

WORKING PAPER

Salience of debt and homebuyers' credit decisions

NORGES BANK
RESEARCH

21 | 2015

AUTHORS:

SUMIT AGARWAL
AND
ARTASHES
KARAPETYAN



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ISSN 1502-819-0 (online)

ISBN 978-82-7553-888-6 (online)

Salience and Mispricing: Homebuyers' Housing Decisions¹

Sumit Agarwal

Georgetown University

Artashes Karapetyan²

BI Norwegian Business School

November 2016

Abstract

We show that a regulatory disclosure of hidden debt eliminated a large mispricing in housing. In a setting where homebuyers must combine several sources of debt, they are biased towards hidden loans, especially if they are young, or have no experience in financial or housing markets. By reducing the mispricing of units with hidden debt, increased salience of debt benefited homebuyers, particularly those suffering most from it. An average homebuyer could save about \$20,000 by acquiring a dwelling with one standard deviation lower debt. We confirm lack of salience was the main source of the bias, by showing the regulation nearly eliminated the mispricing.

Keywords: Salience, Housing, Cooperatives, Mortgage, Household Finance, Mispricing

JEL Classification: D12, G14, G21, G32

¹ This paper should not be reported as representing the views of Norges Bank. The views expressed are those of the authors and do not necessarily reflect those of Norges Bank. Most of this work was carried out when Artashes Karapetyan was at Norges Bank Research. We benefited from the comments of Johan Almenberg, Gene Amromin, Souphala Chomsisengphet, Samuli Knupfer, Øyvind Norli, Ivan Png, Nagpurnanand Prabhala, Wenlan Qian, Tarun Ramadorai, Amit Seru, Nick Souleles, Bogdan Stacescu and seminar participants at the National University of Singapore, BI Norwegian Business School and Barcelona GSE. We thank Eiendomsverdi AS for providing us with the data. Earlier version of the paper was titled "Salience of Debt and Homebuyers' Credit Decisions".

² Corresponding author. BI Norwegian Business School, Nydalsveien 37, Oslo 0442, Norway E-mail: Artashes.karapetyan@bi.no, Tel: +4746410548, Fax: +4746410501.

1. Introduction

Like many consumer financial decisions, buying a house can be complex. Consumers need adequate financial information to make an optimal decision. While diligent consumers tend to gather more information about all the costs involved, others may not exert enough effort to do so, or may differ in their ability to understand the subtleties of the contracts. Indeed, recent evidence suggests that many consumers make poor financial decisions in mortgage and other credit markets (Campbell et al., 2011).

While the failure of households to optimize their financial transactions may often entail a minor cost in other markets, costs can be substantial for participants in the mortgage market. Buying a house is one of the largest financial decisions made by a household. Despite the large costs, recent research from the U.S. shows borrowers in mortgage markets do not optimize: for instance, they struggle to evaluate the tradeoff between interest rates and other fees (Woodward and Hall, 2010), or fail to optimally refinance mortgages (Keys et al., 2016; Agarwal, Rosen and Yao, 2015).³

In this paper we examine whether consumers are able to make an optimal choice regarding their home acquisition in a setting where the total price has both salient and hidden parts. We analyze the impact of a regulatory shock to the salience of debt on home prices. Specifically, we make use of a law that required the disclosure of a sizeable hidden debt – a part of the total acquisition price. We ask whether an observed mispricing in the presence of hidden debt vanishes with mandatory disclosure of that debt. If so, we then ask how much a homebuyer could save by acquiring a correctly priced dwelling, and whether various homebuyers benefit differently from the increased salience.

In the Norwegian housing market, apartments are organized either as housing co-ops or self-owned units. Both are prevalent across the country, with approximately 40 percent of all apartments being co-op units. While these two types of housing are similar in most aspects, co-op units differ from self-owned units in an important way: co-ops borrow debt to finance the development of the housing. As a result, a household acquiring a leveraged co-op unit will have

³See also Agarwal, Ben-David, and Yao (2015) for mistakes on mortgage points.

both a personal and co-op debt, and should consider the total acquisition price. A further important aspect is that there is a high variation in the amount of debt borrowed across the various co-ops. Self-owned units, in contrast, do not inherit debt for the development of the building, and the acquisition will involve only the personal debt, i.e. a mortgage.⁴

Co-op debt can be more shrouded, and therefore consumers are more likely to be unaware of it. Despite this, it must be serviced by each homeowner. Prior to January 1, 2008, the disclosure of co-op debt was not required by law.⁵ At the beginning of 2008, a regulatory change was implemented, requiring that the terms of all sources of debt in dwellings be disclosed. In particular, the regulation required that the amount of joint debt with its servicing terms be disclosed at the time of sale along with the transaction price. This change should hardly affect co-op units with little debt, or self-owned units, since they generally have near-zero debt. On the other hand, it can reduce prices of co-op units with moderate or high debt if homebuyers - at least some of them - were indeed unaware of hidden debt. Before the requirement, intermediaries could choose to not disclose (plausibly, intentionally) the existence of debt, as evidenced by a number of consumer complaints.⁶ We confirm a large price impact suggesting that mispricing stemmed from unawareness about hidden debt.

We begin our empirical analysis by studying how (high-debt) co-op prices are affected by the implementation of the law. Our hypothesis is that the mandatory disclosure of the previously shrouded debt and its terms has a negative impact on the co-op unit prices with high debt, but not on those with (almost) no debt. To test this, we first compare the extent to which co-op debt is reflected in market prices before and after the law (the capitalization rate). Assuming as a benchmark that a dollar increase in co-op debt should nearly result in a dollar decrease in the sales price, we find that before the regulation co-op debt is undercapitalized (i.e., debt was less than fully priced in the transactions) in apartment prices by around 25 (19-32) percent. By

⁴ However, some debt may still arise in the future due to the renovation of common areas in buildings. We will return to this later in the paper.

⁵ Another factor that makes co-op debt less salient is that it is not seen directly in homeowners' monthly fees. The service of debt and its interest payments is not itemized, but is part of a total monthly fee, which also includes capital and maintenance expenses.

⁶ We make an extensive search in all major newspapers and find a number of articles on hiding about co-op debt by agencies preceding the law. Almenberg and Karapetyan (2014) bring survey evidence on consumer unawareness about co-op debt in Sweden.

choosing a co-op unit with a one standard deviation lower debt, a homebuyer could reduce her total cost of home acquisition by about 80,000 NOK, or about 15,000 USD in our benchmark estimation.⁷ After the regulation undercapitalization decreases by around 12 percentage points within short windows (starting one month up to one year), and by 19-30 percent in the longer run. In the long run, undercapitalization vanishes almost entirely.

The correction, therefore, brings about an equivalent price impact. This can be seen in Figure 1: high-debt co-op units started to exhibit a lower relative price in 2007. Because the correction takes place due to disclosing the hidden aspects of the co-op debt, we conclude that it is indeed *lack of salience* that causes the mispricing, and the resulting suboptimal home acquisition decisions.⁸ Furthermore, given that co-ops are nearly always indebted, no co-op is entirely unaffected, and our numbers may underestimate the true mispricing. Therefore, we move on to additionally include self-owned units in subsequent estimations, and confirm a price impact that is larger by about 30-40 percent (over 20,000 USD).

The total acquisition price of a dwelling is the sum of the debt and the sales price, and this calculation does not require much financial knowledge beyond elementary operations. Yet, it does not mean there is no benefit to financial knowledge or experience in the presence of hidden debt. For instance, Stango and Zinman (2014) find that consumers with lower education and financial literacy are more likely to suffer from hidden features of credit. It therefore seems plausible that lack of salience has different costs for homebuyers with various characteristics, including financial experience. To explore this, we match our transaction data with administrative data from Norway, covering demographic and financial information on all Norwegian taxpayers. We find that individuals with no investments in financial markets, young homebuyers, and first-time homebuyers pay the largest price for hidden debt, as well as benefit the most from its disclosure. Using a triple difference approach, we find that young buyers pay about 30,000 USD less for co-ops after the disclosure of co-op debt. Similarly, individuals with

⁷1 USD was around 5.5 NOK in this period.

⁸ Given the competitive mortgage market in Norway and the comparable terms of the two sources of debt, the undercapitalization is unlikely, as we show, to be explained by rational choice.

no investments in financial markets, and those with no prior home ownership benefit more, than their benchmark groups.

Closest to our work, Almenberg and Karapetyan (2014) use data from Sweden to show that the present value of co-op debt is far from being fully capitalized into the sales price.⁹ Unlike our work, the source of the mispricing is not shown and the authors suggest rational choice (e.g., liquidity constraints) or other behavioral biases could be sources of the mispricing as well. Yet, they argue salience is key by bringing survey evidence in which a majority of individuals (about seventy out of a hundred surveyed) are unaware of the amount of debt their co-op has taken, consistent with our empirical findings. Instead, using a shock to the salience, we contribute, first, by showing that lack of salience is the source of this bias; specifically, we show that the bias is indeed eliminated after the shock. Second, we quantify the mistake from the salience shock directly. Third, we identify vulnerable groups for whom hidden debt was the most costly. Finally, Norway provides an ideal setting for the test since self-owned and low-debt units coexist with high-debt co-op units, allowing to analyze the shock with several difference-in-difference approaches.

