Information Salience and Mispricing in Housing

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Abstract

It has been shown that making the full purchase price salient to consumers affects demand and equilibrium prices in various markets (Chetty et al., 2009; Finkelstein, 2009). In a setting where part of the home-acquisition price is in form of shrouded debt, we show that making the full price salient has a significant price effect in the market for households’ largest acquisition: a mandatory display of the shrouded debt and the full price eliminated a large mispricing in housing. Before the regulation, an average homebuyer would lose about $13,000 by acquiring a dwelling with one-standard-deviation ($51,000) higher debt. By matching with individual-level administrative data, we show young, financially inexperienced and first-time homebuyers used to pay the largest price. We provide a number of robustness tests, reject rational explanations, and confirm lack of salience was the main source of the mispricing.

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

JEL Classification: D12, G21, G32

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1. Introduction

Like many consumer financial decisions, buying a house can be complex. Consumers need to consider the relevant financial information and make an optimal decision. Although a number of consumers make rational decisions, others may be inattentive to key information and optimize imperfectly. Indeed, recent evidence suggests poor financial decisions are widespread in mortgage and other credit markets (Campbell et al., 2011; Agarwal et al., 2017).

In the case of mortgage markets, the failure of households to optimize entails substantial costs. Buying a house is one of the largest financial decisions a household makes. Despite the large costs, recent research from the United States 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, 2016).²

In this paper, we examine consumers’ choice regarding their home acquisition in a setting where the total price in some homes (but not in others) has both a salient attribute (i.e., it stands out and is considered important), as well as a shrouded one (i.e., it is ‘in-salient’ or unnoticeable, and is either ignored or draws the decision maker’s attention to a lesser extent than rational). Earlier work has shown that shrouded information changes behavior: consumers adjust spending depending whether taxes are included in the price tag (and so are salient) or applied at the counter (Chetty et al., 2009), or as borrowers are reminded more about overdraft fees, they are more attentive and likely to avoid them (Stango and Zinman, 2014). We show that households make surprisingly large mistakes that stems from information salience and inattention at the time of their largest transaction - buying a home.

Our study is based on data from Norway, where approximately 40% of all apartments are cooperative (co-op) units. Co-ops borrow debt to finance the development of buildings. As a result, a household acquiring a leveraged co-op unit needs to take into consideration both the transaction price (financed by a mortgage and/or equity) and the co-op debt (the loan taken earlier by the co-op). In effect, the total acquisition price is the sum of the two. Prior to 2008, there was no law requiring real-estate agents to

²See Campbell (2006) on the importance of mortgage markets and suboptimal decisions.
disclose co-op debt at the time of sale. Nonetheless, homebuyers must service the debt, just like their personal mortgage. To service the debt and its interest payments, homeowners must pay a monthly fee that also includes capital and maintenance expenses, as well as utility fees. Every homeowner in Norway pays these common (or ownership) costs. However, the separate costs were not itemized in the fee, contributing to the lower salience of co-op debt.

On January 1, 2008, a regulatory change was implemented, requiring that the terms of all sources of debt in dwellings be shown to buyers. In particular, the regulation required that the amount of co-op debt, as well as its servicing terms be visible at the time of sale, along with the transaction price, as well as the total price. After the law, the main online platform that facilitates sale of homes would announce co-op debt next to the transaction price, along with the calculation of the total price, which is the practice until now. This ‘disclosure’ law should hardly affect units with little or no debt. On the other hand, it should reduce prices of co-op units with moderate or high debt if homebuyers—at least some of them—failed to incorporate it. We study the price impact of this regulatory change, and confirm a large effect, suggesting the mispricing mainly stemmed from in-salient debt.

We begin our analysis by examining the impact of the regulation on co-ops with various amounts of debt. We hypothesize 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. Our hypothesis is built on the presumption that at the time of acquisition some homebuyers were unaware of the amount of debt (even if they knew that debt could exist), or simply failed to consider it as an equally important part of the total cost. Such homebuyers could exist because, first, the ownership costs homeowners pay monthly did not disclose the amount serviced for debt (nor the amount of debt): second, not all (or not enough) sellers had voluntarily advertised their (low) co-op debt. Indeed, as we

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3 These are referred to as felleskostnader’ or ‘bokostnader’, i.e., common or ownership expenses in translation.
4 We abuse the term ‘disclosure’ to mean ‘show it as clearly as the transaction price’. The information could be obtained at alternative sources, such as the prospectuses, co-op balance sheets, via inquiry about composition of common fees.
5 In fact, disclosure may not always happen even for low-debt co-ops, and even in the presence of costless information revelation: Gabaix and Liabson (2006) show that in the presence of enough myopic consumers, there is an equilibrium where firms would not disclose information to attract other firms’ customers.
show, such homebuyers benefited from the disclosure law, while others were nearly unaffected, i.e. they were likely aware of the debt as a component of the full price.

To test this hypothesis, we first compare the extent to which co-op debt is reflected in market prices (the capitalization rate) both before and after the law. 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 was undercapitalized (i.e., debt was less than fully reflected) in apartment prices by around 18-26%, in most specifications.\(^6\) By choosing a co-op unit with a one-standard-deviation lower debt (281,000 NOK), a homebuyer could reduce her total price of home acquisition by up to 73,000 NOK, or about 13,300 USD.\(^7\) After the regulation, undercapitalization decreases by around 11-13 percentage points (or by about 50%) within short windows of up to one year. In the long run, undercapitalization vanishes almost entirely. The corresponding price impact of the regulation can be seen in Figure 1: high-debt co-op units started to exhibit a lower relative price in 2007. Finally, as a control group we further use self-owned units, which is the other dominant form of apartments. They differ from co-op units in that there is no debt at the time of development. Our results continue to hold, with a larger price impact in the whole sample.

In the presence of shrouded debt, some cohorts of consumers, such as financially experienced or repeat homebuyers, may be more aware of co-op debt and its importance than others. For instance, attentive buyers may well wonder why one co-op fee is higher than another, and acquire information about the comprising items. Others, due to lack of knowledge about debt and/or their full liability to it, may have just focused on the transaction price (or a combination of price and the common ownership fee, such as the total monthly mortgage service and the common fee); these buyers suffer from salience bias.

To test whether lack of salience can have different impact for homebuyers with various characteristics, we match the transaction data with administrative data from Norway, covering demographic and financial information on all Norwegian taxpayers. We find that young homebuyers, first-time

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\(^6\) Undercapitalization was earlier shown in Hjalmarsson and Hjalmarsson (2009), and in Almenberg and Karapetyan (2014). We return to these later.

\(^7\) 1 USD was around 5.5 NOK during the period.
homebuyers, and individuals with no investments in financial markets pay the largest price for the 
shrouded debt (and thus benefit the most from its disclosure). Using a triple difference-in-differences 
(diff-in-diffs) estimation, we find that young buyers pay around 6.5% less for co-ops after the disclosure 
of co-op debt, or about 19,000 USD. Similarly, compared to their benchmark groups buyers without 
financial investments and those with no prior home ownership benefit by comparable amounts.

