Do mandatory disclosures squeeze the lemons?

The case of housing markets in India*

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November 6, 2023

Abstract

What is the impact of mandatory disclosures of quality on market outcomes in a large developing country? Does impact differ across sub-markets and income groups? We answer these questions in the context of housing markets in India where information asymmetry between homebuyers and developers is high and litigation against housing projects is common. We find that a 2017 reform mandating developers to make litigation details public led to a decline in prices of litigated housing units (lemons). The decline in prices was limited to the non-luxury sub-market and there was no impact in the luxury sub-market. We discuss possible mechanisms for the divergent results across sub-markets. We find evidence for the role of household resources in acquiring private information. Dissemination of information through media coverage of litigated projects does not explain the differential results. We rule out alternative explanations, such as severity of litigation in terms of number of disputes and type of courts, that could affect sub-markets differently. We provide support for disclosure laws in developing countries to reduce market inefficiencies and unequal access to information.

Keywords: Housing, property rights, information asymmetry, mandatory disclosure laws, India

1 Introduction

Markets in developing countries are inundated with poor quality products and services (Akerlof, 1970). Significant information asymmetry − with sellers having better information about the quality than buyers − could adversely affect the functioning of these markets. Information asymmetry can be resolved through private or public action. In developing countries, the wealthy can deploy resources in order to access information. On the other hand, regulatory mechanisms to address the problem of information asymmetry are likely to fail given poor state capacity and weak rule of law (Rajagopalan and Tabarrok, 2021). Further, the wealthy can subvert the regulatory processes in developing countries (see Behrer et al., 2021; Glaeser et al., 2003). Hence, carefully designed disclosure laws are more likely to be efficiency and equity-enhancing.

Evidence for the beneficial effects of disclosure laws has been mixed. However, much of the literature focuses on a developed country setting. This paper examines the nature of information asymmetry and effects of disclosure in housing markets in India. The questions this paper answers are: what is the impact of mandatory disclosure of housing quality on prices? Do mandatory disclosures have differential impact across housing sub-markets and income groups?

Rapid urbanisation in developing countries is driving an increase in housing demand and a construction boom in cities. However, poor land titles and weak contract enforcement in developing countries have resulted in insecure property rights and increased the risk of projects getting delayed or remaining unfinished (see Djankov et al., 2022, 2003). The presence of unfinished projects is a common phenomenon in China (Pettis, 2022), India (Crabtree, 2014), Nigeria, and Uganda (Economist, 2021).

Insecure property rights have led to disputes over the right to build in Indian cities (see Gandhi et al., 2021; Gechter and Tsivanidis, 2023). Litigation in the housing market is common (Times of India, 2018); 30% of the new housing projects in Mumbai are embroiled in legal disputes. Disputes over land titles and civil society action comprise a large share of these litigation.[1] Litigation signals uncertainty in the quality of projects, impedes timely completion due to extremely high rates of judicial pendency[2] and could lead to projects being unfinished[3] or demolished[4]. Thus, litigation against a project is a good indicator that the project is a lemon.[5]

Information about legal disputes and underlying issues with housing projects is costly to acquire for prospective buyers. In 2017, the state government of Maharashtra made it mandatory

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[1] Civil society actors go to the courts via the public interest litigation (PIL) route. PIL was introduced in the 1970s to empower civil society to protect the interests of citizens. The scope of PIL in the real estate sector is wide and could include heritage or environmental conservation, challenging the legal validity of permissions for increasing building heights or building approvals granted.

[2] Data by the National Judicial Data Grid of India shows that there were 42 million pending cases in the Indian courts in October 2022 and nearly 40% of these had been pending for more than two years.

[3] A luxury residential apartment tower in Mumbai, that began construction in 2007, has been left unfinished due to legal disputes (Crabtree, 2014).

[4] Two high-rise residential towers in the national capital region of Delhi, which had been under construction since mid 2000's, were demolished in August 2022, following a court order, for not adhering to planning rules. The court ruling was with respect to a PIL against the development filed by a resident welfare association (Indian Express, 2022).

