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The power of mandatory quality disclosure: Evidence from the German housing market

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Manuel Frondel, Andreas Gerster, Colin Vance

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The Power of Mandatory Quality Disclosure: Evidence from the German Housing Market

Manuel Frondel,* Andreas Gerster,† and Colin Vance‡

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Abstract: Many countries have introduced Energy Performance Certificates (EPCs) to mitigate information asymmetry problems with respect to the thermal quality of houses. Using big data on real estate advertisements that cover large parts of the German housing market, this paper empirically investigates the consequences of a shift from a voluntary to a mandatory quality disclosure regime on the offer prices of houses. Illustrated by a stylized theoretical model, we test the following key hypothesis: Prices for houses whose owners would not voluntarily disclose their house's energy consumption in real estate advertisements should decrease upon a shift to a mandatory disclosure scheme. Employing an instrumental variable approach to cope with the endogeneity of disclosure decisions, our analysis demonstrates the relative advantage of mandatory over voluntary disclosure rules.

JEL classification: D82, L15, Q58

Keywords: Information asymmetry, mandatory disclosure, environmental certification.

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1. Introduction

Ever since Akerlof's (1970) seminal contribution on markets for lemons, it is widely recognized that information asymmetries are pervasive. For example, purchasers of used cars typically know less about the car quality than the sellers. The purchase of used houses is another particularly relevant example, as the financial consequences are large and learning effects are limited due to the typically small number of purchases over an individual's lifetime.

*RWI, Ruhr University Bochum; Hohenzollernstr. 1–3, 45128 Essen, Germany. E-mail: frondel@rwi-essen.de.
†RWI, Ruhr Graduate School in Economics; Hohenzollernstr. 1–3, 45128 Essen, Germany. Tel.: +49 201 8149 521. E-mail: andreas.gerster@rwi-essen.de.
‡RWI, Jacobs University Bremen; Hohenzollernstr. 1–3, 45128 Essen, Germany. E-mail: vance@rwi-essen.de.

To mitigate information asymmetry problems with respect to the thermal quality of houses, many countries have introduced Energy Performance Certificates (EPCs), with labeling schemes ranging from voluntary certification, such as the U.S. Energy Star program, to mandatory schemes that prescribe the disclosure of thermal qualities for all buildings to be sold. For example, as of May 1, 2014, Germany's legislation obliges sellers to disclose a building's energy consumption per square meter in real estate advertisements and to always present the EPC to potential buyers, not just on demand. Prior to May 2014, the German legislation just implied the weak obligation "to make available an EPC [...] as soon as a potential buyer asks for it" (EnEV, 2007), yet the absence of an EPC did not carry legal sanctions. To enforce compliance, heavy fines for non-compliance were introduced, ranging up to 15 thousand euros (EnEV, 2014). With these legal changes, the nature of the labeling scheme has shifted substantially, from an effectively voluntary disclosure of energy information originally to enforcing disclosure today.

Using big data on real estate advertisements that cover large parts of the German housing market and the years 2013 and 2014, this paper empirically investigates the consequences of such a shift in legislation on the offer prices of houses. Motivated by a stylized theoretical model, we test the following three hypotheses: first, under a voluntary disclosure regime, the energy consumption per square meter is disclosed in real estate advertisements more often for houses fulfilling high energy-efficiency standards than for those of low energy quality. Second, offer prices for houses whose owners would not voluntarily disclose the energy consumption should decrease upon a shift from a voluntary to mandatory disclosure scheme. Third, the drop in prices should be correlated with energy quality, that is, sellers whose homes have the worst energy qualities will lower their offer prices the most upon such a shift. Employing an instrumental variable approach to cope with the endogeneity of disclosure decisions, our analysis demonstrates the relative advantage of mandatory over voluntary disclosure rules for the residential sector.

Our first hypothesis accords with early theoretical work stressing the potential of voluntary disclosure for sellers of good-quality products to escape the pooling with bad qualities, thereby possibly achieving higher selling prices (Milgrom, 1981; Grossman, 1981). In fact, there is ample empirical evidence that sellers successfully employ voluntary disclosure as an instrument to overcome adverse selection problems in numerous fields as diverse as food qualities (Ippolito and Mathios, 1990) and online auctions of used cars (Lewis, 2011).

That the best-quality seller is the first to disclose as a means to distinguish from low-quality sellers can trigger an iterative process called "market unraveling ", in which further sellers are induced to disclose information on their product as well, whereas low-quality sellers would prefer hiding in a pool of high-quality sellers. In theory, all but the worst-quality seller discloses, an outcome that is called the "unraveling result" and hinges on several strong assumptions, such as costless disclosure (Dranove and Jin, 2010).

In reality, however, there are many markets in which voluntary disclosure is incomplete (Dranove and Jin, 2010), casting doubt on the efficacy of voluntary disclosure to overcome information asymmetries. In contrast, mandatory rules to disclose quality have been found to improve consumers' welfare in a wide variety of applications, including schooling (Figlio and Lucas, 2004) and health plan report cards (Beaulieu, 2002; Bundorf et al., 2009; Dafny and Dranove, 2008; Jin and Sorensen, 2006; Scanlon et al., 2002).

Studies that investigate the shift from a voluntary to a mandatory disclosure regime are scarce, though. Exploring the impacts of labeling on the fat content of salad dressings, one of the few such studies is Mathios (2000). This author finds that under a voluntary regime, first, the producers of high-fat dressings did not disclose information on the fat content and, second, the sales of high-fat dressings dropped after introducing mandatory disclosure rules.

With respect to energy labels, previous research has largely focused on the market value of energy efficiency. Eichholtz et al. (2010), for example, present empirical evidence that the U. S. Energy Star label is associated with an average price premium of some 16% for office buildings, while for the EU label, Brounen and Kok (2011) and Hyland et al. (2013) analyze the extent to which different energy efficiency classifications of buildings are capitalized into prices. Yet, to our knowledge, empirical evidence on the effect of mandatory disclosure rules for energy information on the offer prices posted by home sellers in real estate advertisements is unavailable so far.

Our study contributes to the literature on information disclosure in at least two respects: First, given that both theoretical and empirical articles stress that the extent of market unraveling under voluntary disclosure rules is context-specific and depends on a confluence of factors, such as the market structure, the kind of quality differentiation, and consumer preferences (Dranove and Jin, 2010), we are the first to analyze the potential of voluntary disclosure rules to overcome information asymmetries in the housing market with respect to energy quality.

Second, we analyze whether it is desirable to go beyond voluntary information disclosure by invoking mandatory disclosure rules. Many theoretical articles argue that mandatory disclosure can raise consumer surplus, but this argument has largely escaped empirical scrutiny. By finding that mandatory disclosure rules trigger price reductions for homes of otherwise non-disclosing sellers, our analysis demonstrates the relative advantage of mandating information disclosure for consumers.