Our findings remain unchanged when we carry out a number of robustness checks. First, we check that our results survive falsifications tests. We then implement several empirical approaches to address endogeneity. To minimize the effects of confounding factors, we focus on observations immediately preceding and succeeding the regulation using short (starting 30 days) as well as long windows. We demonstrate both statistical and economic significance of the impact on the pricing within already short periods.

A further concern is unobserved heterogeneity: larger co-op debt could be positively correlated with the quality of a co-op. For instance, major renovations can be financed raising co-op debt, which, if true, could bias our results. We address this concern by first correcting our debt measure using a proxy for renovation expenses. Then, we narrow our attention to a subsample of new buildings, where major renovations are unlikely to have occurred and where capital depreciation is unlikely to diverge greatly between buildings. Finally, we use building level

⁹ Similarly, Hjalmarsson and Hjalmarsson (2009) also studied undercapitalization, but having no access to information on debt, they rely only on the total monthly fee.

fixed effects and confirm that results remain nearly unaffected.

We then explore alternative hypotheses that could explain our findings. For instance, could rational choice, such as liquidity constraints, be accountable for the results? We provide several tests to show that rational bias is unlikely to contribute to the large misplacing.

Besides designing mechanisms to increase home ownership, an equally important public policy is to improve credit standards and reduce defaults. Our findings have important policy implications. Young, low-income, and first-time homebuyers are the most likely cohorts to fall victim to the costs of the bias. These suboptimal decisions may cause imbalances via increased defaults and have a significant impact on the entire economy.^{10,11} Additionally, the “transfer” that they unknowingly make possibly increases inequality, as it arguably benefits the more well-off groups (such as housing developers), whose marginal utility from the transfer is likely lower.

Apart from increasing salience, our findings on the importance of financial experience suggest that the effectiveness of other policy measures, such as investment in financial education, must be evaluated more broadly. Even in settings, where the need for solid financial literacy (i.e., beyond elementary operations) is not that obvious, or where other biases causing mistakes have not yet been identified by policy-makers and are thus unaddressed, financial education at large can ameliorate ensuing consequences.

Our work contributes to a growing literature in finance and marketing that has explored how individuals respond to the salience of information in various contexts (for instance, to salience in taxation as in Chetty et al., 2009; Chetty and Saez, 2009; Finkelstein, 2009), as well as in different markets, including corporate finance decisions, retail financial products, medical insurance, and credit cards. Heitman et al. (2014) show that the way in which prices and add-on features are presented, can affect how well buyers evaluate the tradeoff between products. Perez-Trugila and Troiano (2015) provide a large field experiment and show that increasing the salience of financial (and shaming) penalties reduces tax delinquency. Graham et al. (2015)

¹⁰ See Campbell (2006) on the importance of mortgage markets and suboptimal decisions (2006).

¹¹ For instance, the sharp decline in housing markets and the associated rise in mortgage defaults in the US during the recent financial crisis were partially caused by suboptimal decisions made by homebuyers.

show the hidden nature of the marginal tax may lead managers to take incremental decisions based on the average tax rate, resulting in large deadweight losses.

In credit card markets, Agarwal, Driscoll, Gabaix and Liabson (2013) find customers lose in penalty fees, and show that learning to reduce fees may be forgotten over time. Agarwal, Chomsisengphet and Souleles (2015) report that a substantial fraction of consumers choose ex-post sub-optimal credit contracts. Many credit card holders fail to minimize costs by switching to a cheaper available source of credit, such as a credit card with better rates and fees, or other liquid and low-yielding assets (Stango and Zinman, 2009). Agarwal et al. (2009) find that consumers take out payday loans at very high interest rates even when they have access to cheaper sources of financing. More recently, Ru and Schoar (2015) suggest that certain credit cards can have lower interest rates (salient feature) combined by even higher hidden payments, which are more easily packaged to less sophisticated consumers.

Closer to our work, a handful of studies have shown how irrational behavior may affect choice in the housing market (see, for example, Genesove and Mayer, 2001; Brunnermeier and Julliard, 2008). Keys et al. (2016) focus on the decision to refinance mortgages and find that borrowers fail to refinance due to irrational biases, leaving large amounts of money on the table. Agarwal, Rosen and Yao (2015) report that in addition to such errors of commission, borrowers also make errors of omission - they may fail to refinance their mortgage or make optimal choices at the right time. Woodward and Hall (2010, 2012) study the relationship between the origination fees that borrowers pay directly and those they pay indirectly through the yield-spread premium (YSP), and find that borrowers fail to evaluate the trade-off well. Homebuyers may also fail to get enough quotes when shopping for their mortgage contract, as nearly half forego benefits by getting only one quote (Allen et al., 2014; Lee and Hogarth, 2000). Finally, Gurun et al. (2016) show that by increasing the salience of the initial interest rate, advertising can entice naïve consumers into shrouded high reset rates. In contrast, we study the salience features of the acquisition price itself. The first striking feature of our experiment is that the benefit is straightforward. While in other contexts calculating the financial benefit may be more complex and benefits can accrue over a long period of time (e.g., mortgage refinancing, trade-off between interest rate and late payment fee), the trade-off here requires knowledge of

elementary operations at best. Furthermore, the remedial price impact is particularly large, previously undocumented to the best of our knowledge.

The remainder of the paper is organized as follows. Section 2 discusses institutional setup. Sections 3 and 4 provide empirical design and evidence, and Section 5 concludes.

2. The Housing Market in Norway

During the 1980s, direct credit regulations of banks were abolished in Norway. The credit market liberalization brought about a sharp boom in real estate markets. This did not last long and was followed by a collapse in stock and housing markets and an increase in credit defaults. This forced many banks to close down or merge with other banks in the banking crisis at the end of 80's and early 90's. Since then housing prices in Norway have increased every year, except for 2008 when they experienced a slight decline. From 1992 to 2014 the national house price index computed by Statistics Norway increased by 402 percent, whereas the consumer price index increased by only 55 percent. During our sample period, 2003-2012, housing prices more than doubled, with an average annual increase of approximately 6.5 percent.

Norway is traditionally characterized by high homeownership. The majority (76 percent) of Norwegian households own their home, and the sharp price appreciation has boosted household wealth. Homes constitute roughly two-thirds of gross household wealth and more than 100 percent of net household wealth (OECD, 2014). In the US, for instance, households hold approximately 18 trillion USD in real estate assets (Agarwal, Driscoll and Laibson, 2013), whereas the Norwegian population of five million owns real estate worth an astounding 0.9 trillion USD.^{12,13}

However, household credit growth has exceeded income growth for more than a decade in Norway, and the average debt-to-income ratio of households increased from 130 percent in

¹² Statistics Norway, 2012, <https://www.ssb.no/en/iformue>

¹³ In 2006, the middle of our sample, outstanding household residential mortgage debt was more than 200 billion USD; see http://www.ecbc.eu/uploads/attachements/75/60/norwegian_covered_bonds_market.pdf

1992 to 220 percent in 2014. So far, high household leverage has not affected the credit default rate, which has been low and stable at approximately one percent since the late 1990s (Solheim and Vatne, 2013). One reason for this could be the full-recourse loan policy in Norway.

Following the international financial crisis, the Norwegian government started to implement macro- and micro-prudential policies to enhance financial stability. So far, stricter capital requirements and tightening of loan-to-value limits do not seem to have contained the increase in housing prices and credit growth.

2.1 Co-op and Self-owned Units

Approximately 99 percent of all multi-dwelling units in Norway are either co-ops or self-owned apartments (“*selveier*”).¹⁴ The prices of both types of units are determined in the market, where units are sold in auctions.

Buying a co-op unit is equivalent to acquiring a share of the co-op and a membership in the co-op association. The shares can be pledged as collateral against the home mortgage, but they do not entitle to property rights over the unit. However, homeowners in Norwegian housing co-ops have about the same command over their homes as those who live in self-owned apartments, and are equally free to renovate or modify their units.

Before a co-op is initially formed and the shares are sold, the residential developer can decide how much of the total cost of the shares will be paid upfront by the prospective buyers and how much of it will be financed by a loan taken by the co-op itself (co-op debt). Importantly, co-op owners service their pro-rata share of the co-op debt. *Ceteris paribus*, higher co-op debt should therefore imply a lower price. Debt and its interests are then serviced by owners monthly as part of a total monthly co-op fee. Co-op fees will be higher if the co-op opts to finance a larger amount.

¹⁴The two types units coexist in different ratios. In Sweden co-ops were the only form of apartments until recently, in New-York alone there are more than 600,000 co-op units, while they are present more moderately in other parts of the world. See more in “Profiles of a Movement: Co-operative housing around the World”, available at <http://www.housingeurope.eu>.

On top of the financing expenses, the fee includes maintenance expenses, but the various costs are not itemized. Maintenance includes common are expenses, such as cleaning, and may vary substantially across co-ops depending whether they also include the provision of heating and/or hot water.

The co-op board needs to approve or reject prospective buyers of co-op units. However, the co-op has no right to reject buyers with documented capacity to service mortgage. Thus, homebuyers who are approved for a mortgage will normally be approved by a co-op.¹⁵

On July 29, 2007, a *Law on Real Estate* (“*Lov om eiendomsmegling*”) was announced. It came into force in January 1, 2008 and is still the current law. The purpose of the act was to “facilitate transfer of real estate using an intermediary by providing impartial assistance to both parties” (§1-1). It regulates the activities of real estate brokerage firms, and in particular, the real estate broker's duty to provide detailed and impartial information about the costs associated with the acquisition of the property.¹⁶ The law requires that the broker always give the potential buyer a written assignment (prospectus), which contains at least the following information: total costs, including share of the joint debt, all fees, and taxes (§ 6-7). Additionally, it requires that the broker provide clear and detailed information about the total sum of the buyer's share of the debt and the transaction price, both in the prospectus and in the sales advertisement.¹⁷ Such requirement was not mandated before the law, and so sellers could hide the information in an attempt to get a higher price. Thus, the regulation is expected to cause a differential effect on units with high versus low debt.