[5] While litigation could result in demolition or unfinished projects only in extreme cases, even non-severe litigation affects ongoing projects since it increases completion times by on average 30% as cases are stuck in courts for long periods (Gandhi et al., 2021).

for housing developers to register their projects with a newly created regulatory authority. Developers had to disclose information about their projects, including the litigation status, to the public on the regulator's website.[6]

To study the reform's impact, we use property prices of housing units bought between 2015-2020 from a mortgage database of India's largest private sector bank. This data also provides us with details such as the area of the housing unit, the homebuyer's income, gender, among others. The housing units in this database belong to multi-family housing projects in Mumbai. We match these units to housing projects using the database of projects registered on the regulator's website. From this database, we get details about a project's litigation status, and details of amenities provided (gymnasium, swimming pool etc). A project may have one or more legal disputes that began at any point between 2015-2020 or before. Thus, projects in our dataset had litigation before and after the reform but information about a project's litigation status was not publicly available before the reform. Our final dataset comprises 11,553 housing units in 972 projects transacted between 2015-2020.

Our identification uses transactions before and after the mandatory disclosure reform − when a project's litigation status became publicly known − to test the impact of the policy on prices of units in litigated projects.[7] Using project and year-quarter fixed effects, we find that the reform led to a 4-6% decline in the average price per square foot of litigated units relative to non-litigated units. Our pre-trends tests show that the parallel trends assumptions holds. We also check for sensitivity to violation of parallel trends using tests by Rambachan and Roth (2023) and find our results to be robust.

We also estimate the impact of the policy separately for luxury and non-luxury sub-markets and for different income groups. We find that the reform had no effect on litigated units in luxury housing projects whereas litigated units in non-luxury projects saw the average price decline by more than 6% relative to non-litigated units. Moreover, litigated units in luxury projects had lower prices on average relative to non-litigated units in the pre-reform period. We also find that homebuyers belonging to the lowest income quartile saw the highest decline in prices of litigated units after the reform. On the other hand, there was no effect for homebuyers in the highest income quartile.

Our results may be attributed to actions of both buyers and sellers. We explore a possible channel for prices falling after the reform viz. developers responding to a decline in sales. The reform could result in developers of litigated projects lowering prices if they believe or find that the reform discouraged prospective buyers and thus reduced sales. We study whether the disclosure reform affected quantity of units sold in litigated projects, using data on project-level quarterly sales provided by PropEquity – a subscription-based real estate information portal. We find that the sales of litigated units fell post reform but only for the non-luxury market.

We propose and test two mechanisms that could be driving the heterogeneous effects of the reform on prices across the luxury and non-luxury sub-markets. The first mechanism relates to the differential ability of households within a sub-market to expend resources to gather private

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[6]To be precise, developers had to submit a legal land title report prepared by a lawyer and provide details of encumbrances on the land as well as any legal proceedings against the project. They also had to report the year in which the dispute began, and the court it was being heard in.

[7]We use "units in litigated projects" and "litigated units" interchangeably throughout this paper.

information and we test whether this ability is determined by the households' income group. The second mechanism pertains to greater media attention on luxury projects facing disputes playing a role in disseminating this information to the public. While we find some evidence for households' income group impacting their ability to access private information, we fail to find evidence for media coverage as a channel for information.

We conduct several robustness checks. To address potential issues in reporting of transaction prices in the mortgage dataset, we use an alternate data source for transaction prices provided by PropEquity. This dataset consists of all real estate transactions registered with the government and includes transactions financed by both private and public sector banks. The signs of coefficients in the overall and sub-market results using this data are comparable to the results using the mortgage data. However, the magnitudes of the coefficients in absolute terms are higher than our baseline estimates. Using data from the same portal, Anagol et al. (2022) show that transactions values for units having mortgages from private sector banks see much less under-reporting compared to transaction values of units with mortgages from public banks.[8] Hence, we believe that the estimates derived using the mortgage dataset from the private sector bank are more accurate than estimates from the data using all public and private sector banks transactions.