We corroborate our results by addressing remaining endogeneity concerns. We carry out a number of 
robustness and falsification tests, as well as address selection issues. To minimize the effects of 
confounding factors, we focus on observations immediately preceding and succeeding the regulation, 
using short (starting 30 days) windows, and demonstrate both the statistical and economic significance 
of the impact on prices. Because a large price correction takes place shortly after the disclosure law, 
with a significant impact on some groups but not others, we conclude lack of salience was indeed the 
major source of the mispricing, and the resulting suboptimal decisions.

The global financial crisis coincides with our event period, and a key issue is whether the value for 
underlying co-op debt could change in a way that is correlated with the treatment. This could happen if 
co-ops with different leverage faced unequal refinancing terms. We address this issue by acquiring 
information on co-op interest rates over the crisis, and show almost identical change in rates across 
control and treatment groups. Furthermore, the two groups may attract different cohorts of buyers whose 
demand would not necessarily move in parallel over the crisis. If demand changes for any reason other 
than the salience of debt during the period, then the cohort heterogeneity effects may to some extent 
reflect those factors. To alleviate these concerns, we directly control for buyers’ financial information, 
as well as match them on a number of characteristics in various specifications. Overall, we show strong 
robustness, both qualitatively and quantitatively.

An important omitted variable is the unobserved heterogeneity in quality: 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, could bias our results. This is hardly a significant concern, since our diff-in-diffs analysis 
shows a price correction in short windows. We further refine our approach by first accounting for
approximate renovation expenses. Second, we analyze old and new buildings separately, since major renovations are likely to have occurred only in old buildings. Finally, we use building-level fixed effects and confirm our results.

Closest to our work, Hjalmarsson and Hjalmarsson (2009) and Almenberg and Karapetyan (2014) show undercapitalization in housing using data from Sweden. Hjalmarsson and Hjalmarsson (2009) rely on data about the common fee, as a proxy for debt. Yet, on top of servicing the debt, the fee includes maintenance and utilities expenses that may vary across units. Almenberg and Karapetyan (2014) contribute by combining housing transaction data with co-op balance sheets, and show that co-op debt is far from being fully capitalized into the sales price.\(^8\) However, unlike our work, the source of the mispricing is not shown in these studies. Almenberg and Karapetyan (2014) bring survey evidence, in which a majority of individuals (about 70 out of 100 surveyed) are unaware of the amount of debt their co-op has taken, which is consistent with our empirical finding that salience is the source. While suggesting shrouded debt is important, the authors suggest rational choice (e.g., liquidity constraints) or other biases could also account for the mispricing.

Could undercapitalization be driven by low-income buyers acquiring high-debt co-op, thus circumventing the need for a mortgage? Could it be rational, because co-op loans have different interest rates, or adverse selection is high? Or perhaps homebuyers are not fully liable for it, either due to co-op or homeowner default? After all, this is a large transaction and buyers must have spent sufficient resources. We exclude these alternative explanations. By arguing that equal value of co-op debt and price is the rational benchmark, we show that salience bias was the source by finding a one-to-one relationship after the shock to salience. Precisely, we find that without changing actual costs for owning

\(^8\) The mispricing they find is much larger in fact over 60 percent in most specifications. Yet there are two differences from us: first, debt is measured by using total co-op debt and the apartment’s share of the area in the co-o, which is accurate only for new houses, as individuals may decide to pay back their debt if they wish. Second, Sweden had at the time only co-ops, while in Norway competition with self-owned units can deter sophisticated buyers (and some biased ones) to self-owned units if co-op asking price increases too high. Proxying debt with common fee, Hjalmarsson and Hjalmarsson (2009) find about 25 percent undercapitalization. We use the precise, dwelling level debt in our transactions data.
a home (i.e., common costs), a shock to how the comprising information stands out made buyers pay less for homes, by way of incorporating debt equally with price.

Thus, our main contribution is to show that salience matters in large transactions. While salience bias is known to be important in other markets, it is striking it has a significant presence in a large housing transaction. To the best of our knowledge, we are the first to show that increasing information visibility in housing, without changing costs of owning the large asset, will restore rationality.

Second, the link to rich administrative and demographic information sheds light on the mechanisms that are related to and can attenuate the bias. Our rich dataset allows us to identify groups of homebuyers that were more biased and those that could understand the problem at hand already before the regulation. We show that while bias elimination is not driven by financial (or other) sophistication (both before and after, all that was needed was to sum two numbers), they are highly correlated, as with experience one could understand the true problem – optimization based on the total price. Unlike earlier work on undercapitalization, this heterogeneity suggests that debt amount was possible to observe.

Third, thanks to the rich setting, we improve identification in several dimensions. The presence of two types of homes allows us to exclude the role of factors confounded with housing markets generally at the time of our event. Further, comparing co-op sales pattern with that in self-owned units, we are able reject the role of adverse selection. Unobservable quality is eliminated thanks to the building level identifying information.

The eliminated bias at the same time invalidates several other explanations: liquidity constraints are not a driver – if there were constraints, there still would be shortly after the event in the basic diff-in-diff. Instead, we confirm no differential value between co-op debt and price. We further provide several tests to study whether more liquidity constrained individuals overpaid for co-op debt to increase debt capacity. Heterogeneity analysis based on unit size and income confirm little role for liquidity.

Standard models with adverse selection imply a reduced transaction volume when buyers are less informed about an asset’s quality: they may become reluctant to carry out a transaction since they realize they may pay an unfairly high price compared to the asset’s fair market value. Consequently, trade will
increase as buyers receive information, and asymmetry is reduced (Akerlof, 1970; Levin, 2001). Therefore, asymmetric information about debt would imply a lower trading volume in co-op units before the implementation of the regulation, and an increase thereafter. Instead, we observe no increase (and, in fact, a slight decrease) in the relative number of co-op transactions around the event. This result is not driven by a change in the relative supply (existing stock or new supply) of the two types of units.

An important public policy is to improve credit standards and reduce defaults. Our findings suggest the mandatory disclosure of debt was an effective step in this direction. First, young, low-income, and first-time homebuyers are the most likely cohorts to fall victim to the costs of the behavioral bias, in our setting for mortgage market or more generally. Consequently, they may become more indebted than they intend to be. In case the resulting suboptimal decisions cause an increased rate of defaults, they may have a significant impact on the entire economy. Furthermore, paying more for hidden debt is eventually a “transfer” to more well-off groups, such as housing developers. When the less well-off cohorts unknowingly make such large transfers, the resulting increased inequality is a welfare loss to the extent that the beneficiaries’ marginal utility is lower.

Our work contributes to a growing literature in finance and marketing that has explored how individuals respond to the salience of the full price in various contexts, such as public finance (e.g., salience in taxation; Chetty et al., 2009; Chetty and Saez, 2009; Finkelstein, 2009), corporate finance decisions, retail financial products, medical insurance, and credit cards. Heitmann 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) 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. Agarwal et al. (2015) report that a

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9 Specifically, as Levin (2001) points out, trade will always increase after new information when demand is downward sloping.

10 Indeed, suboptimal decisions caused by behavioral biases are likely to be costlier for people in poverty in various contexts. See for instance *World Development Report, Chapter 6: Household Finance* (2015).