In the subsequent section, we present a stylized model based on game theory to derive theoretical predictions about the difference in house prices between voluntary and mandatory disclosure. Drawing on data from Germany's largest online platform for real estate advertisements, Section 3 describes the data set and presents summary statistics, followed by a discussion of our empirical strategy in Section 4. In Sections 5 and 6, we present the estimation results and robustness checks. The last section summarizes and concludes.

2. Theoretical Model

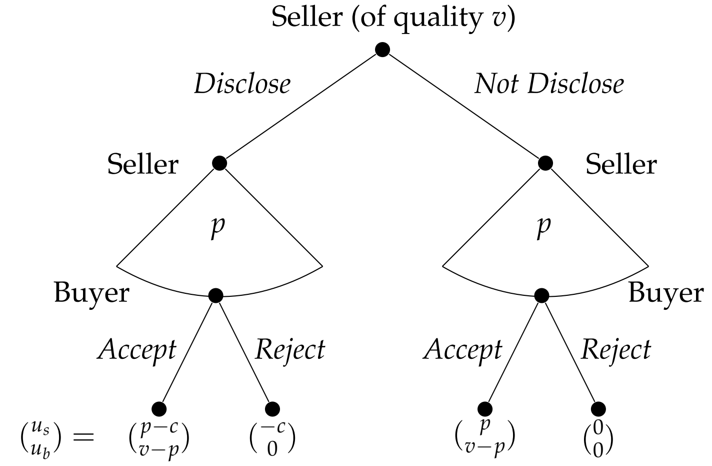
In this section, we propose a stylized game-theoretic model of house purchases under asymmetric information that serves to guide our empirical approach by illustrating the three testable hypotheses presented in the introduction. Inspired by the early work of Milgrom (1981) on voluntary disclosure, we extend his model by adding two features: limited rationality of buyers and disclosure cost. As a result, "market unraveling" is incomplete, as is typically observed in practice.[1]

The structure of the model, as well as its outcomes, are briefly explained here, with the details being presented in Appendix A. Let a seller possess a house with quality v, which, for the sake of simplicity, is assumed to be uniformly distributed on the unit interval $[0, 1]$. Quality v is purely private information of the seller, whereas potential buyers are assumed to be only able to observe the distribution of qualities in the population, given here by density $f(v) = 1$ for $v \in [0, 1]$. When posting a real estate advertisement, the seller can decide to either disclose quality at costs of c or to abstain from disclosure (Figure 1). Upon deciding on disclosure, the seller proposes a house price p, which a potential buyer can either accept or reject.

In case of a buyer's acceptance, the home is sold and the seller's pay-off is determined by

[1] According to Dranove and Jin (2010), three potential explanations for incomplete "market unraveling" have received the most attention in the literature: limited rationality of buyers, positive disclosure costs, and strategic interactions between sellers. As opposed to limited rationality and disclosure costs, strategic interaction between market agents is generally not an issue in our empirical example, as a vast number of sellers participate in Germany's real estate market, making strong interactions between market agents unrealistic.

Figure 1: A Model of House Purchases under Imperfect Quality Information



$p - c$, the house price minus disclosure costs, whereas the buyer's pay-off equals $v - p$, the value of the house, reflected by quality v, less its price. If instead the transaction does not take place, the seller's pay-off is vanishing or even negative ($-c$) if the seller opted for quality disclosure.[2]

Following the concept of "cursed equilibrium" introduced by Eyster and Rabin (2005) to operationalize limited rationality of buyers, we employ parameter $\chi \in [0, 1]$ to capture the extent to which the buyer makes rational inferences on qualities. A fully naive buyer, indicated by $\chi = 1$, does not recognize that sellers of low-quality houses have particularly strong incentives to refuse disclosure and thus wrongly believes that the quality of a house for which no energy information is disclosed equals the population average. In contrast, relying on Bayesian updating, a fully rational buyer ($\chi = 0$) forms rational beliefs over qualities and takes the seller's strategic considerations into account.[3]

Under a mandatory regime, all sellers are forced to disclose and the equilibrium price p reflects house quality v: $p = v$ (Table 1). If instead disclosure is voluntary, "unraveling" occurs to some extent: a seller of a house exhibiting quality v that exceeds threshold value v^*, which discriminates between disclosure and non-disclosure, voluntarily opts for disclosure and sets the price p equal to quality v. Due to positive disclosure costs, $c > 0$, and limited rationality of buyers, however, "unraveling" remains incomplete, indicated in the model by a non-vanishing

[2]To focus on the main mechanisms behind disclosure and retain the tractability of the model, we employ a straightforward setting of asymmetric information in which the outside option of the seller to not sell the house is not a function of quality v.

[3]Behavioral failures that imply a buyer's inability to correctly interpret a seller's strategic behavior with regard to the implications that they have on unobserved qualities ("conditional failures") have been shown to be relevant in both the laboratory (Jin et al., 2015) and using observational data on movie openings with and without previous reviews (Brown et al., 2012).

disclosure threshold $v^* > 0$. Note that the well-known "unraveling result" of the literature, which – among other strong assumptions – is predicated on the notion that all agents are fully rational and disclosure costs are vanishing, is included in our model as a special case: for $\chi = 0$ and $c = 0$, we obtain $v^* = 0$ (Table 1) and, hence, complete unraveling. It also bears noting that the case of vanishing disclosure cost $c = 0$ is highly relevant for our empirical example, as disclosure cost for online real estate advertisements are almost negligible relative to house prices: $c \approx 0$.

Table 1: Price Predictions under Voluntary and Mandatory Disclosure Regimes

Disclosure Regime	Equilibrium Price	Who discloses?
Mandatory	v	Ideally, everybody
Voluntary	$\begin{cases} v & \text{if } v \geq v^* \\ \bar{v} = \frac{\chi}{2} + \frac{(1-\chi)v^*}{2} & \text{if } v < v^* \end{cases}$	House Owners with $v \geq v^* = \frac{2c+\chi}{1+\chi}$

Moreover, note that the price predictions resulting from Table 1 for the two polar cases $\chi = 0$ and $\chi = 1$ are highly intuitive: while a completely naive buyer ($\chi = 1$) expects the unknown house quality v to equal the expected value of the uniform quality distribution: $v = \bar{v} = 1/2$, rational sellers of houses with qualities v that equate or exceed threshold $v^* = 1/2 + c$ would disclose quality, as they can realize a price $p = v \geq v^* = 1/2 + c$, where disclosure threshold v^* equals the price $1/2$ that would be accepted by a naive buyer plus the disclosure cost c.

On the other hand, a fully rational buyer ($\chi = 0$) would expect the average quality in the pool of houses for which no quality information is disclosed to equal $\bar{v} = \frac{v^*}{2}$, as this is the expected quality in the pool of low-quality houses given the underlying uniform distribution of house qualities. With $v^* = 2c$, the selling price at the margin is given by $p = \bar{v} = \frac{2c}{2} = c$, implying that only the owners of houses of very low qualities ($v < c$) would refuse to disclose.