Reputation spillovers for developers with litigated projects could potentially violate the Stable Unit Treatment Value Assumption (SUTVA).[9] To correct for this, we drop units in non-litigated projects by developers who had other projects that were facing litigation from the sample.

We match litigated and non-litigated units on distance and also by propensity score matching based on project size to address potential bias due to the presence of unobserved covariates (such as neighborhood-specific factors) that impact selection into litigation and prices. Our results continue to hold for these different specifications.

One alternate explanation for our sub-market results could be that severity of litigation differs across sub-markets. In this case, the heterogeneous results for luxury and non-luxury sub-markets may be due to this difference in litigation rather than due to unequal access to information. To address this concern, we compare the impact of the reform on prices in the luxury and non-luxury sub-markets separately for units in projects with severe litigation and less severe litigation. We identify projects affected by severe litigation in two ways – level of courts (upper and lower) and number of cases. First, projects with at least one dispute in the upper courts are classified as having severe litigation. We find that the reform had an impact on the prices of units in projects with severe litigation in the full sample and the non-luxury sub-market but had no impact on the prices of units in projects with severe litigation in the luxury sub-market. Second, projects with two or more cases were identified as affected by severe litigation. While the reform had an impact on prices of projects with more than one dispute for the full sample, this result was not statistically significant for projects in the non-luxury sub-market.

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[8]Unfortunately, we cannot distinguish between transactions having mortgages from public sector banks and private sector banks in our PropEquity data.

[9]Anecdotal evidence of issues of reputation in the real estate sector in India can be found in recent newspaper articles (see Economic Times, 2015; Moneycontrol, 2021).

Our paper contributes to a number of literature sets. It adds to the body of empirical work on the impact of mandatory disclosures. The evidence on the impact of mandatory disclosure of quality in various industries has been mixed. A few studies (Fung et al., 2007; Greenstone et al., 2023; Jin and Leslie, 2003; Johnson, 2020; Myers et al., 2022) have found a positive impact of disclosure laws while others found limited or no evidence (Ben-Shahar and Schneider, 2014; Ho et al., 2019; Werner et al., 2012).[10] Our findings lend support to mandatory disclosures improving market outcomes in housing markets in developing countries.

Our paper also relates to the literature that studies the distributional impact of disclosure policies. Luco (2019) studies the impact of a price disclosure law on gas price margins and price dispersion in Chile. He finds that price margins in low-income (high-income) areas that had lesser (more) search activity increased the most (least), thus leading to greater inequality. Our paper also finds heterogeneous impact across income groups but in a progressive direction. Our findings are consistent with the progressive distributional impacts of food labels in Chile as modelled in Barahona et al. (2023).

Specific to the context of disclosure laws in the housing market, we build on a few studies examining the impact of such laws on prices (see Chau and Choy, 2011; Hino and Burke, 2021; Nanda, 2008; Nanda and Ross, 2012; Troy and Romm, 2004). Literature has not looked at this question in the context of India, which has weak property rights and arguably a greater extent of information asymmetry (see Akerlof, 1970). We also add to the literature on frictions in the housing market where search costs and information frictions adversely impact the mobility of lower-income households (Bergman et al., 2020, 2023). Finally, by showing how frictions in information flow could create distortions in urban housing markets in India, we add to the growing literature that focuses on the impact of regulations and institutional frictions on housing market outcomes in developing countries (see Brueckner et al., 2017; Harari, 2020; Henderson et al., 2021).[11]

Section 2 provides details about the mandatory disclosure policy with respect to housing in India. Section 3 lays down the theoretical underpinnings of our paper. Section 4 describes the data sources used in this paper and section 5 discusses the empirical strategy. Section 6 presents results. Section 7 discusses various mechanisms that explain our results and section 8 shows the robustness checks. Section 9 concludes.