11 The sharp decline in housing markets and the associated rise in mortgage defaults in the United States during the recent crisis were partially due to suboptimal decisions, albeit caused by a different mechanism.
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). More recently, Ru and Schoar (2015) suggest 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. Similar to our example, in Stango and Zinman (2014), borrowers become more attentive and avoid fees on credit cards when they are exposed to information about the fees in a survey. In Chetty et al. (2009), including taxes in the price tag decreases consumers’ spending. In Blake et al. (2018) consumers change behavior depending whether the all-inclusive price is shown at the first viewing of an online ticket purchase, or whether the add-ons are presented only at checkout. In light of this example, homebuyers in our market may still make suboptimal decisions when they learn the total price only at the time of home viewing rather than on the online platform at first. In Liabson and Gabaix (2006) a consumer buying an ink printer may or may not incorporate the per page cost of ink, although in principle she knows ink is necessary and may know the price of it. But knowing about it may not be enough; ‘When add-ons are made salient—e.g., through advertising—our newly educated consumers make a more informed choice among the available goods’. Overall, people make choices that are dependent on contexts. We show this happens in the consumer’s largest transaction.

A handful of studies have shown how irrational behavior may affect choice in the housing market (e.g., Genesove and Mayer, 2001; Brunnermeier and Julliard, 2008). Keys et al. (2016) focus on the decision to refinance mortgages, and find borrowers fail to refinance due to irrational biases, leaving large amounts of money on the table. Agarwal, Rosen, and Yao (2016) 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 borrowers pay directly and those they pay indirectly through the yield-spread premium. The authors find 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). Keys et al (2016) show homeowners may
fail to refinance. Yet, calculating the real option of refinancing maybe more costly to someone who values their time highly, than the direct monetary benefits from refinancing. By contrast, first, we study the salience features of the full price itself. Then, a striking feature of our experiment is that the benefit is straightforward: while in other contexts, calculating the financial benefit may be more complex, benefits can accrue over a long period of time (e.g., mortgage refinancing, trade-off between interest rate and late payment fee), and it is difficult to argue agents are irrational, the trade-off here requires knowledge of elementary operations at best.

2. The Housing Market in Norway

Norway is traditionally characterized by high homeownership. The majority (76%) of Norwegian households own their homes, and the sharp price appreciation has boosted household wealth. Homes constitute roughly two thirds of gross household wealth and more than 100% of net household wealth (OECD, 2014). In the United States, for instance, households hold approximately 18 trillion USD in real estate assets (Agarwal et al., 2013), whereas the Norwegian population of five million owns real estate worth an astounding 0.9 trillion USD. 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% in 1992 to 220% in 2014. So far, high household leverage has not affected the credit default rate, which has been low and stable at approximately 1% since the late 1990s (Solheim and Vatne, 2013). One reason for the low default rate is the full-recourse loan policy in Norway.

2.1 Co-ops in Norway

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

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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.
14 Co-ops are seen in many parts of the world. In Sweden, co-ops were the only form of apartments until recently. New York alone has more than 600,000 co-op units, whereas they are present more moderately in other countries. See more in “Profiles of a Movement: Co-operative housing around the World,” available at http://www.housingeurope.eu.
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 buyers 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 shares are sold, the developer can decide how much of the total value of the shares the prospective buyers will pay upfront and how much of it will be financed by a loan taken by the co-op itself (co-op debt). Importantly, buyers will service their pro-rata share of the co-op debt. Ceteris paribus, higher co-op debt should therefore imply a lower price. Owners then service the debt and its interests, along with maintenance and utilities, as part of a total monthly co-op fee.

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, the Law on Real Estate Brokers (“Lov om eiendomsmegling”) was announced. It came into force on 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, the requirements for licensing and authorization to carry out real estate brokerage activities (such as agency issues involved, minimum required qualifications, etc…), the procedures of establishing a business contract with a real estate broker, as well as sanctions. The law provides a clear procedure for an eligible real estate broker to establish a transaction contract; 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

15 In very rare cases, households that 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. For instance, when buying a unit in a co-op that has high enough leverage, the homebuyer may afford to finance the whole transaction by cash without the need to take a mortgage. With reasonable levels of leverage, however, households are unlikely to be able to circumvent borrowing constraints. We do not find any bias towards highly leveraged co-ops in the data.

16 The relevant information-disclosure rules are spelled out in §6-7 of the law. All the details of the act are available at https://lovdata.no/dokument/NL/lov/2007-06-29-73.
broker always give the potential buyer a written assignment (prospectus), which contains at least the following information: total costs, all fees, and taxes. Most significantly, it requires that the broker provide clear and detailed information about the share of the joint debt, the total sum of the buyer’s share of the debt and the transaction price, both in the prospectus and in the sales advertisement.\textsuperscript{17} Additionally, in the presence of common debt, the seller must disclose the loan terms for repayment of the debt, monthly costs for the debt, separate calculation of monthly joint costs together with the installment of the loan as well as other fixed costs related to this debt. These requirements were not mandated before the law, and so sellers with high debt were inclined not to advertise 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 (or zero) debt.\textsuperscript{18}

Furthermore, \textit{The Housing Cooperative Act} regulates, among other things, the owners’ obligations regarding the joint debt.\textsuperscript{19} Importantly, no member is personally liable to creditors for the total liabilities (§1-2). If a member does not meet his or her obligations, the co-op may order the member to sell the shares with an advance notice of at least three months. In such cases, the Cooperative Guarantee Fund will reimburse the housing co-op for missed payments, so other members do not bear the costs.\textsuperscript{20} Therefore, co-op debt will not be more expensive for homebuyers to service, and all homebuyers will be liable for the mortgage as well as their (and only their) share of co-op debt. In addition, rare defaults make homebuyers in our setting unlikely to strategically opt for co-op debt to reduce the costs of

\textsuperscript{17} Thus, debt associated with self-owned apartments had to be revealed as well, but this debt is 0 or rather low. We analyze debt of self-owned units in our robustness analysis.

\textsuperscript{18} One may wonder if low-debt co-ops or lenders approving mortgages should be interested in directing buyers’ attention to find the debt and consider it appropriately. It is possible that this happened, yet for our empirical design it suffices that some buyers’ remained ignorant of the debt. In fact, we see first market appearance of ‘honest’ co-ops only very late. http://www.dn.no/privat/eiendom/2007/04/25/lokker-boligkjopere-med-totalpris? More evidence suggests that most buyers remained ignorant, and that developers were using higher debt to push prices: https://www.dn.no/privat/eiendom/bolig/2008/03/26/solgte-dyrt-ndash-na-sliter-kjoperne; they were disclosing the popular common, ownership fees but not the details of how much of it is to service debt: https://www.dn.no/privat/eiendom/2008/04/01/-moralsk-forkastelig.

\textsuperscript{19} https://www.regjeringen.no/en/dokumenter/the-housing-cooperatives-act/id439595/

\textsuperscript{20} The Cooperative Guarantee Fund (Stiftelsen Borettslagenes Sikringsfond) was founded in 1994 and offers co-ops insurance to secure payments from residents: http://www.nbbl.no/About-NBBL/Subsidiary-companies/Borettslagenes-sikringsfond.
servicing it. The co-op also holds a first lien in the unit that supersedes homeowners’ other obligations. Importantly, these factors are also unaffected and time invariant during our study.