Furthermore, *The Housing Cooperative Act* regulates, among other things, the owners' obligations regarding the joint debt.¹⁸ Importantly, no member is personally liable to creditors

¹⁵ In very rare cases, households who are rationed in the mortgage market due to insufficient regular income may nevertheless be able to afford a co-op unit, but not a self-owned one. This can, however, be true for only those co-ops that have high enough leverage, so as to allow the homebuyer to finance the whole transaction by cash and avoid a mortgage from a lender. While lack of strong veto rights can indeed allow this, we do not find any significant support of this in the data. With reasonable levels of leverage, however, it is implausible that households be able to circumvent borrowing constraints.

¹⁶ The relevant information disclosure rules are spelled out in §6-7 of the law. All the details of the act are available at the link: <https://lovdata.no/dokument/NL/lov/2007-06-29-73>.

¹⁷ Thus, debt associated with self-owned apartments had to be revealed as well, but this debt is 0 or rather low. We return to this in our robustness analysis.

¹⁸ <https://www.regjeringen.no/en/dokumenter/the-housing-cooperatives-act/id439595/>

for the joint liabilities (section 1-2). If a member does not meet his or her obligations, the co-op may order the member to sell the shares with a notice of at least three months in advance. If so happens, the Cooperative Guarantee Fund will reimburse the housing co-op for missed payments, and are not borne by other members.¹⁹ Thus co-op debt will not be more expensive for homebuyers to service and each homebuyer is personally liable for both the mortgage as well as their (and only their) share of co-op debt. In addition, the full-recourse loan policy in Norway and rare defaults make it unlikely that homebuyers in our setting are strategically opting for co-op debt to reduce costs of servicing it. At the same time, the co-op would hold a first lien in the share that supersedes all other obligations. Most importantly for our empirical design, these factors are unaffected during our study, and unlike the disclosure of debt, do not move over time.

Co-ops pay taxes on profits, which typically are very low since they do not have incentives to generate profit. Additionally, they pay a property tax based on the tax value of the property. The latter is calculated as if the property was a rental building and is independent of the market prices of the actual co-op units and also of the capital structure of the co-op. Thus, neither of these taxes should have any impact on the buyer's decision when choosing between a high-debt and a low-debt co-op unit. The Norwegian market is characterized by a high level of competition and rather low interest rate margins on mortgages. The average mortgage margin has been below 1.5 percent, leaving very little room, if any, for a sizable difference between the interest rates for co-op debt and personal mortgage.²⁰ The costs of financing the debt can thus be considered roughly equal.

¹⁹ The Cooperative Guarantee Fund (Stiftelsen Borettslagenes Sikringsfond) was founded in 1994 and offers housing co-operatives insurance to secure payments from residents: <http://www.nbbl.no/About-NBBL/Subsidiary-companies/Borettslagenes-sikringsfond>.

²⁰ See http://www.norges-bank.no/pages/100709/Economic_commentaries_4_2014.pdf , Norges Bank Economic Commentaries, 4/2014.

3. Empirical Analysis

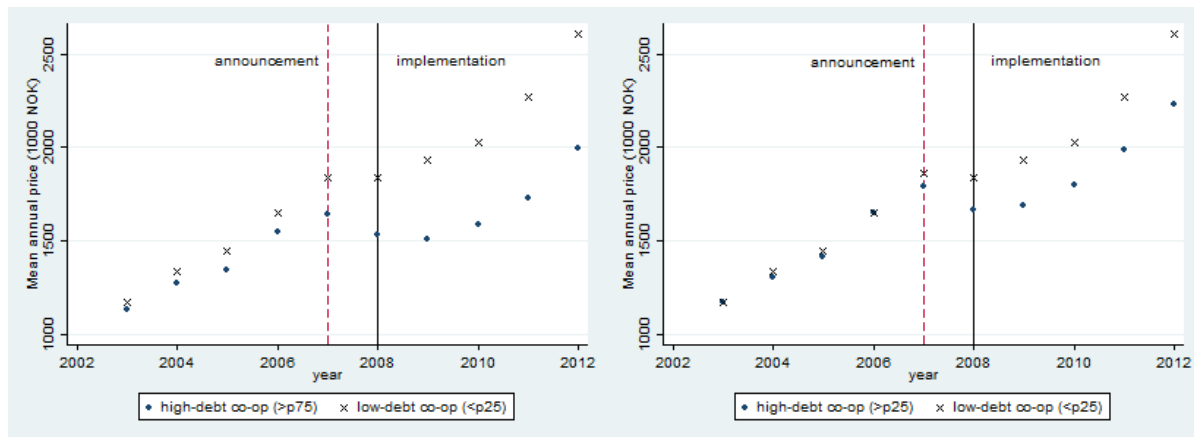
3.1 Data

Our sample consists of high-quality data on all apartment transactions in Oslo, Norway, between January 1, 2003, and December 31, 2012.²¹ In total, we have just above 130,000 transactions. After removing transactions with missing values on some of the explanatory variables, we are left with just above 120, 000. We then remove transactions of units that are sold before or during the year of completion, that comprise less than one percent of the data: these units are sold by developers directly rather than an intermediary, they are not sold in auctions (but at fixed prices), and are sold typically at a discount compared to the evaluated market price. We then are left with 119,372 transactions. For each transaction, we have information about the ownership of the unit, an indicator for co-op unit versus self-owned unit, the sales price of the unit, and the associated share of common debt. Co-op units comprise 67,212 transactions. Additionally, we have information about the date of the transaction, district where the building is located in the city, age of the building, the floor, and number of rooms in the unit.²²

Table 1 summarizes our data. The top panel shows statistics for the whole sample. Mean debt is much higher in an average co-op unit compared to a self-owned unit (by 140, 000). Unlike co-op units, newly built self-owned units have no debt. Debt of self-owned units is raised at a later stage (to finance common renovations and expenses) and increases gradually with the building's age, starting at below 5,000 for a unit under 10 years old and climbing to an average of 62,000 for a unit above 50 years old.

²² We confirm our results (slightly stronger) taking out variables with missing observations: floor, number of rooms, location.

Figure 1. Mean annual transaction prices, 2003 to 2012: Left panel compares low-debt (bottom quartile) with high-debt (top quartile) units, left panel. Right panel compares low-debt (bottom quartile) units with the rest. Announcement date is July 29 2007, implementation date is January 1, 2008.



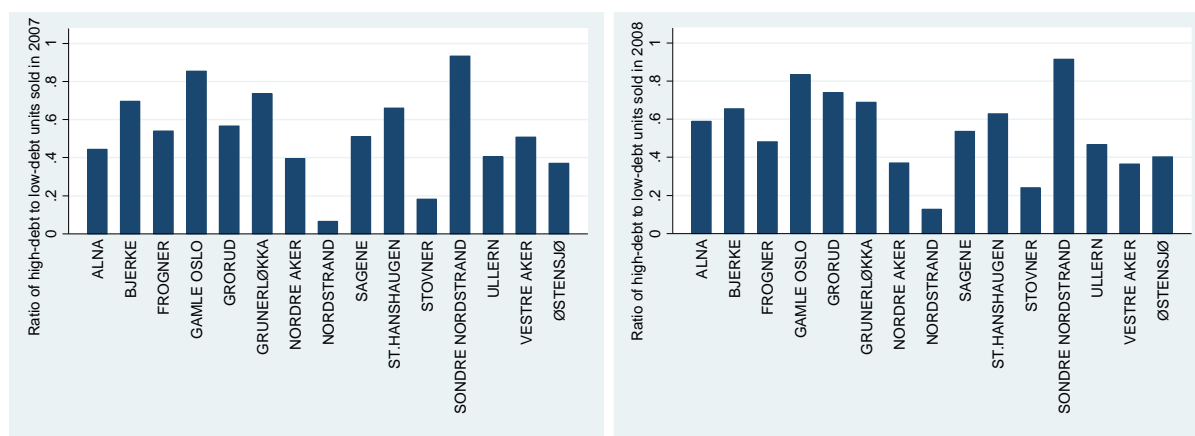
The two types of units differ to some extent in size and in age (self-owned units are larger by almost 14 square meters and are somewhat newer), but are more similar in terms of the number of rooms and the floor. Another explanatory variable that is not in the table is the location: the proportion of the sold units (and stock) of the two types varies across the city's districts, as the more demanded western districts (e.g., Frogner, Ullern) have higher proportion of self-owned units. Along with other differences, this accounts partially for the mean price difference in the table (1.2 million NOK).

In the middle panel, we provide all co-op transactions and compare the most leveraged (in the top quartile) co-op units to the least leveraged ones (bottom quartile). On average, the higher leveraged units are only slightly larger (by 2.7 square meters) and newer (by 2.7 years). The debt is larger by around 400,000 NOK, and the prices are lower by 180,000 NOK.