2 Context

2.1 Indian Housing Market

Housing markets in Indian cities have been slow to respond to growing demand due to binding urban land regulations (see Annez et al., 2010; Brueckner and Sridhar, 2012; Harari, 2020). Complex and lengthy regulatory processes result in long project completion times and delays (Gandhi et al., 2021). Given the binding land use regulations, developers flout rules or get discretionary permissions from the local body for building above the restrictions (see Rajack

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[10]See Goldstein and Yang (2017) and Dranove and Jin (2010) for a review of this literature.

[11]We also add to the literature on how information frictions affect different markets in developing countries like trade outcomes in Philippines (Allen, 2014), the fertilizer market in Tanzania (Michelson et al., 2021), agricultural markets in India (Goyal, 2010), among others.

et al., 2013; Sukhtankar and Vaishnav, 2015; Tandel et al., 2023). This potentially opens them up to litigation. Weak property rights and contract enforcement also increase risks of legal disputes (see Gandhi et al., 2022). Since sales of units begin while the project is still under construction (these are called pre-sales), litigation that could potentially stall or delay construction increases the risk for buyers. For completed projects where buyers can move in, litigation could affect the value of their homes.

The nature of litigation is wide in scope. Besides legal disputes around land titles, public interest litigation in the real estate sector involving civil society groups has been on the rise. Noteworthy examples of public interest litigation include resident welfare associations filing cases against new developments for conserving heritage precincts[12] and activists challenging the legality of additional building permissions granted to developers.[13]

Figure 1: **Disclosure on RERA website**

<div style="text-align:center">Panel A   Panel B</div>

Note: The figure shows two projects on the Maharashtra RERA website. Panel A is an example of a project with litigation and Panel B an example of a project without litigation.

2.2 Policy Reform

In recent years, states in India have enacted laws to regulate real estate developers and protect the interests of homebuyers. The Real Estate (Regulation And Development) Act, 2016 was a federal law requiring all states to frame rules for regulating real estate development and setting up a real estate regulatory authority. The state of Maharashtra was among the first states to set up a Real Estate Regulatory Authority (RERA). The state rules required developers to register their ongoing projects with the authority.[14] Each project would then get a unique registration number which had to be displayed on any advertisements and promotional materials of the project (Economic Times, 2017). In addition, at the time of registration, developers were required by law to provide project-specific details through an online portal set up by the authority. The list of registered projects and their details were made available on a public website. The details include the location, size and type of project, regulatory permissions

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[12]See for example, Normandie Cooperative Housing Society Limited & Ors vs State of Maharashtra (PIL number 48 of 2016 in the Bombay High Court).

[13]See for example Mr. Nitesh Mohanlal Doshi vs The State of Maharashtra & Ors (PIL number 6 of 2016 in the Bombay High Court).

[14]The law mandates that all projects having more than eight apartments or with a land area exceeding five hundred square meters should register with the real estate authority. In major cities like Mumbai, this entails near universal coverage of all upcoming real estate development.

received, estimated completion times, details of any legal disputes, and developer details and past experience (see figure 1).[15] The rules under the Act came into effect on 1st May 2017. The website and registration portal went live on the same day. By July 2017, more than 13,000 projects across the state of Maharashtra were registered on the website. The figure rose to 14,400 projects by December 2017. Thus, the Act led to the creation of a public website where potential homebuyers could easily find crucial information like the estimated completion time and litigation status of projects.

Of the 14,400 new projects, around 20% were in Mumbai. Mumbai had the largest share of projects under litigation at 30%. The projects under litigation account for more than 40% of the total built-up area that is being constructed in Mumbai.

3 Theoretical underpinnings

Housing markets in developing countries are informationally imperfect (see Chau and Choy, 2011; Nanda and Ross, 2012). Information sets of buyers and sellers differ significantly, with the sellers having better information about the quality of housing units. As a result, buyers face uncertainty regarding the quality of an asset. Further, in the case of new housing projects, pre-sales are common around the world. Information asymmetry can play out more prominently in pre-sales as there are uncertainties around the timing of delivery and quality of units. Moreover, in pre-sales, developers have the ability to control information in order to increase sales completions. This raises the typical case for a market for lemons.