It seems plausible that disclosure threshold v^* increases with c, as larger costs of disclosure induce sellers of better qualities to decide against disclosure and to accept the pooling with worse qualities. In a similar vein, v^* increases with the naivety χ of a buyer, which implies that the buyer does not sufficiently account for quality and allows a seller of the pool of houses without quality information to achieve a price that is higher than the reference price set by the house quality v.

This outcome is precisely what our model predicts: all houses for which no quality information is provided are sold at a price $p = \bar{v}$ that can be higher than the house quality v. This prediction can be seen for the special case of vanishing disclosure cost $c = 0$ as follows: a low-quality seller ($v < v^*$) decides against disclosure and sets the price p to what the buyer considers as the average quality in the pool of houses for which no quality information is disclosed (Table 1 and Appendix A):

$$p = \bar{v} = \frac{\chi}{2} + (1 - \chi)\frac{v^*}{2}. \tag{1}$$

Inserting $v^* := \chi/(1+\chi)$ into (1) yields $p = \bar{v} = \chi/(1+\chi) = v^*$ and, hence, price $p = v^*$ is higher than house quality v, as in the case of a low-quality seller it is $v < v^*$. Our model therefore rationalizes our key hypothesis that a shift from voluntary to mandatory disclosure regime should imply a decrease in offer prices for houses whose owners chose to not disclose under the voluntary regime.

3. Data

Drawing on data from Immobilienscout24, Germany's largest online platform for real estate advertisements, we focus on the market for used houses, thereby ignoring both newly built houses as well as unbuilt houses that are already offered on the platform. Our focus owes to the fact that in markets for used goods, information asymmetries are particularly problematic. Our data set captures houses that were for sale in 2013 and 2014 and contains detailed information on housing characteristics, the energy consumption per square meter for those advertisements in which it is disclosed, as well as the last offer price before the advertisement was closed.

As our primary interest is on systematic differences between houses for which energy information is disclosed in the advertisement and those without energy information, Table 2 compares the summary statistics for both sub-samples. While for the majority of characteristics, there are small differences in the means, a quite substantial discrepancy can be observed for house prices. The average price for houses for which energy information is disclosed is about 7% higher than for the remaining houses, although mean living space is virtually identical and the mean lot size is even somewhat larger for those houses without energy information. Another notable distinction is that the shares of seller types, i. e. banks, real agents and private sellers, differ across offers of distinct energy information status. For instance, with 33%, the share of banks is larger among those advertisements including energy information than among those without it.

This distinction and, in particular, the fact that banks and real estate agents respond dif-

Table 2: Summary Statistics

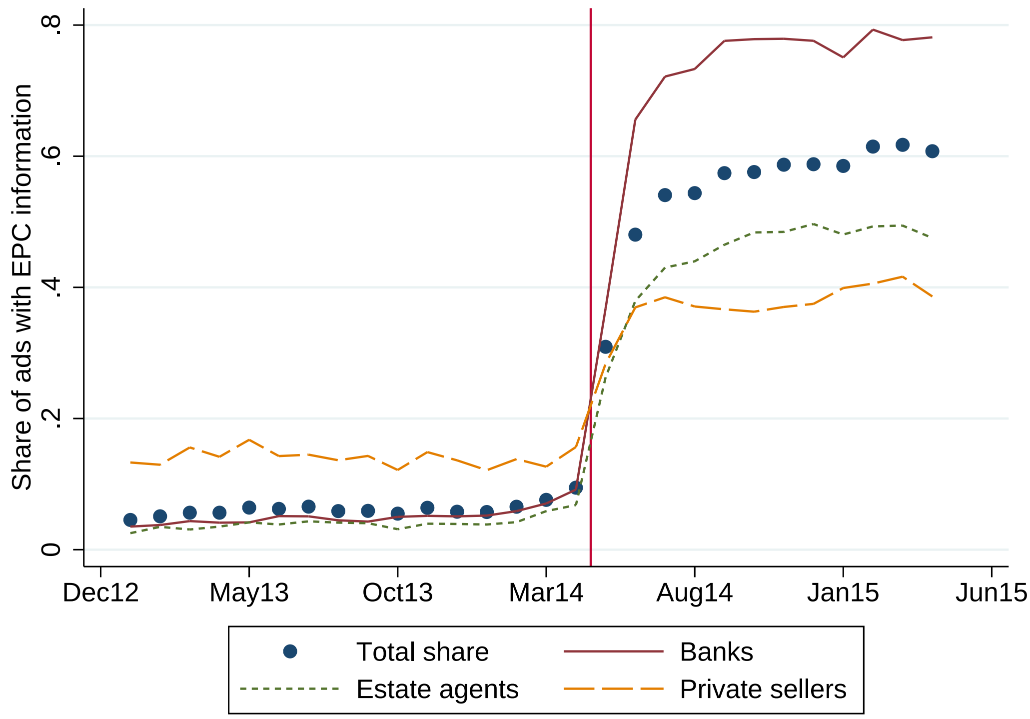
	Ads with energy information			Ads without energy information		
	Mean	**Std. Dev.**	**Numb. of Obs.**	**Mean**	**Std. Dev.**	**Numb. of Obs.**
Price, in 1,000 EUR	279.2	160.5	103,816	237.6	155.1	251,449
Living space, in m^2	169.0	68.3	103,816	170.9	74.5	251,449
Lot size, in m^2	733.0	650.1	103,566	784.0	739.9	235,996
# of rooms	6.1	2.5	103,816	6.2	2.8	251,449
Year of construction	1971	27	97,250	1967	30	203,186
Years since last modernization	2006	8	29,978	2005	8	45,631
House category:						
Multi-family house	0.15	0.36	95,496	0.16	0.37	198,073
One-family house	0.54	0.50	95,496	0.56	0.50	198,073
Row house	0.31	0.46	95,496	0.28	0.45	198,073
Seller type:						
Banks	0.33	0.47	103,816	0.22	0.42	251,449
Estate agents	0.60	0.49	103,816	0.69	0.46	251,449
Private	0.07	0.25	103,816	0.09	0.28	251,449
Self-rated house condition:						
Normal	0.53	0.5	36,694	0.57	0.49	71,014
Superior	0.47	0.5	36,694	0.43	0.49	71,014
Annual energy consumption (kWh/m^2)	162.0	75.9	103,816	–	–	–

Source: Immobilienscout24, own calculations.

ferently to changes in the disclosure rules than private sellers, is exploited in our empirical strategy to identify the causal effect of disclosing energy information on house prices. In fact, as Figure 2 reveals, after April 2014, the share of sales advertisements that include energy information was substantially larger for banks and real estate agents than for private sellers, whereas prior to May 2014, the opposite was true. Overall, the share of advertisements that displayed information on energy efficiency increased substantially, from some 10% prior to May 2014 to about 60% at the end of 2014, indicating that compliance is imperfect.