Equal liability and competitive mortgage markets means that co-op debt and personal mortgage should have similar values for an optimal homebuyer. While mortgage rates in Norway increased during the period 2004 to 2008, both co-op and personal debt in Norway have predominantly floating rates. The share of fixed-rate loans in the total household sector, including both co-op debt and personal mortgage, was less than 10% in 2006. Although the co-op share of fixed rate loans might be somewhat higher than the share in the personal mortgage market, such a difference would have only made co-ops more attractive at a time of increasing mortgage rates, and would have reduced, rather than increased, the magnitude of our results. In fact, from 2007 to 2008, average interest rates on personal mortgages went up from 5.55% to 7.04%, while interest rates on co-op loans went up from 5.54% to 6.66%. The lower co-op rate would translate to a coefficient that is higher in absolute value, rather than lower. In our case, a 0.04 percentage point lower co-op rate must be reflected in a coefficient of -1.042 ceteris paribus, for 30-year monthly installment loans. We take the more conservative benchmark, assuming roughly equal costs for the two sources.

Co-ops pay taxes on profits, which are typically very low because 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 were a rental building and is independent of the market prices of the actual co-op units as well as of the capital structure of the co-op. Interest payments on both co-op and personal debt are tax deductible at the same rate, 28%. 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. Finally,

\[21\] The use of floating-rate loans is also pervasive in the corporate sector at 70%. See Norges Bank Economic Bulleting 2007(1).
\[22\] See Statistics Norway, https://www.ssb.no/en/bank-og-finansmarked/statistikker/renter for mortgage data. The source for coop rates is National Company Register (Bronnoysundregister) and Norwegian Tax Authority, to which we return later.
the owner faces no capital gains tax if he or she has occupied the unit for at least 12 months before selling it.\textsuperscript{23}

Co-op housing exists in many countries throughout the world. In the US, about 10\% of all multi-dwelling homes are co-ops, while New York City, this share is close to a staggering 80\%. Across Europe the numbers vary, too: in Sweden, this was the only form of multi-dwelling homes allowed until 2009 (Almenberg and Karapetyan, 2014), in Austria 18 of such homes were co-ops, while on average 10 percent of Europeans live in co-op housing.\textsuperscript{24}

3. Data

We use several data sources for our analysis. The first one is a multi-dwelling housing transactions data that comes from Eiendomsverdi AS. This is a private firm that collects data from real estate agencies, administrative records, and Finn.no (the major Norwegian online company for selling property). The sample (transactions data, hereafter) consists of high-quality data on all apartment transactions in Oslo, Norway, between January 1, 2003, and December 31, 2012. In total, we have just above 120,000 transactions. We then remove transactions of units that are sold before or during the year of completion, which together with few observations with missing information, make up about 1\% of the data.\textsuperscript{25} We then are left with 119,372 transactions, 67,212 out of which are co-op units. We focus on co-op units in the first part of the empirical analysis.

Table 1 summarizes our data. In Panel A, 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, high-debt units are only slightly larger (by 3 square meters) and are more similar in other characteristics. There are only 281 (38) apartments with 0 bedrooms (on floor 0) so we group them with category 1.

\textsuperscript{23} Otherwise, a capital gains tax of 25\% may apply in certain cases. Also, there are no differences in the rules that determine the base for capital gains tax on co-op shares and self-owned units. See http://www.skatteetaten.no/en/person/Tax-Return/Topic-and-deductions/Housing/Buying-and-selling/-Sale-of-real-property/?chapter=3833#kapitteltekst.

\textsuperscript{24} See “Profiles of a Movement: Co-operative housing around the World”, as well as other resources on https://www.housinginternational.coop documenting existence of co-ops elsewhere in the world.

\textsuperscript{25} Unsold units are sold by developers directly rather than by an intermediary, they are not sold in auctions (but at fixed prices). Our results are not affected when we include those transactions.
Debt is larger by almost 370,000 NOK, and the prices are lower by 130,000 NOK.

In Panel B, we provide a summary for one-year transactions before and after the event. The general pattern is also present here: highly leveraged units are similar in most aspects and quite close in size (about 2 sq. meter difference). The changes in the explanatory variables are minimal over time, yet the prices of high-debt co-ops drop significantly (by around 110,000 NOK). Finally, the proportion of the high-debt and low-debt units remained constant over districts around the reform, as Figure 2 shows.

The second source of housing data is Ambita AS, which is a registry owned by the Norwegian Government and its purpose is to store property data, and develop real estate and land register (registry data, hereafter). 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. The main advantage of the data is that it has an individual-level unique identifier, which allows us to match the information with administrative information from Norway. We thus merge it with detailed information from Statistics Norway about homebuyers’ personal finances and their demographic characteristics. For each Norwegian taxpayer we have assets: cash, property, financial investments (separately in stocks, bonds, mutual funds, derivatives both in Norway and abroad); income, the individual’s total net wealth, sex, age, and a household identifier, including an indicator about the head of the household. This dataset is available to us from 2004. The

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26 A further difference in the dataset is that has a different recorded date of the transaction. While in the first case it is the date when the auction is finalized, in the latter it is the date when the transaction is reported to the administrator for registration. The difference can span from several days to a few months, thus not allowing us to merge the information with our initial transaction dataset. To alleviate the potential concern we try to drop the transactions happening within two months from the date of the regulatory change, and observe somewhat stronger (quantitative) differences in short windows. Finally, along with apartments we also have information about single-family houses that are self-owned units, which we drop from the analysis.
second advantage of the registry data is that it has more detailed hedonic variables, allowing us to use building-level fixed effects. This will allow absorbing any remaining issued regarding unobserved heterogeneity that may exist across various buildings. We return to this later.

Summary statistics for the registry data are provided in Table 2. Unlike co-op units, newly built self-owned units have no debt. Debt in self-owned units is only raised at a later stage (e.g., to finance common renovations and expenses); the average debt increases gradually with the building’s age, starting at below 5,000 NOK per unit for buildings under 10 years old and climbing to between 60,000 and 65,000 for a unit above 50 years old. In the registry data, the availability is shorter (2006 and 2009) with again overall comparable characteristics between self-owned and co-op samples in the three years (Panel A) as well as over a one-year window (Panel B). The two types of units differ to some extent in age (self-owned units are somewhat newer), but are similar in terms of the number of rooms, size 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, because the more demanded western districts (e.g., Frogner, Ullern) have a higher proportion of self-owned units (Figure A1). These differences, along with differences in the existence of debt, account partially for the mean price difference in the table (0.9 million NOK).27, 28

-Insert Table 2 about here-

27 For comparison, in Appendix A we also provide the combined self-owned and co-op dataset from the transactions data (Table A1). Mean debt is much higher in an average co-op unit compared to a self-owned unit (by over 130,000 NOK). The table is followed by analysis using that data for robustness, to which we return in section 3.
28 Developers raise identical buildings in form of co-ops and self-owned units, so the financing aspect is unrelated to dwellings’ quality. There are a number of examples only in Oslo, see the Gartnerkvartalet project for instance, http://www.okernloren.no/2018/07/12/godt-nyboligsalg-pa-okern-og-loren-su-langt-i-sommer/gartnerkvartalet/, where the project and the similarities of buildings can be seen visually here https://www.dropbox.com/home/ambita/MS%20revision?preview=Gartnerkvartalet.png. In a further study, it would be interesting to examine the institutional setup and the developers’ incentives for having the two distinct structures in the market.
In the table, we report the matched Statistics Norway data including income, indicator variables for being young; being a first-time buyer; having financial investments. A key takeaway is that overall being young, a first-time homebuyer and having no investments is more likely with co-op owners, however those change rather proportionally over 2007 to 2008. Similarly, income and being wealthy is more likely in self-owned transactions, yet those change rather proportionally over the period.