In the bottom panel, we provide summary for one year transactions before and after the event. The general pattern is also present here: highly leveraged units are similar in size (<2 sq. meter difference) and have comparable age. The changes in the explanatory variables are minimal over time, yet the prices of high-debt co-ops drop significantly (by around 90,000). Finally, the proportion of the high debt and low-debt units (as well as those of self-owned units) remained

constant over districts before and after the reform, as can be seen in Figure 2 (for self-owned units, see Figure A2 in the appendix).²³

Figure 2. The left (right) panel shows the proportion of low-debt (i.e., in the lowest quantile) co-op units as of total co-op units sold across the different regions in Oslo in 2007 (2008).



3.2 Hypotheses

A law announced on July 29, 2007, and implemented on January 1, 2008, requires that real estate agents disclose all costs related to buying a dwelling at the time of sale. This means that, as of January 1, 2008, the amount of co-op debt as well as the costs related to servicing that debt must be disclosed.

We hypothesize that the mispricing will be corrected after the law and thus lack of salience is the source of the bias. Norway provides an ideal setting for the analysis since co-op units have high variation in leverage, and they coexist with self-owned apartments. We exploit this variation for identification with a difference-in-difference (diff-in-diff) approach. We first test that within the sample of co-op units the increased salience of debt will be reflected in an

²³ In regressions we always include district fixed effects.

increased rate of capitalization of debt into prices.²⁴ As a result, the prices of high-debt co-op units will decline compared to low-debt co-op units after the implementation of the law (the “event”).

The baseline specification that we test is:

$$P_i = \alpha + \beta_1 Debt_i + \beta_2 * Post + \beta_3 (Debt_i * Post) + X_i \gamma + \varepsilon_i \quad (1)$$

where $Debt_i$ is the amount of co-op debt associated with apartment i , X_i is a vector of controls, including the unit’s hedonic characteristics (size of unit and floor, number of rooms, age of building and location).²⁵ $Post$ stands for a dummy that takes value one for any transaction that was completed on or after the event on January 1, 2008. Location is measured at the district level and we include a dummy variable for each district. There is, of course, the possibility that the district fixed effects do not control for all unobservable characteristics. For instance, to the extent that there is variation in apartment standards within a district which is correlated with debt, our estimated debt coefficient may still suffer from an omitted variable bias. While this issue is common to many cross-sectional studies, including hedonic price regressions, we do not believe that this is problematic in Norway for two reasons. First, strict building codes in conjunction with thin tails in the income distribution contribute to fairly homogeneous housing standards in Norway. Second, many indicators of standard are unlikely to be correlated with debt; specifically, idiosyncratic upgrades done by the co-op owner, such as a kitchen renovation, would be reflected in the price of an apartment but not in the co-op debt. We confirm our conclusions using building level fixed effects in an extended dataset later.

The validity of the diff-in-diff hinges upon the underlying assumption of parallel trends. It states that, absent the treatment, prices would have followed a parallel path, which is difficult to verify. The assumption implies that prices should follow a parallel trend in the pre-treatment

²⁴ Given the competitive interest rate markets and lack of liquidity considerations, this capitalization should be close to -1. We return to this issue in our robustness section.

²⁵ Other hedonic studies use similar set of variables (see for instance Gabriel et al., 2016). Lee et al. (2015) use in addition the existence of external facilities, such as pools and garages. Later we add building fixed effects to the model that would take care of unobservables at the building level.

period, which seems to hold. The left panel of Figure 1 plots mean annual transaction prices for housing units, for the lowest and top quartiles, while the right panel plots the lowest quartile against all other units. Until 2007, the prices have closely followed a common trend in house prices. In 2007, prices start to diverge, with a negative impact on highly leveraged co-ops.

It is important to note that mortgage rates in Norway went up during the period 2006 to 2008, from an average 3.98% at the beginning of 2004 to a highest 7.41% in the third quarter of 2008. Yet, both co-op and personal debt in Norway are predominantly floating rate. The share of fixed rate loans to debt in the total household sector, including both co-op debt and personal mortgage, was less than 10 percent in 2006.²⁶ While it is possible that co-op share of fixed rate loans is somewhat higher than that in personal mortgage market, such a difference would only make co-ops more attractive at a time of increasing mortgage rates. This would reduce, rather than increase, the magnitude of our results.

3.3 Results

In Table 2, we fit data to equation 1: we examine the extent of undercapitalization and the degree to which it changes after the event. If all co-op debt was taken into account by homebuyers before the law, the relation between debt and prices would be close to negative 1, while the event would not have a significant impact on the extent of debt capitalization. Our results show that this is not the case.

In the short run, column 1, 2 and 3 show a statistically significant impact of the event on the rate of capitalization at 13 (1 month before and after window), 11.4 (6 month before and after) and 13 percentage points (p.p.), respectively. Column 4 shows that the effect is larger in magnitude by about 26 p.p. in the long-run. As debt was capitalized into prices at 70 p.p. in the total pre-treatment sample, overall undercapitalization nearly vanishes: after the event, the rate

²⁶ The use of floating rate loans is also pervasive in the corporate sector at 70%. See Norges Bank Economic Bulletin 2007(1).

of debt capitalization (sum of the coefficients in the first and second rows) is no longer statistically significantly different from negative 1. As a first step in confirming that results are not driven by time-varying unobservables and observables that are correlated with the *Post* dummy, in column 5 and 6 we divide the sample into before and after event periods, as well as saturate the model with quarter fixed effects. The results are only marginally affected: the coefficient on debt has a statistically significant difference of 0.24.

The economic magnitude is large. An undercapitalization of 30 percent means that a homebuyer could reduce costs of home acquisition by more than 90,000 NOK, or about 17, 000 USD, by choosing a co-op unit with debt that is one standard deviation lower. The correction to full capitalization in the long run suggests a similar price impact, while a diff-in-diff coefficient of 13 percent in the short run suggests half of the impact was realized within a year. The price of an average one-bedroom apartment with debt of 170,000 NOK decreased by around 50,000 NOK, a large amount for a young, first-time homebuyer.

To corroborate the validity of our diff-in-diff approach, we perform a formal test of the parallel trend. Panel A in Table 3 shows results for the generalized model that includes the interaction of debt with dummies for all years, except the year preceding the treatment period, i.e. 2007. As the table shows, there are no statistically significant coefficients before the implementation, while effects are significant in and after 2008.

We further conduct falsification tests by repeating our analysis around other dates, and thus check whether the interaction term is significant. In Panel B of Table 3, we test equation 1 within one-year (6 months before and after) windows starting from 2003. The first six columns do so around dates mentioned in the headings for each column, thus covering transactions until July 2007, the announcement of the law. The table confirms that there is no statistically significant effect on the rate of capitalization at either of the dates.²⁷ The last column shows a regression for a window around December 31, 2008, one year after the event: while the full effect seems to have accrued over time after the event, it does seem that a statistically

²⁷ In unreported regressions we also try one-year before and after windows (covering 2 years' transactions) to make sure insignificance does not result from a low number of observations, as well as other dates not mentioned (6/2003, 6/2004, 9/2004, among others), and still see no significance. All our conclusions also hold for shorter windows.

significant, stark difference appears only around the event itself, but not later. In fact, in unreported regressions we do not notice statistically significant effects in subsequent years after the event either.

3.3.1 Self-owned units: alternative control

We now extend our analysis to the whole sample, including self-owned units. In Table 4, column 1 to 3, we first repeat our test of equation one. Since we have the whole sample, we include an additional dummy for the unit's ownership type, which will capture the differences between co-op and self-owned units that would otherwise not be absorbed by other observables.

In column 1 we observe that the diff-in-diff estimate has increased to about 32 percent. Recall that in this sample we also have a large number of units with zero or near-zero debt (self-owned units), that were not treated. In column 2 and 3 we provide additional tests and include the announcement effect. In column 2 we do so using an additional intermediate dummy: *Post2* takes value 1 for transaction after the announcement and before the implementation, and is 0 otherwise. The announcement effect is statistically significant and is equal to 12 percentage point. When compared to the pre-announcement period, post-implementation has, as expected, a slightly higher effect (-0.326). Column 3 uses *Post-announcement*, which takes value 1 for any transaction after the announcement and zero otherwise. It confirms that results also hold when the effects are evaluated in combination, as one post-treatment period.

We then move on to test our hypothesis in an alternative but reminiscent approach: we take self-owned units as a control group. Our hypothesis is that co-op unit prices would decline compared to self-owned units after the event. When debt becomes more salient, households are more likely to be aware of the total cost of the co-op unit, while for self-owned units there is no such “new” debt. After the law, buyers' estimate of the total cost would be more accurate and higher, and this should adjust the co-op prices downwards.

We estimate the impact of the event on co-op unit prices with the following equation:

$$P_i = \alpha + \beta_1 C_i + \beta_2 * Post + \beta_3 (C_i * Post) + X_i \gamma + \varepsilon_i \quad (2)$$

where C_i is a dummy that takes value one if a unit belongs to a co-op, and zero otherwise. An

important control in addition to the ones used in equation (1) is the debt of the units, and we provide specifications with and without it. We fit data to the equation in columns 4-6 in Table 4. Column 4 and 5 fit the model excluding and including debt as control, respectively. While the impact on co-op unit prices reduces somewhat in the model with debt, it still remains very high, with a magnitude larger than before: co-op unit prices decrease by around 130,000 NOK (24,000 USD) on average compared to self-owned units. In the last column, the results are confirmed using announcement of the law, rather than its implementation, as the treatment period.