The heterogeneity in the degree of compliance across seller types appears to be plausible given the different institutional characteristics of these groups (for a detailed discussion of seller type differences, see Appendix B). Due to the costs associated with non-disclosure, most notably, image and reputation losses, it must be in the interest of banks and real estate agencies that their employees comply with the legal rules. This endeavor is frequently supported in banks by compliance departments. In addition, for bank employees, awareness about law changes and their consequences can be expected to be high. In contrast, private sellers are likely to be much less informed about legal obligations, such as the disclosure of energy information in real estate advertisements, than bank employees and real estate agents.

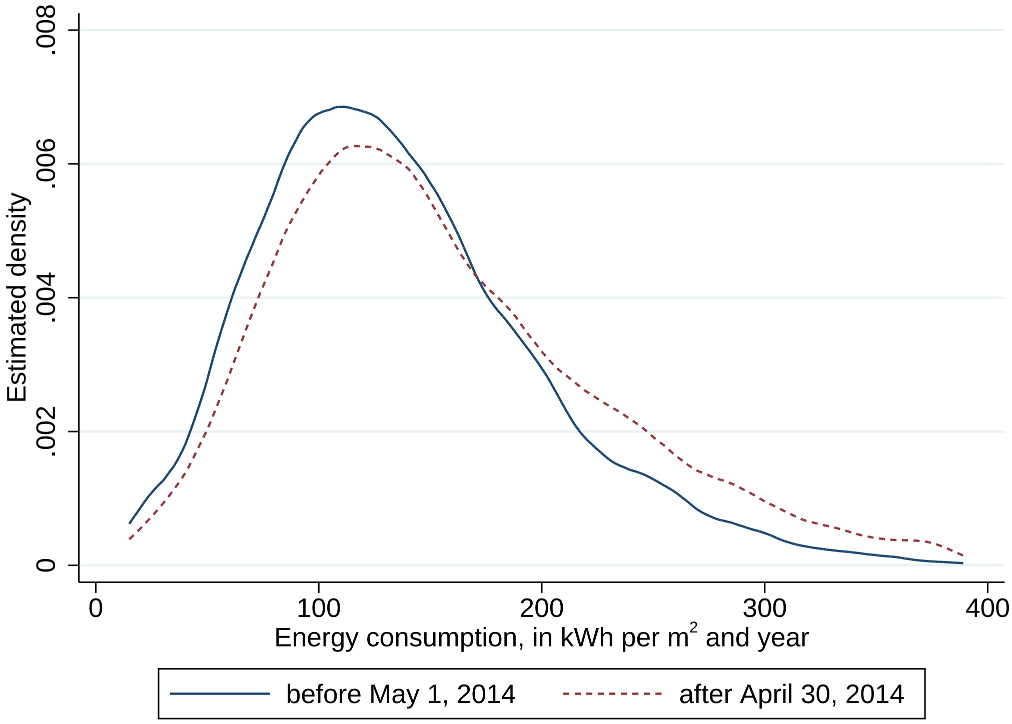
Figure 2: Shares of Advertisements in Immobilienscout24 including Energy Information across Seller Types.



In sum, on the basis of this discussion, it seems to be highly warranted to assume that the change in disclosure rules as of May 2014 created exogenous variation in the provision of energy information in real estate advertisements across sellers types. In the subsequent section, the heterogeneous response of various seller types is exploited to instrument the likely endogenous choice of disclosing energy information. After all, it is likely that energy information is more often included in real estate advertisements for buildings fulfilling high energy-efficiency standards than for houses of low energy quality.

To provide empirical support for our assumption that the disclosure decision is often strategic in nature, in other words, endogenous, we plot the distribution of the energy consumption per square meter before May 1, 2014, and after April 30, 2014, expecting that this distribution would shift to the right, as the share of houses with low thermal qualities among those whose consumption value is disclosed would increase as a consequence of the change in legal rules that mandates quality disclosure. Our expectation of a shift to the right is confirmed by Figure 3. In contrast, the distribution should have remained unaltered if the disclosure of energy consumption values would have been a matter of chance or sellers' inattention, rather than strategic decisions.

Figure 3: Distribution of Energy Consumption per m^2 before May 1, 2014, and after April 30, 2014.



4. Methodological Issues

As our goal is to estimate the impact of a seller's potentially endogenous choice to disclose information on a building's energy consumption on offer prices, disclosure is the binary treatment variable of interest. Following the framework of Rubin (1974), the potential offer price for house i is denoted by $p_i(1)$ if energy information is disclosed and by $p_i(0)$ if the energy information is not disclosed. For an individual house i, the causal effect of disclosure is given by the difference between potential outcomes: $p_i(1) - p_i(0)$. This difference, however, is principally unobservable, as we either observe $p_i(1)$ or $p_i(0)$, yet not both, giving rise to the well-known evaluation problem (Frondel and Schmidt, 2006).

As researchers are commonly interested in average impacts, rather than in individual effects, the empirical literature typically focuses on the average treatment effect (ATE): $E(p_i(1)) - E(p_i(0))$, where $E(.)$ denotes the expectation operator. To estimate the ATE, we exploit the difference in the response across seller types before and after the regulatory change in May 2014 by specifying instrumental variables Z_i that exogenously determine the seller's choice to disclose the energy quality of house i in the real estate ad. In detail, we employ $Banks_i \cdot PostApril$ and $Agents_i \cdot PostApril_i$ as instruments, where $Banks_i$ and $Agents_i$ are dummy variables that designate the seller type of house i and $PostApril_i$ represents a dummy variable that equals 1 if the time of sale of house i is after the introduction of mandatory disclosure rules on May 1, 2014, and 0 otherwise. Using variation at the seller type-time level as instruments, our empirical strategy corresponds to an instrumental variable differences-in-differences approach akin to that pursued by, e. g. Burgess and Pande (2005), Field (2007) and Lochner and Moretti (2004).

Under the identification assumptions discussed below, Imbens and Angrist (1994) show that an instrumental variable estimator identifies the local average treatment effect (LATE).[4] This corresponds to the average treatment effect for a subgroup of individuals that are induced by the binary instrument to change the treatment status and are accordingly referred to as "compliers" (Imbens and Angrist, 1994), where the treatment status is designated by the change in residential energy disclosure from a voluntary to a mandatory regime in May of 2014. In our case, "compliers" refer to sellers that are induced by the policy change to disclose energy information and would not have done so otherwise.[5]

The LATE is of particular interest in our analysis. In the second hypothesis, we anticipate that sellers who do not disclose energy information under a voluntary disclosure scheme would lower their offer price when disclosure becomes mandatory. As the LATE corresponds to the treatment effect for sellers who disclosed energy information only as soon as they had to, it allows us to explicitly test this hypothesis.

Identification of the LATE using an instrumental variable approach hinges on several identifying assumptions (Imbens and Angrist, 1994), the first being that the instrument is correlated with the treatment, but uncorrelated with potential outcomes. While the correlation between the instrument and the treatment variable can easily be assessed, the requirement that instrumental variables are unrelated to potential outcomes (the "exclusion restriction") is in principle untestable. In the following, we argue why we consider the exclusion restriction to be plausible for the instrumental variables that we use.