4. Empirical Analysis

This section presents the methodology, hypotheses and the results. We first focus on the baseline cases, followed up by heterogeneity results. We address selection issues, discuss the role of other potential explanations, and present further robustness checks. Throughout, we address endogeneity issues with various approaches.

4.1 Methodology and Hypotheses

We build our hypotheses around the law, announced on July 29, 2007, and implemented on January 1, 2008. Norway provides an ideal setting for the analysis because co-op units have high variation in leverage, and they coexist with self-owned apartments. We exploit both variations for identification.

To the extent that some homebuyers were unaware or ignorant about the importance of debt before the law, the debt in such homes would be taken into consideration only at a discount by their buyers on average. Our hypothesis is that the mispricing for these homes will be corrected after the regulation; if so, lack of salience is the source of the bias. In contrast, the law should not affect homes without debt: the willingness to pay a certain amount for such homes will not change even for biased buyers. In our first specification, we test that the increased salience of debt will be reflected in an increased rate of capitalization of debt into prices. Specifically, we estimate the following equations:

\[ P_i = \alpha + \beta_1 Debt_i + \beta_2 \times Post + \beta_3 (Debt_i \times Post) + X_i \gamma + \epsilon_i, \]  
\[ (1) \]

\[ P_i = \alpha + \beta_1 Treat_i + \beta_2 \times Post + \beta_3 (Treat_i \times Post) + X_i \gamma + \epsilon_i, \]  
\[ (2) \]
Where $P_i$ is the transaction price (or the natural logarithm of it), $Debt_i$ is the amount of co-op debt associated with apartment $i$, and $X_i$ is a vector of controls, including the unit’s hedonic characteristics (size of the unit and floor, number of rooms, age of the building, and location). $Post$ stands for a dummy that takes the value of 1 for any transaction that was completed on or after the implementation of the regulation. Location is measured at the district level, and we include a dummy variable for each district. In equation (2), $Treat$ is an indicator that is equal to 1 for high-debt apartments, using the top quartile in the debt distribution as a benchmark. In the registry dataset, $Treat$ is 1 for co-op and 0 for self-owned units. This equation is thus estimated with a diff-in-diff methodology, where we have a control ($Treat=0$) as well as a treatment group.

Taking self-owned units as a control group is the only approach applicable to our registry data due to lack of information on debt. The analogue 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 price of the co-op unit, whereas for self-owned units, no such “new” debt exists.

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 also implies prices should follow a parallel trend in the pre-treatment period, which can be tested. Figure 1 plots mean annual transaction prices for housing units, for the lowest quartile versus the rest of the units: the lowest quartile can be seen as the control group. Until 2007, the prices closely followed a common trend. In 2007, prices start to diverge, with a negative impact on high-debt co-ops.

While the pre-event parallel trend in outcomes alleviates concerns for identification, it does not

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29 Other hedonic studies use a similar set of variables (e.g., 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. Although more popular is to use the logarithm of the price as the dependent variable in part due to its convenient elasticity interpretation, Cropper et al. (1988) show that the linear-form does equally well when compared with log-linear or log-log forms, both in the presence of perfect information as well as when some key hedonic characteristics are missing. In equation (1) we instead choose the price as the dependent variable and debt in the right hand side, due to the linear relationship between the two that is constant over the level of debt, whereas the price elasticity of debt varies with the level of debt. In specifications where we use an indicator variable for the treatment status, we take the more popular logarithmic form, which also serves as further robustness; we have checked that our results would be qualitatively and quantitatively comparable.
eliminate the worry that treated and control groups can differ in unobservables just after the treatment in a way that violates the assumption. For instance, debt could be endogenous due to an omitted variable, which, if also correlated with the event period, would jeopardize identification of the diff-in-diff estimate. To ameliorate this, in most specifications we first will saturate the model with year-month fixed effects to capture any unobservable time varying changes. Furthermore, in the registry data we account for time invariant building level characteristics by adding building level fixed effects in specifications with short and long windows. In short windows, the role of time-varying building level characteristics is also neutralized.

A remaining worry is that the value of debt may change differently across co-ops with various leverage over the same period. Indeed, the value of the underlying co-op debt directly affects the transaction price, and this value can potentially change a lot more for high debt co-ops compared to the control group. If true, our findings could not then be fully ascribed to the increased salience of debt. We check to verify this is not the case by analysing the underlying co-op debt and its interest rates in section 3.3.2.

Similarly, the district fixed effects might not control for all unobservable characteristics. For instance, to the extent that variation exists in apartment standards within a district and is correlated with debt, our estimated debt coefficient may still suffer from an omitted variable bias. However, note first our diff-in-diff estimate would not suffer from a bias caused by such variation, if any, unless there was a systematic and significant quality reduction in transacted high-debt units kicking in after the regulation. At the same time, that 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 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. Furthermore, we confirm our conclusions by separating old and new buildings. Finally, these concerns would be eliminated in the registry data where we use building-level fixed effects.

In the next section, we provide further tests to shed more light on the validity of the parallel trend.
4.2 Results

This subsection presents the results. First, we present the baseline cases for transactions data, followed by a brief comparison with registry data. We then provide analysis of various buyer cohorts, followed by heterogeneity analysis in other dimensions. We finish by discussing the potential role for other explanations.

4.2.1 Baseline results

In Table 3, we fit the data to equation 1, where we examine the extent of undercapitalization and the degree to which it changes after the event.

In the short run, columns 1, 2 and 3 show for 1-, 6-, and 12-month window, respectively, a statistically significant impact of the event on the rate of capitalization at about 12%. Column 4 shows the effect is larger in magnitude by about two times in the long run, 23%.

Assuming as a rational benchmark, that a one dollar increase in debt should be reflected in one dollar decrease in prices after the law, we should expect to see full capitalization after the treatment.\textsuperscript{30} Unless some homebuyers suffered from salience bias before the law, the relation between debt and prices would be close to -1 already then, without any impact on capitalization upon treatment. To confirm that time-varying observables and observables that are correlated with the Post dummy do not drive the results, in column 5 and 6, we divide the sample into before- and after-event periods, and saturate the model with year-month fixed effects. The results are only marginally affected: the coefficient on debt has a statistically significant difference of 0.232. To conclude, debt was capitalized into prices at 74-79% in

\textsuperscript{30} 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.
the total pre-treatment sample (columns 4 and 5), but overall undercapitalization vanishes after the event: the rate of debt capitalization (sum of the coefficients in the first and second rows) is no longer statistically significantly different from -1.

The economic magnitude is large. An undercapitalization of 25% means a homebuyer could reduce the costs of home acquisition by more than 70,000 NOK, or about 13,000 USD, by choosing a co-op unit with debt that is one standard deviation lower (i.e., 281,000 NOK in the whole co-op sample used in Table 3). The correction to full capitalization in the long run causes the price impact.