3.3.2 Heterogeneity

Under our hypothesis, debt was not incorporated into prices due to lack of salience. If so, units with larger debt should undergo a larger price impact. In columns 1 to 3 in Table 5 we confirm this is the case. The first three rows, respectively, show the interaction with *Post* of dummies standing for units with debt in the second, third and the highest quartile. Column 1 provides the results for the subsample of co-op units only, column 2 incorporates all self-owned units, while column 3 saturates the model with quarter fixed effects. An average unit in the highest quartile decreases in price by around 250,000 NOK (over 40,000 USD) relative to a unit in the lowest debt quartile, that undergoes a price increase of around 600,000 NOK (*Post* in columns 1 and 2). The relative price decrease of units in the second quartiles is more than twice smaller (up to 117,000 NOK).

Some highly leveraged co-ops may have units with transaction prices so small that buyers may be able to cover the total transaction price with their own savings. This can allow homebuyers to circumvent the need for a mortgage. In these rare cases it is harder for co-ops to reject the entrants easily based on concerns for buyers' long-term financial viability. While our diff-in-diff approach and the similarity in the leverage structure around the event suggest this is unlikely to be a major driving force, we further corroborate our findings by showing this effect is marginal at best.

Specifically, to test for the importance of liquidity constraints, we break the sample into large debt units (above 90th percentile) versus low debt units. If consumers were indeed buying highly

leveraged co-ops due to mortgage constraints, then the impact of the law should be at best muted for this subsample, since buyers presumably remain constraint. In the last column, the statistically significant large coefficient for the triple interaction (140,000 NOK) means that large debt co-op units were affected to an even larger extent by the law. In addition, the fact that the highest quartile of debt underwent the largest price impact supports our conclusions, too.

Similarly, if individuals are constrained by the amount of mortgage they can get access to, we would expect to see a more moderate effect for small units. We run the model for the subsamples of medium and small (two bedrooms and smaller), and large (more than two bedrooms, as in in columns 2 and 3) apartments. As can be seen by the capitalization of debt into prices, we do not find evidence for such liquidity constraints. On the contrary, in column 4, where we add a triple interaction with small units (1 bedroom or smaller), our results show that the effect is even larger for those units.²⁸

3.4 Robustness Tests

If observable and unobservable characteristics change over time in a way that correlates with the treatment group, then our diff-in-diff estimate could be biased. For instance, if transacted high debt units are smaller in size after the event than before the event, then this could bias our diff-in-diff estimate downward (and make it larger in magnitude) in the baseline specification in Table 2. Using the sample of co-op units, in column 1 and 2 in Table 7 we interact the *Post* dummy with the observable characteristics (age, size, floor), as well as add quarter fixed effects (column 1) to account for, respectively, time-varying observables that may be correlated with *Post*, and any time-varying unobservables. Column 3 and 4 extend the sample to self-owned units. In column, 3 we first interact co-op with the *Post* along with *Post*debt* interaction, while in column 4 we add interactions with the rest of the controls. Our results remain unchanged across all specifications above.

²⁸ A more pronounced effect for small units could be driven from other characteristics of their buyers, such as little financial experience. We return to this later.

Another potential threat to identification could rise from some characteristics of co-op units that change post-treatment. To address this issue, we further restrict our sample to only units built pre-treatment. Column 5 shows that results are similar for this subsample.

Columns 6 and 7 divide the sample into before announcement and after implementation periods. We confirm a significant increase in the rate of capitalization when the interim period between announcement and implementation is omitted (slightly stronger compared to what we observe without omitting it in unreported regressions in this sample, as well as compared to the last two columns in Table 2). In the last two columns we divide the sample into before and after the event, and further saturate the model with quarter fixed effects. Overall, our results are hardly affected statistically or economically.

3.4.1 Debt in self-owned units

Debt in self-owned units is typically much smaller than debt of co-op units. The former is raised over the years typically for renovation purposes (e.g., renovation of common areas), and should be comparable to the common debt that co-op units raise for renovation.

When debt is used for renovation, its capitalization in the price should arguably be closer to zero. Indeed, the borrowed money is spent to make the unit better, and this is a cost that buyers otherwise would incur if they needed a renovated dwelling. Thus, to the extent that buyers value the renovation, such debt should be less reflected in the price reduction.

To take this into account, and isolate the initial debt (raised for development) of co-op units, we calculate the comparable debt of self-owned apartments by age categories: 1-10 years old, 10-20 years old, and so on. Assuming that co-op housing in each age group would require similar debt for renovation, we deduct the average renovation debt of the given age group from each co-op unit's total debt. We thus expect to get a close estimate of the initial debt.

We use the renovation-corrected measure of initial debt for co-ops in Table 8. Column 1 shows that our main results are as expected; initial debt is capitalized at approximately 70 p.p. but rises to just below one after the event. Another way to address the issue is to look at new apartments: indeed, debt in an average self-owned unit that is 10 years old or younger is below

5,000 NOK. Column 2 confirms a similar result: capitalization increases by around 21 percentage point after the event. Finally, in unreported regressions we divide the sample into quartiles based on the age of the building, and our results remain unchanged and similar across the groups whether we use debt or initial debt.

Columns 3 and 4 confirm our hypothesis that indeed the co-op specific debt is significantly more capitalized in co-op prices (albeit it is still far from full capitalization before the event). Self-owned debt is capitalized at a low 41-46 percent in the whole sample. Co-op debt capitalization is at around 70 p.p. before, but rises to full capitalization after the event. Thus, in self-owned housing, capitalization is much lower, as it should be, since the borrowed resources are only spent to improve the unit.

4. Financial experience, age, and income

Undercapitalization of debt can be caused due to unawareness of debt by some, but not necessarily all, homebuyers. Which type of homebuyers are likely to be less informed and/or less diligent? For instance, individuals with no investments in financial markets may be more likely to ignore co-op debt since participation in financial markets is related to financial literacy (Rooij et al., 2011; Calvet et al., 2007). Similarly, previous experience in home ownership may have eventually taught individuals about the details of their expenses, while older individuals are more likely to have been in a co-op board or have ties with a co-op member. If true, certain groups are more likely to overpay for hidden debt. Moreover, Stango and Zinman (2014) and Ru and Schoar (2015) bring evidence showing consumers with lower education and financial literacy are more likely to suffer from hidden features of credit.

In light of the above, it seems a-priori plausible that individuals with less financial experience or no previous home, as well as young buyers paid higher price for dwellings with hidden debt. To test this, we use detailed information from Statistics Norway about homebuyers' personal finances and their demographic characteristics. The database provides information on each Norwegian taxpayer's assets (e.g., cash, investments, and property), debt, and net worth. It further provides information about individuals' occupation type, sex, age, and a household

identifier, including an indicator about the head of the household.

To match this dataset with real estate transaction information, we use an individual's unique administrative number. In the dataset we have used so far this information is absent. We therefore use a different source of housing transaction data, which provides this unique key. The data is only available for the 2007-2009 period. While this is a short span, it falls around our experiment date. This dataset has all the characteristics of our main dataset except for information on the actual amount of co-op debt; however, it still does indicate whether the purchased unit is a co-op or a self-owned unit.²⁹

Finally, our dataset has building identifiers, allowing us to use building-level fixed effects and thus absorb any unobserved heterogeneity that may exist across various buildings and change in a way that is correlated with either *Post* or *co-op* dummy. This seems to be unlikely given the short window periods we use, as well as given the unilaterally high standards of housing across Oslo's districts. We still absorb those potential unobservables by adding building fixed effects, and confirm that results hardly change to a significant extent. We provide the baseline test of equation 2 in the Appendix (Table A1), where one can see that the correlation coefficient indeed increases compared to our tests in Table 3. Yet the correlation coefficient was already quite high at around 80 percent even without using building fixed effects.^{30,31}

In columns 1 and 2 of Table 9 we test whether young homebuyers are more likely to overpay for a leveraged co-op unit before the law. Young homebuyers are those who are under 35 years old (the median age in the dataset). In column 1 we notice that on average they pay less for self-owned units, but not for co-op units: thus, young homebuyers exhibit a stronger demand for co-op units than for self-owned units, which can be explained by certain characteristics of co-ops

²⁹ A further difference in the dataset is that it has a different recorded date of the transaction. While in one case it is the date when the deal is finalized, in the other it is the date when the transaction is reported to the administrator. The difference can span from several days to a few months, thus not allowing us to merge the information with our initial transaction dataset. Since we do not merge, we no longer use the actual co-op debt, but only co-op identifier to distinguish between the control and the treatment group.

³⁰ Note that in Table A1 correlation coefficient is around 60 percent without building fixed effects, lower than in Table 3, since we do not have debt in the explanatory variables, which is a major determinant of price.

³¹ The number of observations that come from buildings with only one transaction within one year window, or with more than one transaction but no transaction after the event, is only 0.9 percent. Exclusion of those do not alter our results.

that are more sought for among the young. To test whether these characteristics include bias for hidden debt, which they may (unintentionally) pay premium for, in column 2 we add a triple-interaction term with the *Post* dummy: young homebuyers pay approximately 160,000 NOK less after debt becomes salient. This suggests that hidden debt is a significant reason for overpaying. The regulatory reform benefited young buyers to a larger extent.

We also know whether the homebuyer owned any real estate starting 2004. We define first-time homebuyers as those who did not previously own a home (as of 2004) and are young. In columns 3 and 4 we control for income and show that these first-time homebuyers suffer from the same bias and that the amount they pay is reduced by over 130,000 NOK after the reform.