Information asymmetry in the housing market often leads to a pooling equilibrium where both lemon and non-lemon housing units are transacted and cannot be distinguished by buyers. Here, lemons are sold at prices that are greater than their underlying value and developers selling lemons capture the difference between the equilibrium price and true value of lemons. A mandatory disclosure can dissipate information asymmetry and transform a pooling equilibrium into a separating one, where either only non-lemons are transacted or where both types are transacted with a clear distinction in terms of quality and price.[16]

Suppose, i denotes a non-lemon housing unit, j denotes lemons, t is the timing of enacting a mandatory disclosure law with $t+k$ being the post-disclosure period and $t-k$ being the pre-disclosure period. In the pre-disclosure period in $t-k$, price of a non-lemon housing unit is P_i^{t-k} and for a lemon, it is P_j^{t-k}. Similarly, in the post-disclosure period in $t+k$, price of a non-lemon housing unit is P_i^{t+k} and for a lemon, it is P_j^{t+k}. Under a pooling equilibrium in the pre-disclosure period, as both types are not readily distinguishable, P_j^{t-k} - $P_i^{t-k} = 0$. We expect that in the post-disclosure period, the price of non-lemons will reflect a greater level of confidence in quality and willingness to pay compared to that in the pre-disclosure period i.e. $P_i^{t+k} > P_i^{t-k}$. Similarly, exposure as lemons will lead to lower willingness to pay i.e. $P_j^{t+k} <$

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[15]Heavy penalties charged to developers found to be providing incorrect information about their projects safeguards against misreporting.

[16]In a typical market for lemons, there is adverse selection which drives out higher-quality products unless there are public or private counteracting institutions such as branding or regulation (see Akerlof, 1970). Because housing is a highly heterogeneous commodity whose value is determined by a bundle of attributes, we may see both lemons and non-lemons being sold with litigation as an additional attribute in the price function in the post-reform period.

P_j^{t-k}, which would entail P_j^{t+k} - $P_i^{t+k} < 0$. Thus, the difference between prices of lemons and non-lemons in the pre- and post- disclosure periods will be negative i.e. $[P_j^{t+k} - P_i^{t+k}] - [P_j^{t-k} - P_i^{t-k}] < 0$.

However, there could be some heterogeneity in prices of lemons and non-lemons in the pre- and post-disclosure periods. In the absence of mandatory or public disclosures, buyers need to incur costs for acquiring private information. The degree of information asymmetry is likely to have a strong positive association with transaction costs. These transaction costs are monetary (costs for hiring intermediaries such as brokers, solicitors, surveyors) and non-monetary (time spent in gathering information or acquiring knowledge of the market) in nature. Low-income households may lack the resources to incur these transaction costs. Hence, in a market with acute information asymmetry, high-income buyers may be able to alleviate information asymmetry more effectively than low-income buyers. Hence, for a high-income buyer H, there could be a difference in prices between lemons and non-lemons in the pre-disclosure period i.e. P_{jH}^{t-k} - $P_{iH}^{t-k} < 0$ but not for a low-income buyer L i.e. P_{jL}^{t-k} - $P_{iL}^{t-k} = 0$. As a result, when information about housing quality is not publicly available we may observe a high level of heterogeneity in terms of prices of lemons. However, the literature on this is somewhat ambivalent (see Turnbull and Sirmans, 1993).

In the following sections, we test whether the mandatory disclosure policy led to a lower willingness to pay for the lemons (and higher willingness to pay for non-lemons) and if the policy had a differential impact across sub-markets and income groups.

4 Data

To examine how the reform affected the housing market, we use three datasets: a mortgage dataset on housing unit transactions, a dataset of quarterly sales in projects and unit-level transactions recorded with the government registration department, and a database of ongoing real estate projects. Appendix table A.1 provides detailed description for each variable and the data source.