As our instrumental variables are interactions between seller types and a post-treatment dummy, time-constant variables that may be correlated with seller types and potential outcomes are not of concern, similar to traditional differences-in-differences settings that also allow for "constant bias", i.e. selection into treatment that leads to differences in unobservables,

[4]Strictly speaking, using multiple instruments and estimating a two-stage least squares (2SLS) model, as we do, yields an estimate for a weighted sum of both LATEs (Angrist and Pischke, 2008).

[5]"Compliers" for the $Banks_i * PostApril$-instrument correspond to banks that sold a house after the policy change and disclosed energy information only because it became mandatory. Similarly, "compliers" for the $Agents_i * PostApril$-instrument correspond to estate agents that fulfill the same criteria.

as long as they are time-constant (Angrist and Pischke, 2008). Therefore, threats to the validity of the instrument can only arise if potential outcomes for the seller types do not follow common trends.

One might, for example, be concerned that the composition of housing types as well as the geographical location of houses evolves differently over time for the seller types, which might cause a violation of common trends. Recognizing that our pooled data comprises cross-sections of homes observed over different points in time, we address this concern by conditioning on zip code fixed effects as well as on housing characteristics, such as the housing type, the age of the house, its size and the number of rooms. Furthermore, we include time-of-sale fixed effects to capture the evolution of general price levels of the German housing market. Accordingly, we compare houses that are located in the same local housing market, have the same observable characteristics and are sold in the same month of the year. We argue that conditioning on these variables makes a common-trend type assumption very plausible: it is not apparent why the price of houses that are very similar in terms of characteristics and location should evolve differently, especially when comparing sales of estate agents and banks that use very similar selling strategies. We undertake robustness checks using placebo regressions to gauge the validity of this assertion.

Another concern might be that seller types change the pricing of real estate differently in response to the mandatory disclosure rules, which might induce different trends. However, there is no evidence that this is an empirically relevant phenomenon. Especially for banks and estate agents, there is no specific reason to assume that only one of them adjusted the valuation formulas.

Besides validity of the instrument, "monotonicity" (Imbens and Angrist, 1994) is required to identify the LATE, which implies that the instrument can only influence treatment uptake in the same way, so that the instrument should not simultaneously increase treatment probabilities for some sellers and decrease them for others. In our case, this means that the legal obligation to disclose energy information should not have decreased the disclosure probabilities for some sellers – which does not seem to be a critical assumption.

Finally, identification requires the stable-unit-treatment-value assumption (SUTVA), implying that the treatment solely exerts a direct effect on the unit being treated, thereby excluding indirect effects. Specifically, SUTVA rules out the existence of general equilibrium effects and treatment externalities. With respect to our empirical example, one might argue that disclosure of energy information in an advertisement may affect other house sales by, for instance, increasing the attention of buyers to energy efficiency attributes. While we cannot deny the possibility of such spill-over effects, we argue that for two reasons it is highly unlikely that they are of critical magnitude. First, compliance with the new disclosure rules is far from being perfect, rendering substantial shares of non-compliers. Second, taking the weak response of private sellers as a benchmark, the awareness of potential buyers about new disclosure rules seems limited.

To empirically investigate the determinants of the decision on whether to disclose the energy consumption per square meter in a real estate ad, captured here by the binary variable EPC_i, with $EPC_i = 1$ indicating the disclosure of the energy consumption value that originates from an EPC, we estimate the following linear probability model:

$$\begin{aligned} EPC_i &= \alpha_0 + \boldsymbol{\alpha}_{\mathbf{x}}^T \mathbf{x}_i + \alpha_{PostApril} PostApril + \alpha_{Bank} Bank_i + \alpha_{Agent} Agent_i \\ &\quad + \alpha_{BankPostApril} Bank_i \cdot PostApril + \alpha_{AgentPostApril} Agent_i \cdot PostApril + \mu_z + \epsilon_i, \quad (2) \end{aligned}$$

where $\mathbf{x}_i$ captures a wide range of housing characteristics, μ_z denotes zip-code fixed-effects and ϵ_i is the error term. $PostApril$ is a dummy variable designating that advertisement i appeared in Immobilienscout24 after April 30, 2014, $Bank_i = 1$ and $Agent_i = 1$ indicate whether advertisement i either owes to a bank or a real estate agent, respectively, with private sellers representing the reference group in Equation 2. Motivated by Figure 2 and the discussion in this section, we have included interaction terms to capture seller-type-specific differences in the disclosure of energy consumption values after April 2014.

To explain house prices p, the following instrumental variable (IV) approach is pursued in which we employ $Banks_i \cdot PostApril$ and $Agents_i \cdot PostApril$ as instruments for the likely endogenous energy information disclosure decision indicated by EPC_i:

$$\begin{aligned} \ln p_i &= \beta_0 + \boldsymbol{\beta}_{\mathbf{x}}^T \mathbf{x}_i + \beta_{Bank} Bank_i + \beta_{Agent} Agent_i \\ &\quad + \beta_{EPC} EPC_i + \nu_t + \mu_z + u_i, \quad (3) \end{aligned}$$

where ν_t designates time fixed effects referring to the month of the appearance of real estate ad i, u_i denotes the error term and living space per square meter and lot size are two important housing characteristics that are included in vector $\mathbf{x}$.

A potential pitfall of estimating linear models is that differences in covariate distributions across groups can decrease the robustness of the results. This is due to the assumption of a linear functional form that leads to particularly pronounced extrapolations if covariates vary largely by group (see e.g. Imbens, 2014). Reflecting our identification strategy that uses variation at the seller type-month level, we aim at assuring balance of covariates across groups defined by seller type and month. To this end, we employ propensity score matching techniques proposed by Rubin (2006). Taking houses offered by banks in December 2013 as a baseline, we repeatedly employ propensity score matching to find similar houses offered in another month or by another seller. By only keeping "nearest neighbors" in terms of the propensity score, i. e. houses that are very similar to those offered by banks in December 2013, this procedure leads to covariate distributions that are balanced both over months and over seller types.[6] We use this balanced sample of matched observations for the regressions reported below.

5. Empirical Results

The results of the ordinary least squares (OLS) estimation of Equation 2, which corresponds to the first stage of our instrumental variable approach, reveal that information on energy efficiency is disclosed more often for more recently constructed houses than for older buildings (Table 3), with the propensity to disclose energy consumption values being highest for houses constructed between 2008 and 2014. Although partly driven by the fact that EPCs are less readily available for older houses than for more recently constructed homes, this finding may also reflect strategic behavior to obscure poor thermal quality. In a similar vein, it is not surprising that the disclosure of consumption values is most likely for houses that are recently renovated. Disclosure is also frequent for those houses that are self-rated as being of superior, rather than of normal condition. These results lend support for our first hypothesis that owners of houses with better thermal qualities are more likely to disclose energy information than those of low-quality houses.