Finally, we examine homebuyers who previously did not have financial investments. We define a dummy variable that takes value 1 if the homebuyer does not have any investment in stocks, bonds or derivatives. As column 5 shows, they are more likely to overpay. However, this is not the case after the event (column 6). These results suggest that inattention to hidden debt is related to lack of financial experience, and arguably to financial literacy. The bias benefited the wealthier market participants, at the expense of the less well-off, with mistakes stemming from very large transactions.

Our triple-difference results are also robust to using short-time windows (6 months), as well as time and building fixed effects. Columns 1-3 in Table 10 show this for the case of financial investments. Furthermore, the results also survive when we include other measures that can be correlated with financial literacy or attention, such as income. Columns 4-6 repeat the analysis by replacing *No investment* dummy by a *Low income* dummy, defined as 1 for those in the lowest quartile of income distribution (and 0 otherwise).

We further explore how factors affecting demand by homebuyers for the two unit types change around the event date. If demand from these two groups does not move in parallel, the results that we find may in fact reflect something else. To shed more light on the issue, we present descriptive statistics on homebuyers' income, age and financial experience before and after the event. Table 11 shows that there is hardly any differential change in those observables: while average income of co-op buyers was around 20 percent lower than those of self-owned

homebuyers, this comparison remains nearly unchanged after the law. Similarly, the ratio of individuals with financial investment was twice as high among self-owned homebuyers before (25 vs. 12), and did not change much after (28 vs. 14). Additionally, when we look at the number of homebuyers in the two groups in 2007 and 2008, we see only insignificant switching from co-op units to self-owned units.

5. Discussion and concluding remarks

In many countries household debt has increased greatly over the last decades. So have household assets, particularly in the form of housing wealth. An improved understanding of how households use debt to finance homeownership is central to household finance.

We provide evidence that increased salience of debt in mortgage transactions has a significant effect on prices: a mandatory disclosure of debt nearly eliminates the bias for hidden debt and the mispricing caused by it. The increased salience reduces the relative prices of co-op units with high-debt and allows households to estimate more accurately the actual cost of the property. Salience could reduce households' total debt, which can be larger due to unintentional borrowing.

Our results provide important policy implications. They show a preference for co-op debt over personal debt in the presence of hidden debt (but not in its absence), for which some market participants unknowingly pay a premium. We show that this preference is driven entirely by salience, rather than other biases or liquidity constraints. Young, first-time, low income homebuyers paid the highest premium, arguably to the benefit of richer homebuyers. Thereby, the regulation on debt disclosure in 2008 benefited more the former groups, which seem even less informed about hidden co-op debt. This means that in such large consumer transactions significant welfare effects from redistribution can be obtained with appropriate policy measures, such as increasing salience, as well as promoting financial education.

Taking policy measures should above all incorporate the provision of detailed information about the size of the various loans, thereby facilitating comparison between potential

acquisitions. While co-op loan can be inferred from other sources (e.g., co-op's annual statement), it seems likely that consumers were ignorant to gather and lacked enough exposure to such information, at a time when necessary calculations were being made. Providing this information in the advertisement itself does serve as a timely reminder to consumers that the total transaction value is the sum of transaction price and debt.

In addition to the size of the loan, its servicing terms too can have a significant impact on a homebuyer's debt-servicing capacity. In many cases a part of the co-op loan's costs may kick only in the future, and so the buyer observes only "part" of the future monthly fee. The monthly fee can allow consumers to evaluate their capacity to acquire the unit. Yet, it indeed has often been misleading in practice: sellers fail to provide important information regarding major increases in fees when, for instance, repayment of the principal loan comes into effect years after the transaction, and adds up to the interest payments.³² This means that even reasonable attention and inquiry about the monthly fee may not guarantee the homebuyer access to information about future changes in those fees. Systematic occurrence of such instances is likely to increase the probability of default, bringing about challenges at the level of the macroeconomy.

Could it be that homebuyers in fact get additional disutility from information about indebtedness? Indeed, lower debt salience may carry hedonic benefits for which some people are willing to pay. As Almenberg and Karapetyan (2014) note, increasing salience after purchase, by for instance itemizing co-op interest and co-op maintenance costs separately in the monthly fee, could reduce the well-being of individuals with a preference for low salience and increase that of others. Consistent with this recent evidence finds that investors are less likely to check their portfolio balances following market declines (Sicherman et al., 2015). This tendency to avoid bad news is labelled as the *ostrich effect*. While an ostrich effect may potentially be present in housing decisions to some extent, with borrowers paying a premium for hidden debt, the large price response to the increased salience does suggest borrowers were

³² Anecdotal evidence suggests majority of sellers use such practice. A handful of articles in major newspapers in Norway describe this. For instance, see <http://www.dn.no/privat/eiendom/2006/09/05/fellesgjeldbloff-fra-meglere> (in Norwegian), where a realtor claims "This is a completely normal way of doing it. I think that 90 percent of us do so."

likely doing so unknowingly. Moreover, if sale advertisements accurately disclose the co-op debt, few individuals with such a preference would make mistakes ex-ante.

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Table 1: Summary statistics

Panel A: All sample.

VARIABLES	Self-owned		Co-op		t-test
	mean	s.d.	mean	s.d.	
Price (1000 NOK)	2,908	1,802	1,694	759	1,213***
Debt (1000 NOK)	27.4	62.9	168	309	-141***
Area (sq. meter)	75.6	41.0	61.7	19.5	13.89***
Floor	2.92	1.66	3.07	2.12	-0.15***
Rooms	1.89	0.92	1.71	0.76	0.17***
Age of building	58.5	38.3	54.1	28.6	4.39***

Panel B: Low-debt is the lowest quartile and high-debt is the highest quartile.

VARIABLES	Low-debt co-op		High-debt Co-op		t-test
	mean	s.d.	mean	s.d.	
Price (1000 NOK)	1,794	832	1,611	741	183***
Debt (1000 NOK)	25.9	14.7	431.3	447	-405.4***
Area (sq. meter)	59.6	20.1	63.2	19.9	-2.69***
Floor	3.35	10.1	3.08	4.04	0.27***
Rooms	1.64	0.77	1.78	1.03	-0.14***
Age of building	54.4	25.4	51.7	37.5	2.69***

Panel C: Before event (2007) and after event (2008) comparison.

VARIABLES	2007		2008		2007		2008	
	Low-debt co-op		High-debt co-op		Low-debt co-op		High-debt co-op	
	mean	s.d.	mean	s.d.	mean	s.d.	.mean	s.d.
Price (mln NOK)	1.89	0.75	1.92	0.88	1.67	0.72	1.58	0.66
Debt (1000 NOK)	20.5	14.3	21.0	14.4	491	440	428	406
Area (sq. meter)	60.1	20.8	61.0	20.8	62.1	20.7	63.1	19.2
Floor	3.97	21.2	3.09	2.31	3.04	1.85	3.11	2.12
Rooms	1.65	0.77	1.70	1.02	1.76	1.30	1.76	0.76
Age of building	56.7	93.7	58.5	87.6	57.4	119	56.5	117

Table 2: Impact of the law on co-op price correction: capitalization of debt

Dependent variable is the transaction price of dwellings. Column 1, 2 and 3 include transactions taken place 1, 6, and 12 months before and after the implementation date (January 1, 2008). Column 4 includes the whole sample of co-op units. Column 5 and 6 divide the sample into before and after the event. Post is a dummy that is one if the transaction took place after the law implementation. Dummies for the floor of the unit, as well as district and time fixed effects are not reported. Robust standard errors are in parenthesis.

VARIABLES	(1) 1 month window	(2) 6 month window	(3) 1 year window	(4) All co-op sample	(5) Before	(6) After
Post*debt	-0.130* (0.0720)	-0.114*** (0.0401)	-0.129*** (0.0298)	-0.262*** (0.0200)		
Post	22,220 (24,629)	38,805* (21,758)	-105,086*** (7,383)	568,906*** (4,500)		
Debt	-0.803*** (0.0576)	-0.808*** (0.0238)	-0.830*** (0.0196)	-0.693*** (0.0194)	-0.794*** (0.0244)	-1.032*** (0.0122)
Meter	21,717*** (4,117)	15,708*** (2,831)	16,022*** (1,903)	15,292*** (1,089)	16,181*** (1,135)	15,077*** (1,172)
Meter sq.	-3.385 (34.14)	75.61*** (23.45)	67.97*** (15.81)	58.37*** (9.383)	21.99** (9.775)	100.5*** (10.05)
2 rooms	36,956 (49,998)	96,773** (45,128)	117,642*** (37,248)	102,198** (40,979)	96,167** (40,833)	127,452** (53,123)
3 rooms	66,988 (62,437)	115,227** (46,462)	149,560*** (38,135)	149,537*** (41,218)	120,392*** (41,043)	185,533*** (53,457)
4 rooms	102,875 (79,071)	62,578 (49,015)	104,530*** (39,952)	113,635*** (42,003)	57,199 (41,998)	162,130*** (54,406)
5 rooms	-508,589*** (187,441)	-105,372 (106,930)	-35,567 (80,787)	-8,817 (54,811)	-33,173 (52,724)	92,613 (74,536)
>5 rooms		113,414** (44,478)	125,657*** (37,030)	73,063* (40,872)	78,378* (40,853)	77,303 (52,910)
10-20 years old	73,291 (148,156)	-105,376** (41,575)	-124,189*** (30,019)	-283,113*** (17,193)	-213,160*** (17,680)	-99,036*** (22,349)
20-30 years old	-26,655 (102,072)	-167,954*** (34,421)	-172,581*** (25,795)	-342,149*** (14,881)	-257,234*** (17,453)	-224,947*** (18,536)
30-40 years old	-342,981*** (85,157)	-420,673*** (30,180)	-419,911*** (22,937)	-364,807*** (13,827)	-314,129*** (16,788)	-465,577*** (16,941)
40-50 years old	-177,217** (78,115)	-252,731*** (26,318)	-250,563*** (20,336)	-327,691*** (12,589)	-269,624*** (16,159)	-318,591*** (14,929)
>50 years old	-204,512*** (71,392)	-210,116*** (23,571)	-208,519*** (18,676)	-222,927*** (11,676)	-215,541*** (15,357)	-282,483*** (12,989)
Quarter FE	N	N	N	N	Y	Y
District FE	Y	Y	Y	Y	Y	Y
Observations	714	6,280	11,722	67,212	36,154	31,058
R-squared	0.783	0.770	0.778	0.672	0.775	0.803

Table 3: Parallel trend and falsification test.