4.1 Property price data

We use housing unit transactions reported in a proprietary database by one of India's largest private mortgage lenders. The data was provided to us in 2021. We use data from 2015 to 2020 for the city of Mumbai.

The data contains details on mortgage applications approved by the bank, which includes the price[17] and area of the unit being purchased by the applicant. Using both, we calculate the price per square foot. We convert the price per square foot from nominal to real terms using the Maharashtra urban consumer price index. The log transformation of real price per square foot of the housing unit is the outcome variable of interest. The dataset includes other details like postcode of the unit, loan amount sanctioned, and age, occupation, gender and income of

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[17]Given the high prevalence of under-reporting of property values in sales agreements in India in order to evade taxes and fees, valuations reported in the private bank's mortgage dataset are more likely to be closer to the true value of the property (Anagol et al., 2022).

the homebuyer. We use these variables as controls. All units in the dataset are apartments in multi-family housing projects. For these projects, the dataset provides the unique registration number allotted by the Real Estate Regulatory Authority.

There could be a selection issue in this dataset if banks were able to verify whether a unit being purchased by an applicant faced litigation and rejected applications involving the purchase of litigated units. This could change the composition of litigated and non-litigated units in our sample, especially after the disclosure reform which made it easier for banks to verify the litigation status of a unit. We discuss the composition of litigated and non-litigated units in section 4.3. To know more about the approval process and ascertain whether the litigation status is verified before approving a loan application, we sought information about the loan approval process from the bank's website. The process involves assessing a loan applicant's eligibility based on criteria such as income and employment status, verifying documents such as the sale agreement with the developer and the building permissions given to the project (if it is under construction), and conducting a physical inspection of the unit. The litigation status is not explicitly included among the set of different aspects of the unit that the bank verifies.

Our second dataset is a dataset on unit-level transaction prices in Mumbai provided by PropEquity – a subscription-based real estate information portal. This dataset provides details recorded in registered real estate sale deeds and includes transaction prices, unit sizes (in square feet), and the unique registration number allotted by the Real Estate Regulatory Authority.

4.2   Housing projects data

Our third dataset comprises all ongoing housing projects in Mumbai that were registered with the Real Estate Regulatory Authority after its creation in May 2017. Each project is assigned a unique registration number by the authority. These projects had begun at different points in time, including in the years prior to and after the introduction of RERA, and were still under construction when the regulatory reform came into effect. Developers provide a range of information about the registered projects on a public website (see figure A.1). We scraped the website to compile a dataset of 2,953 under-construction projects in Mumbai.

The dataset includes attributes such as the size of the project, amenities provided in the project, start date and estimated completion dates of the project, litigation status, and details of the developers. We found geocoordinates for around 2,600 projects through a name search of each project on popular property search portals which provided their location on google maps.

For information about project-level sales of units, we make use of sales data from PropEquity. This dataset provides total number of units launched and quarterly number of units sold for every housing project in Mumbai.

4.3   Construction of dataset and key variables

We used the unique registration number provided in both the mortgage dataset and housing project dataset to match units to projects. We were able to match 11,553 units to 972 housing projects. Around 25% of these units were bought before the introduction of RERA and 75% were bought in the post-RERA period.

We check if the sample of 972 matched projects is representative of the population of 2,953 projects in terms of observed attributes like size, litigation status, and developer experience. For this, we use a t-test of differences in means of these attributes for projects in our sample and projects that are not within our sample. The mean share of projects built by developers with prior experience is slightly higher in our sample.[18] However, there is no difference between the two groups in terms of the average share of projects with litigation and average project size.

We use the unique registration number provided in the PropEquity dataset to match 39,009 units to 620 projects in the real estate project dataset. The number of transactions is higher in this dataset compared to the mortgage dataset since it includes all transactions registered with the government.

