0.10	0.0043	0.0096	0.652
Cough and cold preparations (R05)	0.05	-0.0015	0.0088	0.866
Panel E: vulnerable (child, senior or poor)				
Respiratory medication (R)	0.26	-0.0044	0.0100	0.661
Asthma/COPD (R03)	0.14	-0.0030	0.0086	0.730
Antihistamines (R06)	0.08	0.0009	0.0064	0.883
Cough and cold preparations (R05)	0.04	-0.0019	0.0067	0.773

Notes: The table shows estimates of Equation (1) for 20 separate regressions. Mean is the sample mean at baseline (i.e. year before treatment). Number of observations (resp. number of unique treated individuals): Panel A 1,485K (170K); Panel B 314K (36K); Panel C 293K (34K); Panel D 331K (39K); Panel E 756K (88K). The control group comprises not-yet-treated individuals (Callaway and Sant'Anna, 2021). Standard errors clustered at housing complex level.

E Appendix Long and short run effects

This appendix contains the long and short run effects that correspond our main result Table 4.

Table E1: Short run health effects (1 to 4 years)

	Mean	Estimate	SE	p-value
Panel A: whole sample				
Respiratory medication (R)	0.25	-0.0010	0.0010	0.323
Asthma/COPD (R03)	0.12	0.0005	0.0007	0.424
Antihistamines (R06)	0.09	-0.0015	0.0007	0.025
Cough and cold preparations (R05)	0.04	-0.0002	0.0006	0.741
Panel B: children (age below 18)				
Respiratory medication (R)	0.18	-0.0057	0.0023	0.012
Asthma/COPD (R03)	0.09	-0.0035	0.0017	0.035
Antihistamines (R06)	0.08	-0.0028	0.0016	0.084
Cough and cold preparations (R05)	0.01	-0.0009	0.0005	0.093
Panel C: seniors (age over 65)				
Respiratory medication (R)	0.31	-0.0010	0.0021	0.640
Asthma/COPD (R03)	0.19	0.0003	0.0015	0.843
Antihistamines (R06)	0.06	-0.0008	0.0013	0.542
Cough and cold preparations (R05)	0.07	-0.0020	0.0016	0.203
Panel D: poor (income below 130% social minimum)				
Respiratory medication (R)	0.29	-0.0048	0.0023	0.041
Asthma/COPD (R03)	0.15	-0.0012	0.0016	0.453
Antihistamines (R06)	0.10	-0.0037	0.0015	0.014
Cough and cold preparations (R05)	0.05	-0.0020	0.0014	0.160
Panel E: vulnerable (child, senior or poor)				
Respiratory medication (R)	0.26	-0.0026	0.0014	0.069
Asthma/COPD (R03)	0.14	-0.0010	0.0010	0.321
Antihistamines (R06)	0.08	-0.0019	0.0009	0.037
Cough and cold preparations (R05)	0.04	-0.0016	0.0008	0.063

Notes: The table shows estimates of Equation (1) for 20 separate regressions. Mean is the sample mean at baseline (i.e. year before treatment). Number of observations (resp. number of unique treated individuals): Panel A 11,811K (177K); Panel B 2,217K (37K); Panel C 2,410K (36K); Panel D 2,601K (40K); Panel E 5,851K (91K). The control group comprises never-treated individuals (Sun and Abraham, 2021). Standard errors clustered at housing complex level.

Table E2: Long run health effects (5 to 8 years)

	Mean	Estimate	SE	p-value
Panel A: whole sample				
Respiratory medication (R)	0.25	-0.0017	0.0018	0.361
Asthma/COPD (R03)	0.12	-0.0001	0.0012	0.956
Antihistamines (R06)	0.09	-0.0018	0.0012	0.135
Cough and cold preparations (R05)	0.04	-0.0000	0.0011	0.985
Panel B: children (age below 18)				
Respiratory medication (R)	0.18	-0.0100	0.0043	0.018
Asthma/COPD (R03)	0.09	-0.0061	0.0031	0.051
Antihistamines (R06)	0.08	-0.0037	0.0030	0.217
Cough and cold preparations (R05)	0.01	0.0008	0.0009	0.346
Panel C: seniors (age over 65)				
Respiratory medication (R)	0.31	-0.0037	0.0038	0.328
Asthma/COPD (R03)	0.19	-0.0006	0.0028	0.830
Antihistamines (R06)	0.06	0.0014	0.0021	0.499
Cough and cold preparations (R05)	0.07	-0.0040	0.0028	0.156
Panel D: poor (income below 130% social minimum)				
Respiratory medication (R)	0.29	-0.0081	0.0044	0.062
Asthma/COPD (R03)	0.15	-0.0043	0.0030	0.149
Antihistamines (R06)	0.10	-0.0060	0.0028	0.032
Cough and cold preparations (R05)	0.05	-0.0022	0.0026	0.395
Panel E: vulnerable (child, senior or poor)				
Respiratory medication (R)	0.26	-0.0059	0.0026	0.023
Asthma/COPD (R03)	0.14	-0.0030	0.0019	0.107
Antihistamines (R06)	0.08	-0.0023	0.0016	0.150
Cough and cold preparations (R05)	0.04	-0.0026	0.0015	0.087

Notes: The table shows estimates of Equation (1) for 20 separate regressions. Mean is the sample mean at baseline (i.e. year before treatment). Number of observations (resp. number of unique treated individuals): Panel A 11,811K (177K); Panel B 2,217K (37K); Panel C 2,410K (36K); Panel D 2,601K (40K); Panel E 5,851K (91K). The control group comprises never-treated individuals (Sun and Abraham, 2021). Standard errors clustered at housing complex level.

F Appendix Placebo healthcare costs

This appendix contains the estimates on a range of other healthcare costs that are unlikely to be affected by home thermal quality upgrades.

Table F1: Effects on placebo costs

	Mean	Estimate	SE	p-value
Dental care costs (euro)	62.22	-0.09	0.87	0.921
Medical devices costs (euro)	99.04	-0.69	1.07	0.520
Foreign care costs (euro)	13.64	-1.20	1.12	0.285
Sensory impairment care costs (euro)	12.54	0.45	0.76	0.562
Other GP costs (euro)	118.42	-0.45	0.45	0.313

Notes: The table shows estimates of Equation (1) for 5 separate regressions. Mean is the sample mean at baseline (i.e. year before treatment). Number of observations (resp. number of unique treated individuals): 11,760K (177K). The control group comprises never-treated individuals (Sun and Abraham, 2021). Standard errors clustered at housing complex level.