Our results are slightly more pronounced when we fit the data in Table 1, Panel B to equation (2), by defining as a control group dwellings with debt in the lowest quartile, while treatment group are those in the top quartile. The results are presented in Table 4, where we use only the two quartiles. We report results with the logarithm of the transaction price as our dependent variable (and thus the logarithm of the continuous ‘Area’ variable in the right hand side of the equation), although magnitude and significance are comparable in the linear model. The table thus shows elasticities and semi-elasticities: within 6 months (1 year) after the event, a treated unit’s price declines by 5% (7.4%), which correspond to 75,000 NOK (111,000 NOK) for an average priced treated unit, or 16% (over 25%) of its debt in the highest quartile. This is the extent of the additional debt capitalization after the event. Finally, it corresponds to 13% of price difference when comparing columns 5 and 6, and is more pronounced in column 4. This means the relative price of a treated unit (which has on average 445,000 NOK more debt) declined by around 190,000 NOK, i.e., a stronger capitalization effect.

To corroborate the validity of our diff-in-diff approach, we perform a formal test of the parallel trend. Panel A in Table 5 shows results for the generalized model that includes the interaction of debt with dummies for all years, except the year preceding the treatment period, namely, 2007. As the table shows,
no statistically significant coefficients exist before the implementation, whereas effects are significant in and after 2008.

We further conduct falsification tests by repeating our analysis around other dates. In Panel B of Table 5, we test equation 1 within one-year (six 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 no statistically significant effect on the rate of capitalization at any of the dates.\textsuperscript{31} The last column shows a regression for a window around December 31, 2008, one year after the event: while the full effect accrues over time after the event, a statistically significant, stark difference seems to appear only around the event itself.

4.2.2 The value of co-op debt

A remaining concern is how the value of debt changes for high versus low debt coops. Indeed, the value of the underlying co-op debt will directly affect the transaction price, and can potentially change a lot more for high debt co-ops, especially if refinancing possibilities are different across co-ops over the financial crisis happening at the same time. Such changes, if different for treated and control groups around our event, will jeopardize the validity of the diff-in-diff estimate. We are able to analyse this issue by acquiring information on all co-ops active in Oslo, Bergan and Stavanger, the two other major cities in 2007 and 2008 (1569 co-ops in total) from Brønnøysundregister, the national public register that compiles annual reporting by firms. We get information on their identity (organizational number), as well as their total assets.\textsuperscript{32} The financial statements here do not include information on interest rates.

\textsuperscript{31} In unreported regressions, we also try one-year-before and -after windows (covering 2 years’ transactions) to ensure insignificance does not result from a low number of observations. All our conclusions also hold for shorter windows.

\textsuperscript{32} We are however unable to merge the information with our datasets as we lack information on co-op identifier.
of the debt, so we merge the dataset with the Norwegian tax authority’s dataset on firm-bank relationships. In this dataset, we have all outstanding exposures in Norway at the firm-bank-account level. We extract this information as of the end of 2006, 2007 and 2008, as well as interest rate paid at the bank-firm-account level throughout 2007 and 2008. This dataset contains annual, matched firm-bank data for the universe of Norwegian firms.33

Using the interest paid, we can approximate interest rate on the co-op loan by dividing the interest paid during a given year on the average of previous and current end-of-year loan balances. Table 6 reports the summary statistics for co-op debt, interest rates and total asset size, where interest rates are calculated as an average across all loans (in those cases with more than one loan) for a given firm in a given year.

---Insert Table 6 about here---

We report all co-ops (below and above 50th percentile), but the comparison is qualitatively similar for top and bottom quartiles. As the table shows, interest rates change from 2007 to 2008 for both low-debt as well as high debt apartment, and to similar, almost identical degree. At the same, the table shows that size (as measured by total assets) is unrelated to the leverage of co-ops, too.

We further support our interest rate approximations by reading in details the notes in original language to financial statements for 100 co-ops: while the co-ops do not report the interest rate at which they finance their loan in their income statements, for the majority these are mentioned in their notes.34 In cases when the co-op has more than one loan (13 out of 100 co-ops), we weight the interest rate by the loan size, for years 2007 and 2008. We do so by randomly choosing 50 large and 50 small

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33 For detailed description of the full data, see Juelsrud and Wold, 2018. Interest paid on all types of loans are tax deductible in Norway, so the tax authority keeps the relevant information.

34 The source of this information is public, and one can access one-by-one by providing the organizational number. We thank Susanne Solfjeld and Marius Vedum Thomassen for careful translation of the notes and extraction of this information.
cooperatives. The average interest rate in this sample goes up from 5.54 to 6.66%, which is quite comparable to the rates we obtain by approximation in the administrative data.

4.2.3 The role of age and market experience

Undercapitalization can be caused due to ignorance or unawareness about debt by some, but not necessarily all, homebuyers. For instance, having had a home increases the odds that the homebuyer has already read the prospectus in detail, seen the balance sheet of the co-op, or wondered about increases in ownership fees. Similarly, consumers with lower education and financial literacy are more likely to suffer from less salient features in financial contracts in other loan markets, such as checking overdrafts or credit cards (Stango and Zinman, 2014; Ru and Schoar, 2015). Finally, older individuals are more likely to have been in a co-op board, or have had connections with a board member.

In light of the above, it seems a priori plausible that individuals without financial market experience, first-time homebuyers, as well as young homebuyers, paid even higher price for dwellings with shrouded debt before regulation. To test these, we use the combined housing registry and statistics Norway dataset and analyze the role of these characteristics.

This task is challenging because factors affecting demand by various groups of homebuyers for the two unit types may change around the event date, which coincides with the global financial crisis. Indeed, if demand from young versus old buyers does not move in parallel for reasons other than information salience, the results that we find may to some extent reflect those factors. An example of such a factor is changing income levels during the crisis that could hit the young more than the elderly. To shed more light on the issue, we first analyze the descriptive statistics on homebuyers’ characteristics before and after the event. Table 2, Panel B, shows that there is hardly any differential change in those: while the average income of co-op buyers was around 15% 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 the law, and it hardly changed.

35 Only in two cases did we not find information on interest rates either in one or both of the years.
36 Evidence shows that participation in financial markets is related to financial literacy (Rooij et al., 2011; Calvet et al., 2007).
after. Importantly, looking at the number of homebuyers in the two groups in 2007 and 2008, one observes only a small switch from co-op units to self-owned units, likely reflecting lower demand due to increased salience of debt; in fact as a result of the regulation, we may also have smaller supply by recent high-debt co-op buyers that resist to realize capital loss, as with salient debt buyers that were previously biased would now require a corresponding discount. Finally, note that income levels within each group increased by about 7 percent in the matched homebuyer dataset in our Table 2, which is comparable with the aggregate statistics of all population from Statistics Norway (GDP and GNI increased by almost 5% in real terms from 2007 to 2008). Together these are indicative that mortgage standards did not tighten. Throughout our sample of the registry data (2007-2009), GDP and GNI per capita increased from 498.9 to 503.4 and 497.5 to 506.3 (1000 NOK), respectively.37

Generally, Norway went through the global financial crisis rather unscathed. The Norwegian banking crisis of the early 90s had already developed a cautious attitude in the country. In general, Norwegian banks hardly invested in sub-prime based bonds and did not incur associated losses. There were no US or UK banks based in Norway, which reduced the direct sale of such bonds. Bank losses were very low and nearly unchanged, hovering around 0.05 to 0.1 percentage of total lending. Aggregate credit levels did not decline, unlike Denmark or the Eurozone (see Figure A4). Overall, this is suggestive evidence that the crisis did not influence our market in any significant way. Lindquist et al (2014) document, using data on the total population in Norway, that income levels were hardly affected during 2007-2008 (see Figure A3).