For analysing the impact of the reform on sales, we match 682 projects from the PropEquity sales database with projects from the real estate project dataset using the unique registration number.[19] We construct a project-level panel dataset with number of sales in each quarter. We then calculate the share of launched units that are sold in each quarter for every project.

Identifying sub-markets

The project-level data contains detailed information about the different types of amenities provided in the project. These include standard amenities like basement, podium, elevator, terrace, compound wall and so forth, and additional amenities (such as swimming pool or gymnasium). We classify projects having amenities that are available in a typical housing project (such as elevator or terrace) as non-luxury projects. We further classify projects with amenities like swimming pools, gymnasiums, club houses as luxury projects.[20]

Litigation status

The real estate project dataset reports the following details with respect to litigation: whether there is any litigation related to the project, the year of the litigation, and the court in which the dispute is being heard. Appendix figure A.1 shows the litigation details available on the website. These were self-reported by developers at the time of registering their project with the regulatory authority and include details of legal disputes that may have occurred before the reform. The litigation details are updated by developers periodically. Thus, if a project did not have litigation in 2017 but had a dispute at some point later, the litigation status is updated from "No" to "Yes" with details of the litigation (including the year of the litigation) uploaded on the project webpage. Hence, our dataset has projects that had litigation that began before and after the disclosure reform in 2017.

We create a litigation dummy that is assigned value 1 for projects having litigation, and 0 for projects that have no litigation. 30% of all projects in the real estate dataset face litigation and 48% of all housing units in our sample belong to projects facing litigation. If the litigation began between 2015-2020, the unit switches from being non-litigated to litigated – that is the

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[18]The difference in means is 1% and is statistically significant at 5%.

[19]Some projects in the PropEquity dataset had sales data but no transaction data.

[20]A project is considered to be a luxury project if it has at least one of the following exhaustive list of amenities: swimming pool, gymnasium, health club, steam room, spa and sauna, jacuzzi, fitness centre, amphitheatre, game room, reflexology centre, yoga studio, tennis court, badminton court, basketball court, golf course, car lift, jogging track, theatre, barbeque area, and bar.

litigation dummy switches from 0 to 1 – starting from the year of the dispute. For units in projects where the litigation began before 2015, the litigation dummy is 1 throughout.[21] Some litigated units were sold in the pre-reform period and some units were sold in the post-reform period when information about litigation status of a project became freely and publicly available. In the pre-reform period, potential homeowners may not easily be able to determine if a project faced litigation. Getting this information involved incurring substantial transactions costs and expending time and resources. If buyers or lenders were able to identify and hence avoid units facing litigation due to the disclosure, then we would see a fall in the share of litigated units in our dataset in the post-reform period. Moreover, if buyers anticipated the reform and timed the sale of litigated units right before its introduction, we would see a bunching of the share of litigated units sold in the period before the reform. In our sample, the share of litigated units sold in the pre-reform period is 45% and the share of litigated units sold in the post-reform period is 49% (see table A.2). The average share of litigated units sold in the last quarter before the reform is 51% and the average share of litigated units sold in the quarter after the reform is 53%. Hence, the composition of litigated and non-litigated units in our sample is similar across the pre- and post-reform periods.

Projects could have undergone a change in litigation status over time in terms of new legal disputes or cases getting resolved. Further, projects can have one or many legal disputes across different years. For units in projects with more than one legal cases, the litigation dummy switches to 1 starting from the year of the first case.

Table 1 provides summary statistics of the outcome variable and covariates for litigated and non-litigated units. The mean price per square foot is higher for litigated units compared to non-litigated units. The mean loan-to-value ratio is similar for litigated and non-litigated units, implying that on average lenders do not attribute a greater risk for litigated units.[22]

As seen in appendix figure A.1, we have information on the type of court in which the dispute is being heard. The Indian judiciary is made up of the Supreme Court of India at the apex and a high court for every state. These upper courts hear appeals of cases filed in lower courts. In addition, cases can directly be taken to the Bombay High Court if they are above a monetary value of INR 10 million.[23] Thus, we consider cases that are in these upper courts (that is, the Bombay High Court and the Supreme Court of India) to be more severe. Projects can have multiple legal disputes in multiple courts. Accordingly, projects with at least one case being heard in the upper court are considered to be affected by severe litigation[24] and projects

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[21]Of the 5,561 litigated units, 1,424 units were in projects where the dispute began between 2015-2020 and the remaining were in projects where the dispute began before 2015.