Panel A: Parallel trend.

The table shows regression results in the generalized model that includes interaction of treatment (debt) with all years except the pre-treatment year (2007). Controls, time and district fixed effects are not reported. Robust standard errors are in parentheses.

VARIABLES	
Debt	-0.819*** (0.0183)
Debt*year2002	0.247 (0.186)
Debt*year2003	-0.229 (0.211)
Debt*year2004	-0.0508 (0.0400)
Debt*year2005	0.0187 (0.0310)
Debt*year2006	-0.0156 (0.0269)
Debt*year2008	-0.0966*** (0.0307)
Debt*year2009	-0.148*** (0.0263)
Debt*year2010	-0.174*** (0.0218)
Debt*year2011	-0.189*** (0.0250)
Debt*year2012	-0.213*** (0.0298)
Quarter FE	Y
District FE	Y
Contorls	Y
Observations	67,212
R-squared	0.787

Panel B: Falsification test.

The first 6 columns show regression results before the event for transactions around last day of the month specified in the heading, over a one-year period (+/- 6 months). The last column shows the results around 12/31/2008, 1 year after the event. Controls and district fixed effects are not reported. Robust standard errors are in parentheses.

VARIABLES	(1) 12/2003	(2) 12/2004	(3) 6/2005	(4) 12/2005	(5) 6/2006	(6) 12/2006	(7) 12/2008
Debt	-0.764*** (0.0859)	-0.694*** (0.0688)	-0.750*** (0.0603)	-0.791*** (0.0536)	-0.818*** (0.0528)	-0.823*** (0.0612)	-0.942*** (0.0341)
Post*debt	-0.0256 (0.0316)						
Post	54,104*** (12,025)						
Post*debt		-0.0317 (0.0579)					
Post		63,785*** (10,027)					
Post*debt			-0.0488 (0.0505)				
Post			61,683*** (9,643)				
Post*debt				0.00287 (0.0509)			
Post				140,902*** (10,610)			
Post*debt					-0.0688 (0.0694)		
Post					183,369*** (10,213)		
Post*debt						0.0189 (0.0566)	
Post						106,539*** (9,869)	
Post*debt							-0.0582 (0.0429)
Post							54,752*** (12,031)
District FE	Y	Y	Y	Y	Y	Y	Y
Contorls	Y	Y	Y	Y	Y	Y	Y
Observations	5,026	6,575	7,002	7,400	7,988	7,304	4,509
R-squared	0.723	0.721	0.737	0.742	0.752	0.772	0.776

Table 4: All sample: alternative control and announcement effect

Dependent variable is the transaction price of dwellings. Column 1(2) test the benchmark model the whole sample, including self-owned units. Column 3 and adds the announcement effect: *Post2* (=1 if a transaction takes place between announcement and implementation date, and is 0 otherwise). Column 4-6 use self-owned units as treatment group (equation 2). Controls for the unit include the area, squared area, number of rooms, floor, category (decade) for the age of the building. Regional dummies and time fixed effects are not reported. Robust standard errors are in parentheses.

VARIABLES	(1) All sample	(2) All sample	(3) All sample	(4) All sample	(5) All sample	(6) All sample
Post *debt	-0.316*** (0.0498)	-0.326*** (0.0579)				
Post2*debt		-0.124** (0.0470)				
Post announcement*debt			-0.317*** (0.0482)			
Post*coop				-181,969*** (48,881)	-122,799* (60,445)	
Post announcement*coop						-134,311** (60,389)
Debt	-0.724*** (0.0316)	-0.717*** (0.0364)	-0.717*** (0.0373)		-0.948*** (0.0198)	-0.945*** (0.0203)
Coop	-122,368*** (24,118)	-115,114*** (25,269)	-114,688*** (25,302)	-207,748*** (46,118)	-50,621 (44,225)	-33,901 (46,120)
Post-event dummy	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Observations	119,372	119,372	119,372	119,372	119,372	119,372
R-squared	0.777	0.780	0.781	0.753	0.776	0.780

Table 5: Robustness: high vs. low debt units

Dependent variable is price of transaction. Specifications in column 1 to 3 include dummies for transactions in the 2nd (debt2), 3rd (debt3), and 4th (debt4) quartiles. Column 4 includes a further interaction with units that have debt in the highest decile.

VARIABLES	(1) All coop	(2) All sample	(3) All sample	(4) All sample
Post*debt2	-55,785 (45,134)	-81,134** (30,256)	-117,168*** (30,375)	
Post*debt3	-93,623* (50,710)	-123,716*** (28,384)	-145,185*** (29,013)	
Post*debt4	-236,565** (81,832)	-239,982*** (60,913)	-271,102*** (58,809)	
Debt2	5,687 (34,327)	28,440 (24,597)	3,218 (23,377)	
Debt3	-50,670 (33,310)	-44,171 (27,668)	-61,470** (21,397)	
Debt4	-183,976*** (38,995)	-244,585*** (35,176)	-308,881*** (36,747)	
Post	599,895*** (54,121)	655,579*** (54,781)		662,303*** (6,223)
Coop		-190,650*** (24,646)	-111,105*** (27,075)	-122,262*** (5,553)
Large debt				-102,673 (69,176)
Post*large debt				-105,622 (73,692)
Large debt*coop				-287,880*** (69,674)
Post*coop				-110,485*** (7,205)
Post*large debt*coop				-140,297** (58,566)
Quarter FE	N	N	Y	N
District FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y
Observations	67,212	119,372	119,372	119,372
R-squared	0.615	0.762	0.814	0.766

Table 6: Heterogeneity in size

The dependent variable is the price of the unit. The model is run for the subsamples of small and medium (2 bedrooms and smaller), large apartments in columns 1, 2, 3 below. Small unit is a dummy standing for units that are 1 bedroom or smaller. Regional dummies and controls are not reported. Robust standard errors are in parentheses.

VARIABLES	(1) < 3 rooms	(2) 3 rooms	(3) > 3 rooms	(4) small (<2)
Post*debt	-0.325*** (0.0459)	-0.342** (0.133)	-0.102 (0.128)	-0.231*** (0.0619)
Debt	-0.690*** (0.0360)	-0.759*** (0.0912)	-0.887*** (0.103)	-0.842*** (0.0448)
Post	582,410*** (43,273)	965,593*** (123,990)	1.193e+06*** (176,598)	711,708*** (63,332)
Coop	-130,932*** (20,361)	-117,872* (67,008)	42,364 (142,963)	-127,203*** (24,276)
Small unit*debt				0.312** (0.110)
Post*small unit				-212,688*** (33,517)
Post*small unit*debt				-0.219*** (0.0708)
District FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y
Observations	95,154	14,860	1,689	119,372
R-squared	0.752	0.775	0.810	0.780

Table 7: Robustness

Column 1 and 2 control for the interaction of units' size, age and floor with *Post* dummy. Column 3 and 4 enlarge the sample and include self-owned units. In column 3 we control for the interaction of debt with the post-event dummy, in addition to the post-event and coop dummy. Column 6(8) takes the subsample of transaction before the announcement (implementation), while 7 and 9 take those after the implementation. The usual controls and their interactions are not reported.

VARIABLES	(1) Co-op	(2) Co-op	(3) All sample	(4) All sample	(5) Units built before 2008	(6) Before	(7) After	(8) Before	(9) After
Post*debt	-0.205*** (0.0378)	-0.314*** (0.0371)	-0.247*** (0.0394)	-0.282*** (0.0354)	-0.297*** (0.0200)				
Debt	-0.801*** (0.0428)	-0.646*** (0.0395)	-0.767*** (0.0421)	-0.725*** (0.0345)	-0.749*** (0.0162)	-0.685*** (0.0270)	-1.016*** (0.0104)	-0.710*** (0.0273)	-1.016*** (0.0366)
Post		10,717 (60,254)	673,437*** (73,913)	210,648*** (50,014)	698,214*** (5,622)				
Post*coop			-89,721 (65,376)	38,600 (36,892)	-91,451*** (7,769)				
Coop			-73,046 (45,828)	-144,980*** (32,391)	-83,604*** (6,291)	-206,566*** (21,884)	-60,579*** (6,265)	-197,106*** (20,150)	-60,579*** (24,243)
Quarter FE	Y	N	N	N	N	N	N	Y	Y
District FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls*post	Y	Y	N	Y	N	Y	Y	Y	Y
Observations	67,212	67,212	119,372	119,372	117,078	49,732	62,802	56,570	62,802
R-squared	0.808	0.689	0.778	0.793	0.776	0.729	0.755	0.823	0.845

Table 8: Robustness, initial debt vs. debt for renovation

Initial debt is current co-op debt less proxy debt that raised for renovation. Column 1 and 2 include the whole sample, column 3(4) restricts transactions to those before (after) the event. Regional dummies and controls are not reported.