We still more formally address the issue by, first, directly controlling for income (log of income) and wealth of homebuyers. Because we are concerned that income or wealth levels (and thus demand) may change to different extents by the various demographic groups, we control for both income and its interaction with Post, for the separate subsamples of each cohort of interest. The results are reported in Table 7, where in all specifications we cluster standard errors at the postal-code level, and control for

year-month and postal code fixed effects.\textsuperscript{38} In column 1 and 2, we report regressions for young versus old homebuyers separately. Young homebuyers are those who are under 35 years old (the median age in the dataset). Taking into consideration that the dependent variable is the log of transaction price, we see that after the law they paid about 7.3\% (coefficient of -0.0761 on Post*coop) less for co-ops, compared to a statistically insignificant impact on old homebuyers. At the same, the young used to pay only 30\% less for co-ops, where as their older counterparts paid about 39\% less for co-ops (a statistically significant -0.496 on Coop in column 2). This shows young buyers overpaid about 9 percentage point for co-op units before the law, but the large part of the bias (about 80\%) was ‘recovered’ after the law.

-Insert Table 7 about here-

From the administrative data in Norway, we also know whether the homebuyer owned any real estate starting 2004. We define first-time homebuyers as those who did not own a home from 2004. These homebuyers are more likely to have analyzed the prospectus more closely during the buying process or otherwise seen the co-op balance sheet. The third cohort we examine are the homebuyers who do not own financial investments. We define a dummy variable that takes value 1 if the homebuyer does not have any investment in stocks, bonds or derivatives. Column 3 and 4 (5 and 6) show results for first-time and repeat homebuyers (without and with investments), and results are comparable with a slightly smaller quantitative effect for buyers with no investments.

We then repeat the analysis for the whole sample in Table 8, where we cluster standard errors at post-code level in the last three columns. In column 1 and 4, we test whether young homebuyers are more likely to overpay for a leveraged co-op unit before the law. To do this we add a triple-interaction term, Young*coop*post. The results show, that while young homebuyers paid about 9\% (Young*coop coefficient at -0.97 in column 1) more for co-ops before the law, they pay approximately 5\% less after

\textsuperscript{38} In the registry set we have 280 postal-codes and we always cluster the errors with this data unless otherwise mentioned.
debt becomes salient. These are comparable to results in Table 7, showing somewhat smaller ‘recovery’ effect. At the same time, older individuals are nearly unaffected (Post*coop is mostly statistically insignificant). In columns 2 and 5, we confirm the results for first-time homebuyers, controlling for income both before and after the regulation, while in columns 3 and 6 we see the results for buyers without financial experience.

Insert Table 8 about here

To balance the groups of homebuyers in the pre-event and post-event subsamples more rigorously, we next apply a propensity score matching procedure. We resort to nearest neighbor matching of propensity scores, as first proposed by Rosenbaum and Rubin (1983). We use the predicted probabilities of the probit model to match each homebuyer before the event with its nearest neighbor that buys after the event. We take all the regressors including postal effects to have a more precise match. We start the matching by running a probit regression for the full sample, and estimate the propensity score for buying a home after (as opposed to before) regulation: Post is the dependent variable in the score estimation. The matching is done with replacement, so that each after-event buyer can be used as a neighbor for multiple pre-event buyers: this should improve the accuracy of the matching (Smith and Todd, 2005). The procedure confirms a sizeable common support: 18367 matched after-event homebuyers out of 20122 in total, which is not surprising given the rather stable characteristics of the observations over the event. The biases decrease for all variables with the exception of area, rooms and floor. Except for income, the biases across variables are close to or below 5 percent in the matched pre and post-event samples, as shown in Table B1.39

39 In the cases where biases in fact increase, and with large percentages, the economic effect of the bias appears insignificant, such as for instance with the case of the floor variable the mean in the pre- and post-event groups is 2.91 and 2.72.
In Table 9, the dependent variable is the log of transaction price. The table reports results for one nearest neighbor match.\textsuperscript{40} Columns 1 to 3 use postal code fixed effects, while columns 4 to 6 use matched-homebuyer fixed effects: this controls for any unobserved characteristics that are common to the matched pair. Our results are overall hardly affected, not only qualitatively but also quantitatively, across all six specifications.

-Insert Table 9 about here-

Finally, in Table 10 our sample uses matched buyers, after two-way matching of the observations. We do the same matching procedure separately in the subsamples for co-ops and then for self-owned units. These procedures should further balance the sample of buyers acquiring a co-op (or a self-owned unit) after and before the event. For each case, in the propensity score estimation we use \textit{Post} as the dependent variable in the probit regression. In columns 1, 2 and 3, we show the results conducting matching separately for co-ops, then for self-owned units. Overall results are comparable. In columns 4, 5 and 6 matching is done separately for the pre-regulation period, then for post-regulation period, where the treatment variable in each case is \textit{Coop}. Once again, our results remain robust both qualitatively and quantitatively.

-Insert Table 10 about here-

4.2.4 Unobserved housing quality

Can building level quality that is correlated with our treatment explain our results? For instance, if self-owned units happen to have a higher quality compared to co-op units in the market just after 2007 (or a higher value of any other unobservable characteristic), then our diff-in-diff estimate may partially

\textsuperscript{40}Results are similar when we use nearest 3 or 5 neighbors.
reflect the relative decrease in the valuable attribute of co-op units, not related to salience of debt. Using building level identifying information, we are able to eliminate the role of omitted variables at the house level. In table 11, we report specifications used in Table 3 and 4, applied at the registry data, where we saturate the regressions with building level fixed effects. These will absorb building level time invariant unobservable characteristics. From 6 months (column 2) to the total sample (columns 4, 5 and 6) coop prices decline by 7-8%: an average priced co-op decline over 100,000 NOK in value. 41

4.2.5 Heterogeneity

Under our hypothesis, debt is not incorporated into prices, because of the lack of salience. If this is true, units with larger debt should undergo a larger price impact. To directly test for this, we turn to the transaction data and exploit variation in debt. In columns 1 to 3 in Table 12, we confirm this is the case. Column 1 (2) compares units in the lowest (highest) quartile of debt in the sample of both co-ops and self-owned units, with units that have no debt. The coefficient on Post*debt shows a decline in prices by over 18,000 NOK (over 295,000), which is economically sizeable: in fact mean debt in the lowest (highest) quartile is 28565 (491523), while standard deviation is 14256 (522291). In columns 3 to 5, the first three rows, respectively, show the interaction of Post with dummies standing for units with debt in the second, third, and the forth quartile. Column 3 provides the results for the subsample of co-op units only, column 4 incorporates all self-owned units, and column 5 saturates the model with year-month fixed effects. In all columns, our hypothesis is confirmed. An average unit in the highest quartile decreases in price by 210,000 - 240,000 NOK (around 40,000 USD at the time) relative to a unit in the lowest quartile.