Confirming the conclusions drawn from Figure 2, our estimation results indicate that the obligation to disclose energy consumption values as of May 2014 had a substantial impact, with the share of advertisements including this information rising by some 26 percentage

[6] The estimation of propensity scores is based on the housing characteristics $\mathbf{x}$, as well as on variables at the zip-code level that describe the local housing market, such as the population density, per capita income, the share of adults and the share of commercial buildings. As the number of house sales is typically lowest in December, we use this month as the baseline for the nearest neighbor matches.

points after April 2014 for private sellers. Even more pronounced is the reaction of other seller types: conditional on zip-code fixed-effects and other covariates, the disclosure rate of estate agents is some 15 percentage points higher than for private sellers, with the disclosure rate of banks even exceeding that of private sellers by nearly 40 percentage points.

On the basis of Equation 2, we are able to gauge the strength of our instruments using Stock et al.'s (2002) rule of thumb to detect weak instruments. This rule requires that the F statistic for the coefficients $\alpha_{BankPostApril}$ and $\alpha_{AgentsPostApril}$ of the instruments exceed the threshold of 10. With an F statistic of 1,815 resulting from the first-stage estimation, we can clearly reject the hypothesis that the second-stage equation is only weakly identified.

To provide for a reference point for our IV estimates, we first estimate specification (3) by using OLS, thereby ignoring the potential endogeneity of disclosure. The OLS estimates reported in Table 4 suggest that the average treatment effect of disclosure is slightly positive. This result confirms our conjecture that the disclosure decision is endogenous, rather than being purely random, as the owner of a house with a good thermal quality is likely to disclose energy information more often than other market agents. The OLS estimate on the coefficient of EPC_i is thus likely to be biased upwards as a consequence of unobserved thermal quality being part of the error term, ultimately resulting in an omitted-variable bias.

Contrary to the OLS estimate, the IV estimation result for the coefficient on EPC presented in the second column of Table 4 has a negative sign and reflects the local average treatment effect (LATE), indicating that upon the disclosure of energy efficiency information house prices decrease for the subgroup of compliers, i. e. for owner of houses that have only disclosed the information in response to the mandatory disclosure rule – a finding that is in accord with our second hypothesis.

Using information on the year of construction, we now split the sample according to those years in which building codes were either introduced or tightened in Germany. The LATE estimates reported in Table 4 indicate a statistically significant price reduction of -4.4% for houses that were built before 1977, the year in which the first building codes went into effect. In contrast, we cannot detect any statistically significant effect for houses that were built after 1977. Under the reasonable assumption that older houses tend to exhibit low thermal qualities, these findings confirm our third hypothesis that predicts low-quality homes to experience the most pronounced price reductions in response to the introduction of mandatory disclosure rules.

Table 3: OLS Regression Results for Linear Probability Model 2 of the Determinants of Energy Consumption Disclosure in Real Estate Advertisements

	Coeff. s	Std. Errors
Year of modernization (base category: pre 1977)		
1977-1989	0.025	0.020
1990-2001	0.048**	0.018
2002-2007	0.045*	0.018
2008-2014	0.056**	0.018
n.a.	0.011	0.018
Year of construction (base category: pre 1930)		
1930-1959	0.023**	0.004
1960-76	0.038**	0.003
1977-89	0.059**	0.004
1990-2001	0.050**	0.004
2002-2007	0.055**	0.005
2008-2014	0.119**	0.008
n.a.	-0.064**	0.004
Self-rated house condition (base category: normal)		
superior	0.035**	0.004
n.a.	0.006	0.003
House category (base category: row house)		
Multi-family house	-0.001	0.004
One-family house	-0.004	0.002
n.a.	-0.059**	0.004
Seller type (base category: private sellers)		
Banks	-0.003	0.004
Agents	-0.017**	0.004
Appearance after April 30, 2014		
PostApril	0.258**	0.004
Interactions terms		
Banks*PostApril	0.397**	0.005
Agents*PostApril	0.154**	0.005
Further controls		
Zip code fixed-effects	✓	
Further house characteristics	✓	
Number of observations:	165,056	

Note: ** and * denote statistical significance at the 1% and 5% level, respectively. Further house characteristics include lot size, living space, number of rooms, heating system, and self-rated house condition.

Table 4: IV Estimates of the Effect (= LATE) of Energy Information Disclosure on House Prices

	OLS	IV	IV (by subgroups of age)			
			pre 1977	1978-2002	after 2002	n.a.
EPC	0.032**	-0.051**	-0.044**	-0.005	0.004	-0.270**
Standard Errors	(0.002)	(0.011)	(0.016)	(0.017)	(0.028)	(0.056)
Zip code fixed effects	✓	✓	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓	✓	✓
House characteristics **x**	✓	✓	✓	✓	✓	✓
Number of observations:	165,057		82,346	47,905	12,182	22,624

Note: Standard errors are in parentheses. ** denotes statistical significance at the 1% level, * at the 5% level.

A similar argument may explain why the LATE is particularly strong for houses whose owners do not communicate the year of construction in advertisements, as reported in the last column of Table 4. In such cases, uncertainty of potential buyers about the quality of a house may be particularly strong, as its age is a characteristic that allows for an approximate assessment of the energy efficiency of a building. It therefore seems plausible that disclosure has a pronounced effect for buildings for which no age is announced.

6. Robustness Checks

To check the plausibility of our estimation results, we run so-called placebo regressions to explore whether for any month X prior to May 2014, house price trends diverge across seller types in the aftermath of month X. In case of divergence, the assumption of common price trends prior to May 2014, which is crucial for our identification strategy presented in Section 4, would be violated. These placebo regressions, for which we presume a fictitious policy intervention in month X, are based on the following reduced-form equation for all months X between January 20113 and May 2014, thereby benefitting from the richness of the data set:

$$\begin{aligned} \ln p_i &= \beta_0 + \beta_{PostX} PostX + \boldsymbol{\beta}_{\mathbf{x}}^T \mathbf{x}_i + \beta_{Bank} Bank_i + \beta_{Agent} Agent_i \\ &\quad + \beta_{Bank,PostX} Bank_i \cdot PostX + \beta_{Agent,PostX} Agent_i \cdot PostX + \nu_t + \mu_z + u_i. \end{aligned} \tag{4}$$

In this equation, placebo treatment dummy *PostX* refers to the month X for which the fictitious policy intervention is presumed. For example, PostJan2013 denotes a dummy variable that equals unity for all advertisements that appeared between January 2013 and May 2014 and equals zero otherwise. For each of these placebo dummies, we individually estimate Equation 4, taking into account only observations before May 2014 in order to avoid any overlap with the regulatory change in May 2014.

These placebo regressions allow us to check the plausibility of the core identifying assumption of common price trends across seller types. If this assumption were to be invalid and, hence, price trends were to differ across seller types prior to May 2014, the coefficients $\beta_{Bank,PostX}$ and $\beta_{Agent,PostX}$ would turn out to be statistically different from zero. Estimates of these coefficients, reported in Table 5, are close to zero and not statistically significant for any of the placebo treatments. Thus, on the basis of these placebo regression outcomes, we are confident that the estimation results presented in the previous section indeed reflect the impact of disclosure, rather than a failure of the common trends assumption.