VARIABLES	(1) All coop	(2) New units	(3) Before	(4) After
Post*initial debt	-0.262*** (0.0321)			
Initial debt	-0.693*** (0.0362)			
Post	568,906*** (32,712)	576,064*** (27,624)		
Post*debt		-0.213*** (0.0499)		
Debt		-0.746*** (0.0513)	-0.465*** (0.0937)	-0.407*** (0.114)
Coop*debt			-0.251** (0.0919)	-0.591*** (0.117)
Coop			-168,089*** (19,001)	-40,365 (25,541)
District FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y
Observations	67,212	3,129	44,728	9,547
R-squared	0.670	0.798	0.758	0.824

Table 9: Effects of financial experience, age, and first-time homebuyers

Young are those homebuyers who are less than 35 years old. *No investment* is 1 if the homebuyer has no investments in financial markets, and 0 otherwise. *First-time* homebuyers (first time) are those who have not owned a dwelling since 2004. Controls, as well as time fixed effects are not reported.

VARIABLES	(1) All sample	(2) 1 year window	(3) All sample	(4) 1 year window	(5) All sample	(6) 1 year window
Young	-419,114*** (23,203)	-541,796*** (45,717)				
Young*coop	418,401*** (24,676)	512,176*** (49,828)				
Young*post		167,575*** (54,948)				
Young*post*coop		-160,130*** (60,927)				
First time			-418,634*** (22,876)	-532,336*** (45,117)		
First time*coop			421,207*** (24,390)	511,378*** (49,520)		
First time*post				133,594** (54,041)		
First time*post*coop				-131,580** (60,299)		
No investment					-257,054*** (30,166)	-384,435*** (36,065)
No investment*coop					132,357*** (26,473)	319,435*** (55,990)
No investment*post						318,238*** (43,070)
No invest*post*coop						-214,182*** (64,229)
Post		-184,152*** (47,570)		-166,383*** (46,384)		-387,910*** (34,884)
Post*coop		85,202 (52,994)		71,225 (52,036)		84,299 (55,015)
Quarter FE	Y	N	Y	N	Y	N
District FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Observations	42,736	28,925	42,736	28,925	42,736	28,925
R-squared	0.574	0.600	0.574	0.600	0.571	0.570

Table 10: Robustness

Dependent variable is transaction price of units. Column 1 and 4 use 6 month before and after windows around implementation of regulation, while other columns use the whole sample. *Low income* is dummy equal to one for a buyer whose income belongs to the lowest quartile. The fixed effects are mentioned in the table.

VARIABLES	(1) 6 month window	(2) All sample	(3) All sample	(4) 6 month window	(5) All sample	(6) All sample
No investment	-308,271*** (40,694)	-71,845*** (18,847)	-67,962*** (18,195)			
No investment*coop	324,815*** (63,301)	57,039** (28,644)	51,347* (27,687)			
No investment*post	168,138*** (55,609)	80,145*** (22,974)	43,014* (23,763)			
No invest*post*coop	-149,180* (88,992)	-58,933* (33,353)	-56,806* (32,366)			
Low income				-76,107*** (13,776)	-59,215*** (15,801)	-59,021*** (15,736)
Low income*post				54,334*** (20,029)	31,517 (19,452)	31,032 (19,377)
Low income*post*coop				-70,571** (28,233)	-64,278** (25,820)	-61,776** (25,713)
Low income*coop				76,683*** (20,451)	67,929*** (20,937)	65,594*** (20,850)
Post*coop	26,362 (81,763)	25,244 (29,619)	25,169 (28,690)	23,172 (17,244)	-930.7 (16,481)	-120,240*** (20,086)
Post	-175,952*** (49,166)	-122,980*** (19,906)		-149,885*** (22,226)	-119,976*** (20,167)	
Quarter FE	N	N	Y	N	N	Y
Building FE	N	Y	Y	Y	Y	Y
District FE	Y	N	N	Y	N	N
Observations	15,228	42,727	42,727	15,228	42,736	42,736
R-squared	0.906	0.917	0.923	0.653	0.920	0.920

Table 11: Borrower characteristics by type of acquired dwelling

VARIABLES	2007				2008			
	Self-owned		Co-op		Self-owned		Co-op	
	mean	s.d.	mean	s.d.	mean	s.d.	mean	s.d.
Wages or pension (1000 NOK)	443,232	489,517	344,177	222,758	482,548	488,921	383,803	242,950
No investment	0.75	0.43	0.88	0.32	0.72	0.45	0.86	0.35
Young (<35) homebuyers	0.49	0.50	0.63	0.48	0.45	0.50	0.57	0.49
Observations	6,645		8,259		6,498		7,562	

Appendix

Figure A1. Mean transaction prices for unfinished units (about 1 percent of the total transactions).

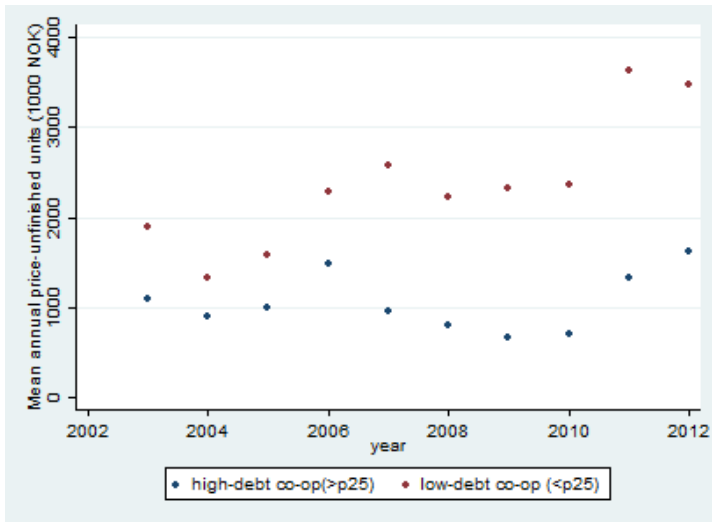


Figure A2. The graph to the left (right) shows the proportion of co-op units sold across the different regions in Oslo before (after) December 2007.

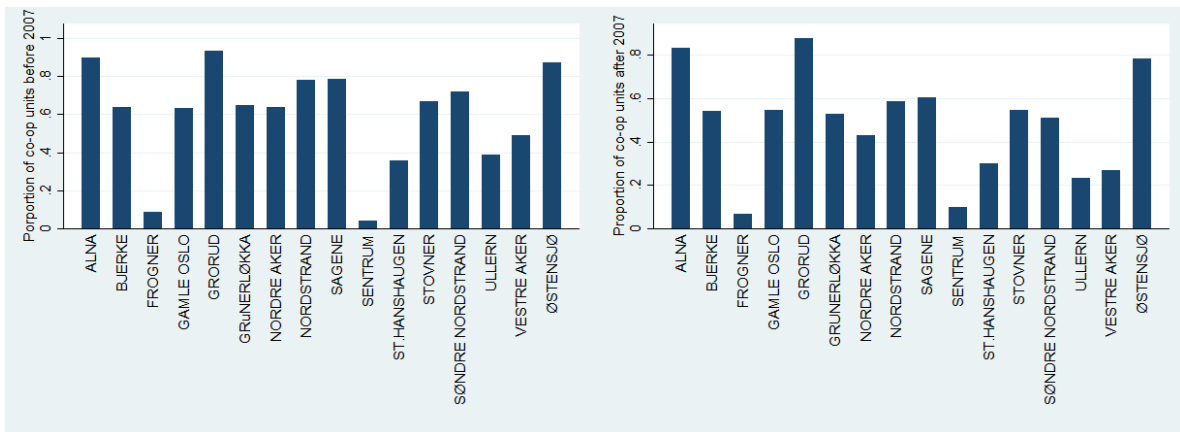


Table A1. Dependent variable is transaction price of units. Column 1 and 2 use 6 month before and after windows around implementation of regulation, while columns 3-6 use 1 year. The fixed effects are mentioned in the table.

VARIABLES	(1) 6 month window	(2) 6 month window	(3) 1 year window	(4) 1 year window	(5) 1 year window	(6) 1 year window
Post*coop	-40,695* (24,315)	-38,422* (23,831)	-74,885*** (28,939)	-61,806** (25,306)	-69,353** (28,912)	-54,312** (23,272)
Post	-68,723** (27,200)	-30,132 (21,746)	-29,151 (22,100)	-26,962* (15,633)		
Coop	-790,862*** (34,762)		-822,646*** (27,566)		-786,549*** (27,529)	
Quarter FE	N	N	N	N	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Building FE	N	Y	N	Y	N	Y
District FE	Y	N	Y	N	Y	N
Observations	15,228	15,228	28,925	28,925	28,925	28,925
R-squared	0.688	0.924	0.666	0.926	0.668	0.926