4.2.6 Rational Constraints?

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. Thus, homebuyers can circumvent the need

41 While Almenberg and Karapetyan (2014) provide robustness with co-op fixed effects as well, absence of diff-in-diff setting does not allow them to exploit variation in initial debt, but only in those co-ops where debt varies, i.e. older co-ops taking additional debt. We exploit the salience shock as variation within new buildings as well where debt is only used to finance the value of a new home.
for a mortgage. In these rare cases of cash-financed transactions, financially unviable homeowners may end up owning co-op units, since it is hard for co-ops to reject buyers easily based on concerns about their long-term financial viability. Whereas our diff-in-diffs approach and the stable leverage structure around the event suggest cash transactions are 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 in Table 12, column 6. If consumers were indeed buying into highly leveraged co-ops by cash due to mortgage constraints, the impact of the law should be at best muted for this subsample, because buyers presumably remain constrained. The results show that large debt co-op units are 280,886 NOK cheaper than other co-op units, while the statistically significant large coefficient for the triple interaction (over 167,000 NOK) means the law affected large-debt co-op units by 60% more. In addition, column 4 confirms the conclusions based on the quartiles of transactions.

Similarly, if individuals were constrained by the mortgage amount to which they had access, 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 columns 2 and 3) apartments. As shown 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 (one bedroom or smaller), our results show the effect is even larger for those units.42

A potential alternative explanation to our findings is the possibility of asymmetric information. According to asymmetric information theories, when buyers are less informed about an asset’s quality, transaction volume may be at a low level or the market may even reach a complete breakdown (Akerlof, 1970; Levin, 2001). This is because buyers are reluctant to carry out a transaction since they realize they may pay an unfairly high price compared to the asset’s fair market value. Therefore, trade will increase as buyers receive information, and asymmetry is reduced.43 If this were true, reduced

42 A more pronounced effect for small units could be driven by other buyer characteristics, such as little financial experience. We return to this possibility later.
43 Specifically, as Levin (2001) points out, trade will always increase after new information when demand is downward sloping.
asymmetric information about debt would imply a higher trading volume in co-op units after mandatory
debt disclosure. Instead, we observe no increase (and, in fact, a slight decrease) in the relative number
of co-op transactions around the event window. Importantly, this result is not driven by a decrease in
the relative supply of new co-op units. In particular, the relative number of transactions of newly built
homes remained constant across the two types of units (as illustrated in Figure A2 in the appendix, right
panel), and so did the relative supply of already built units (more than one-year old).

4.2.7 Debt in self-owned units

Debt in self-owned units is typically much smaller than debt in co-op units. In fact for new self-owned
units, debt is zero, and is raised over the years for older buildings 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 less than -1 in
absolute terms, and be closer to zero. This argument is true for those homebuyers that have full
information about the amount of debt raised for financing and one raised for existing or future
renovations even before the law. Indeed, the borrowed money is invested in renovations of common
areas, such as the façade, playgrounds, or often attaching of balconies to the units in the house.
Eventually, those investments make the units better, and increase their market value. Thus, to the extent
that there exist buyers with full information and they value such renovation, the associated debt should
be at least less reflected in the price reduction. To take this into account, and isolate the initial debt of
coop units (i.e., debt raised for the development before initial sale), 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
coop housing in each of these age groups would require similar debt for renovation, we deduct from
each co-op unit’s total debt the average renovation debt of the respective age group. We thus expect to
get a close estimate of the initial debt for older units.

In Table 14, we use the renovation-corrected measure of initial debt for co-ops. Column 1 shows that
our main results are as expected; initial debt is capitalized at approximately 73%, but rises to just above

30
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 19% 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 rate between 0.43 and 0.51%, suggesting that homebuyers (unbiased, aware ones) attach value to debt for renovation: the borrowed resources are only spent to improve the unit. Co-op debt capitalization is at around 78% before, but rises to full capitalization after the event.

### 4.3 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 15 we interact the $Post$ dummy with the observable characteristics (age, size, floor), as well as add year-month 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.

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 those units that were built before treatment. Column 5 shows that results are similar for this subsample.

In columns 6 to 10, we repeat specifications in the same order, restricting the sample to units that are less than 11 years old. In light of the discussion in the previous section, we should see a more
pronounced effect in those specifications, since in new apartments debt is unlikely to have been used for major renovations. This is confirmed in the results in columns 7 to 9, while in 6 and 10, capitalization of debt is comparable to the case with all buildings.

5. Discussion and concluding remarks

We provide evidence that increased salience of total acquisition price in housing transactions has a significant effect on prices. Inattentive homebuyers pay a premium for shrouded debt that is part of the housing price, but they no longer do so after a mandatory disclosure of the debt. Homebuyers start to rationally incorporate co-op debt, often a large part of a housing transaction, after its increased salience. Young, first-time, no-investment homebuyers paid the highest premium, arguably to the benefit of richer cohorts. Therefore, the regulation was more beneficial to the former groups, that were more inattentive to shrouded co-op debt.

Our paper has important policy implications. Increased salience of the main attribute such as the full price itself can reduce households’ total indebtedness. Unintentional, inattentive borrowing may lead to larger indebtedness than planned, and inability to repay. Therefore, in such large consumer transactions there are significant welfare gains of salience stemming from, first, a reduction in potential default rates. Individual lenders will not optimally internalize the negative externalities. So transparent presentation of the total price is especially important when other regulatory measures, such as LTI and LTV limits, are absent. Further welfare benefits would accrue from curbing unintended transfers that homebuyers could make to housing developers, as the latter are likely wealthier with lower marginal utility.

The findings on the importance of financial experience suggest the effectiveness of other policy measures, such as investment in financial education, must be evaluated more broadly. Even in settings, such as ours, where being rational does not require knowledge beyond elementary operations (i.e., calculating the sum of the transaction price and co-op debt), financial education seems to ameliorate the consequences coming from other, unaddressed issues such as failure to scan for the important information.
References


Appendix A

-Insert Figure A1 here

-Insert Figure A2 here

-Insert Figure A3 here

-Insert Figure A4 here

-Insert Table A1 here

**Self-owned units: Alternative control**

This section extends the analysis to the whole sample in transactions data, including self-owned units. Self-owned units are an obvious candidate for a control group, because they have comparable characteristics and yet little debt. In Table A2, columns 1 to 3, we first repeat our test of equation 1. Because 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.

-Insert Table A2 here-

In column 1, we see our estimate has increased to about 24%. 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 columns 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 the value of 1 for a transaction after the announcement...
and before the implementation, and 0 otherwise. The announcement effect is statistically significant and is equal to 6%. When compared to the pre-announcement period, post-implementation has, as expected, a slightly higher effect (-0.28). This effect could be interpreted by either debt having become partially silent or more sophisticated homeowners selling their units at a discount before the implementation and a larger price adjustment would take place. Column 3 uses Post-announcement, which takes the value of 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 fit data to a variation of equation (2): in columns 4 and 5 the model excludes and including debt as a control, respectively. As expected, the regulatory impact on co-op unit prices is reduced somewhat in the model with debt, although it still remains quite high, with a magnitude larger than before: co-op unit prices decrease by around 120,000 to 160,000 NOK (21,000 to 28,000 USD) on average compared to self-owned units. Finally, in the last column, the results are confirmed using the announcement of the law, rather than its implementation, as the treatment period.

Appendix B

-Insert Table B1 here-