Table 5: OLS Placebo Regression Results for $\beta_{Bank,PostX}$ and $\beta_{Agent,PostX}$

	$\beta_{Bank,PostX}$		$\beta_{Agent,PostX}$	
	Coeff. s	Std. Errors	Coeff. s	Std. Errors
PostJan2013	0.002	0.014	-0.000	0.014
PostFeb2013	-0.002	0.010	0.009	0.010
PostMar2013	0.002	0.008	0.004	0.008
PostApr2013	-0.004	0.007	-0.000	0.007
PostMay2013	-0.002	0.007	0.002	0.007
PostJun2013	0.001	0.006	0.002	0.006
PostJul2013	-0.002	0.006	-0.002	0.006
PostAug2013	-0.002	0.006	-0.004	0.006
PostSep2013	-0.002	0.006	-0.003	0.006
PostOct2013	0.002	0.006	-0.003	0.006
PostNov2013	0.005	0.007	0.003	0.007
PostDec2013	0.005	0.007	0.004	0.007
PostJan2014	0.002	0.008	0.000	0.008
PostFeb2014	0.009	0.009	-0.001	0.009
PostMar2014	0.021	0.012	0.007	0.012
Zip code fixed effects	✓			
Month fixed effects	✓			
Housing characteristics $\mathbf{x}_i$	✓			
Number of observations:	95,844			

Note: ** denotes statistical significance at the 1 % level, * at the 5 % level.

Another concern might be that some sellers may have withdrawn their advertisements after May 1, 2014, when energy information disclosure became legally compelling, falsely fearing that not disclosing would be punished by heavy fines – a fear that was actually unwarranted

Table 6: Robustness Checks for the IV Estimation of Model 3

	Observations originating from Jan 2014 - Feb 2014 or July 2014 - Dec 2014	House Prices p in Levels Full sample
EPC	-0.054**	-13,031**
	(0.012)	(2,571)
Zip code fixed effects	✓	✓
Month fixed effects	✓	✓
House characteristics $\mathbf{x}_i$	✓	✓
Number of observations	140,756	165,057

Note: Standard errors are in parentheses. ** and * denote statistical significance at the 1 % and 5 %, respectively.

before May 2015, when the transition period without fines ended. To address the concern of a large number of withdrawals of advertisements, we re-estimate Model 3, thereby omitting advertisements originating from a symmetric interval of four months around May 1, 2014. Consequently, all observations falling into the period March to June 2014 are dropped. The LATE estimate of -0.054, displayed in the first column of Table 6, is almost the same as that resulting from the IV estimation presented in Table 4, suggesting that potential withdrawals of advertisements does not cause any problem. Finally, for a last robustness check, we estimate Equation 3 using house prices in levels, rather than logarithms, still finding a negative estimate of the effect of disclosure that is statistically significant from zero (Table 6).

7. Summary and Conclusions

Based on a comprehensive data set that covers major parts of the German housing market for the years 2013 to 2015, we have investigated the consequences of a policy change that mandates the disclosure of energy information in real estate advertisements as of May, 2014. Motivated by a stylized theoretical model, we have tested the following hypothesis: Offer prices for houses whose owners would not voluntarily disclose their house's energy consumption should decrease upon a shift to a mandatory disclosure scheme.

Our empirical identification strategy rests on the variation in the response of seller types, such as banks and real estate agents, to the policy change: while banks complied with the new rules to a large extent, the response was weaker for both real estate agents and private sellers. We exploit this variation to estimate the causal effect of the disclosure of energy information on the offer prices of houses, using an instrumental variable approach to cope with the endogeneity of the disclosure decision

Our empirical results indicate that the local average treatment effect (LATE) of disclosure, that is, the effect for the subgroup of sellers that only include energy information into real-estate advertisements in response to the policy change, is negative. Notably, we find that the magnitude of the LATE is particularly large for older houses that were constructed prior to the introduction of building codes in 1977.

Consistent with the theoretical outcome of a strategic withholding of quality information for low-quality houses under a voluntary disclosure, our empirical results point to a reduction in the offer prices of houses when a policy of mandatory disclosure forces sellers to disclose otherwise hidden information.

These findings suggest that a voluntary approach is insufficient to induce sellers to disclose qualities. In other words, the central finding of "market unraveling" from information economics, claiming that voluntary disclosure regimes can be sufficient to overcome information asymmetries, is only of limited empirical relevance for the German housing market. From a policy perspective, our study thus underlines the necessity and the power of mandatory disclosure rules to address information asymmetries with respect to the thermal quality of houses.

Appendix A Derivation of Equilibrium Strategies

In this section, we solve the voluntary disclosure game presented in Section 2 for its sequential Bayesian Nash Equilibrium. The equilibrium strategy of the seller is denoted by $\sigma_s^*(v) = (DISC(v)^*, P_D^*, P_{ND}^*)$, where $DISC(v)^*$ represents the decision to either disclose (D) or to not disclose (ND) and $P_j^*, j \in \{D, ND\}$, corresponds to the decision of setting the offer price, after having disclosed (P_D^*) or not disclosed (P_{ND}^*). Furthermore, the strategy of the buyer is given by $\sigma_b^* = (BUY_D^*, BUY_{ND}^*)$, where $BUY_j^* \in \{A, R\}$, that is, buyers can either accept (A) or reject the offer price (R).

We first focus on the subgame that follows the decision of the seller to disclose. In this subgame, quality information is revealed. Accordingly, the buyer acts under perfect information and his best response to a seller's price offer P_D^* is:

$$BUY_D^* = \begin{cases} A & \text{if } v \geq P_D^* \\ R & \text{if } v < P_D^*. \end{cases}$$

Anticipating this, the profit maximizing strategy of the seller implies $P_D^* = v$ and profits of the seller, taking disclosure costs into account, correspond to $v - c$.

Next, we consider the subgame that follows the decision of the seller not to disclose. In this situation, quality information is private, so that a buyer has to form beliefs over the quality. Following Eyster and Rabin (2005), we assume that the belief of the buyer over qualities, given the seller's actions a_s and equilibrium strategy σ_s^*, $\widehat{prob}(v_j|a_s, \sigma_s^*)$, is of the form:

$$\widehat{prob}(v_j|a_s, \sigma_s^*) = \left((1-\chi) \frac{\sigma_s^*(a_s|v_j)}{\sum_{v_j \in V_j} prob(v_j) \sigma_s^*(a_s|v_j)} + \chi \right) prob(v_j),$$

where χ denotes the degree of naivety of the buyer, v_j represents one of J non-overlapping intervals on the unit interval $[0, 1]$ with $\sum_{j=1}^{J} prob(v_j) = 1$ and $prob(v_j)$ corresponds to the probability that v falls into the interval v_j. In our setting, we focus on the two intervals $[0, v^*)$ and $[v^*, 1]$, where v^* is a constant cutoff value for disclosure that occurs if $v \geq v^*$.

Accordingly, the belief of the buyer that $v \geq v^*$, given that the seller acts according to the equilibrium strategy σ_s^*, is:

$$\begin{aligned}
&\widehat{prob}(v \geq v^*|a_s = ND, \sigma_s^*) \\
&\quad = \left((1-\chi)\frac{\sigma_s^*(ND|v \geq v^*)}{prob(v \geq v^*)\sigma_s^*(ND|v \geq v^*) + prob(v < v^*)\sigma_s^*(ND|v < v^*)} + \chi\right) prob(v \geq v^*) \\
&\quad = \left((1-\chi)\frac{0}{(1-v^*)0 + v^*1} + \chi\right)(1-v^*) \\
&\quad = \chi(1-v^*).
\end{aligned}$$

Similarly, we obtain for $v < v^*$:

$$\begin{aligned}
&\widehat{prob}(v < v^*|a_s = ND, \sigma_s^*) \\
&\quad = \left((1-\chi)\frac{\sigma_s^*(ND|v < v*)}{prob(v \geq v^*)\sigma_s^*(ND|v \geq v^*) + prob(v < v^*)\sigma_s^*(ND|v < v^*)} + \chi\right) prob(v < v^*) \\
&\quad = \left((1-\chi)\frac{1}{(1-v^*)0 + v^*1} + \chi\right) v^* \\
&\quad = (1-\chi) + \chi v^*.
\end{aligned}$$

At the last decision node, the buyer decides under uncertainty and maximizes expected utility. Given the beliefs over the type of the seller, the buyer accepts the offer as long as the buyer's expected utility is larger than zero. In our case this condition reads:

$$\begin{aligned}
E\left(u_b(\sigma_s^*(v), BUY_{ND} = R)\right) \leq & E\left(u_b(\sigma_s^*(v), BUY_{ND} = A)\right) \\
\Leftrightarrow \quad 0 \leq & \widehat{prob}(v < v^*|a_s = ND, \sigma_s^*)E(v - P_{ND}^*|v < v^*) \\
& + \widehat{prob}(v \geq v^*|a_s = ND, \sigma_s^*)E(v - P_{ND}^*|v \geq v^*), \qquad (5)
\end{aligned}$$

where $u_b(\cdot)$ denotes the buyer's utility function. By inserting the beliefs of the buyers from above and taking advantage of closed form solutions for the truncated means of a random variable that is uniformly distributed on the unit interval, $E(v < v^*) = v^*/2$ and $E(v \geq v^*) = (1+v^*)/2$, Inequality 5 can be rearranged as follows:

$$\frac{\chi}{2} + \frac{(1-\chi)v^*}{2} \geq P_{ND}^*.$$

Accordingly, the best response to a price offer $P_{s,ND}^*$ is:

$$BUY_{ND}^* = \begin{cases} A & \text{if } \frac{\chi}{2} + \frac{(1-\chi)v^*}{2} \geq P_{ND}^* \\ R & \text{if } \frac{\chi}{2} + \frac{(1-\chi)v^*}{2} < P_{ND}^*. \end{cases}$$

Anticipating this, the seller adopts the following pricing strategy:

$$P_{ND}^{*} = \frac{\chi}{2} + \frac{(1-\chi)v^{*}}{2}$$

Having solved the two subgames for their unique sequential (Bayesian) Nash Equilibrium allows to consider the decision of the seller at her first decision node. Anticipating the utility levels that are implied by equilibrium strategies, the seller's strategy implies the following decision rule on disclosure:

$$DISC(v)^{*} = \begin{cases} D & \text{if } v - c \geq \frac{\chi}{2} + \frac{(1-\chi)v^{*}}{2} \\ ND & \text{if } v - c < \frac{\chi}{2} + \frac{(1-\chi)v^{*}}{2}. \end{cases}$$

Accordingly, the seller discloses the quality a house with a quality weakly above a certain cutoff value v^{*}, which is determined by $v^{*} - c = \frac{\chi}{2} + \frac{(1-\chi)v^{*}}{2}$ and, hence:

$$v^{*} = \frac{2c + \chi}{1 + \chi}.$$

Appendix B Comparison of Seller Types

The portfolios of houses offered by either private sellers, estate agents, or banks are quite homogeneous, with some notable differences (Table 7). For instance, houses offered by banks are cheaper, on average, than those sold by real estate banks and private sellers, they are more likely to be situated in regions with slightly lower average per-capita incomes, have less living space and somewhat worse energy efficiency ratings. Private sellers tend to sell row-houses more often than banks and real estate agents, houses offered are on average some years younger compared to both estate agents and banks and energy efficiency ratings of the houses seem to be slightly better. In general, however, differences between the seller types are rather small and all lie below 67% in terms of the normalized difference in means, which are normalized by the average standard deviation.

Table 7: Differences in Characteristics, by seller type.

	Banks	Estate Agents	Private Sellers	Normalized Difference Banks - Agents	Normalized Difference Agents - Private
Offer price, in 1,000 EUR	232.0	283.6	282.9	-35.8	0.5
Ad running time, in days	86	91	50	-5.4	48.4
Lot size	737	714	645	3.9	12.3
Living space	162	172	161	-14.9	16.1
House category:					
Row house	0.29	0.32	0.41	-7.6	-19.5
Multi-family house	0.17	0.14	0.10	7.8	12.9
One-family house	0.55	0.54	0.49	1.4	10.4
House Condition:					
Normal	0.39	0.46	0.47	-21.6	???
Superior	0.64	0.54	0.47	21.6	13.8
Age	43.2	39.6	36.5	13.0	11.7
Years since last modern.	9.3	7.5	6.3	23.3	17.3
# of rooms	6.0	6.1	6.2	-7.0	-1.6
# of bathrooms	1.8	1.9	1.8	-5.7	12.3
# of bedrooms	3.4	3.7	3.3	19.6	32.9
Guest toilet	0.29	0.60	0.77	-66.9	-36.0
EPC disclosed	0.28	0.21	0.22	14.9	-1.3
Energy consumption (kWh/m^2)	164	146	131	25.3	23.3
Population density	1.3	1.8	1.8	-17.2	2.2
Income per capita	20.2	21.3	22.1	14.9	-1.6
Sample size	28,399	73,604	19,496		

Notes: Normalized differences equal the differences in means, normalized by the average of the standard deviation in the two sub-samples.

Selling activities differ strongly across seller types (Table 8): on average, real estate agents

Table 8: Number of Sales on Immobilienscout24 in 2013 and 2015 across Seller Types.

	Banks	Estate Agents	Private Sellers
Number of sellers	1,777	14,332	19,221
Number of sales	28,399	73,604	19,496
Average number of sales	16.0	5.1	1.0
Std. Dev.	46.3	19.3	0.2

Source: Immobilienscout24, own calculations.

sold 5 houses in the years 2013 and 2014, compared to 16 houses by bank agents, while private sellers typically sold only one house. This finding may reflect a different degree of professionalism across seller types. For example, many banks have established departments that ensure compliance with laws, while such departments are typically not available for real estate agents.

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