[22]However, the loan-to-value ratio is lower for litigated units than for non-litigated units in luxury projects. A t-test comparing the mean loan-to-value ratio across litigated and non-litigated units in luxury projects confirms that the means are statistically different for the two groups. On the other hand, there is no difference in the mean loan-to-value ratios between litigated and non-litigated units in high-priced sub-markets when sub-markets are classified using homeowners' incomes. Thus, it is difficult to say whether lenders systematically discriminate between litigated and non-litigated units by lowering their risk in case of lending to purchase litigated units in the high-income or luxury housing sub-market.

[23]This rule was promulgated in 2012 by adding section (3) in the Bombay City Court Act 1948 through an amendment following the Maharashtra Act. No. 25 of 2012. The full Act can be accessed here. Times of India (2012) reports the details and implications of this change in rules.

[24]For instance, if a project has two cases with one case in an upper court and one case in a lower court, it is classified as being affected by severe litigation. If a project has two cases and both are in a lower court, it is classified as being affected by less severe litigation.

Table 1: **Summary statistics**

Variables	All units		Litigated		Non-litigated	
Panel A: Variables used in baseline						
	Mean	Std dev	Mean	Std dev	Mean	Std dev
Unit price per sq. ft. (INR)	14903	6475	15894	6456	13984	6356
Log of price per sq. ft.	9.54	0.38	9.61	0.36	9.47	0.39
Unit area (sq. ft.)	796	528	872	542	726	505
Unit completion (Dummy)	0.48	0.50	0.50	0.50	0.46	0.50
Buyer's annual income (INR million)	3.06	10.90	3.78	13.67	2.40	7.41
Loan to value ratio	0.60	0.20	0.60	0.20	0.60	0.20
Buyer's gender (Female =1)	0.18	0.39	0.17	0.37	0.20	0.40
Obs	11,553		5,561		5,992	
Panel B: Luxury and non-luxury projects						
	Mean	Std dev	Mean	Std dev	Mean	Std dev
Luxury projects						
Log of price per sq. ft.	9.59	0.3	9.66	0.32	9.49	0.33
Unit area (sq. ft.)	831	624	880	505	756	768
Buyer's annual income (INR million)	3.29	8.92	3.87	11.00	2.38	3.68
Loan to value ratio	0.60	0.20	0.59	0.20	0.62	0.20
Obs	3,427		2,090		1,337	
Non-luxury projects						
Log of price per sq. ft.	9.51	0.4	9.58	0.38	9.46	0.41
Unit area (sq. ft.)	782	482	867	564	719	399
Buyer's annual income (INR million)	2.97	11.65	3.72	15.07	2.42	8.18
Loan to value ratio	0.60	0.20	0.60	0.20	0.59	0.20
Obs	8,104		3,465		4,639	

Note: Data spans years 2015 to 2020. Unit price and buyer's income are in real terms. Loan to value ratio is the ratio of the total loan amount and the value of the property. Property completion is a dummy variable with units under construction at the time of the loan application being assigned value 0, and completed units being assigned value 1. Some units get dropped when categorising projects as luxury or non-luxury as not all projects have data on amenities. For data sources see table A.1.

where all cases are in the lower court are considered to be affected by less severe litigation. Of the total units affected by litigation, 68.6% had cases in the upper courts (severe litigation).

There could be a further selection issue due to a switch from units facing severe litigation being sold in the pre-reform period to units facing less severe litigation being sold in the post-reform period. We do not see evidence of such a switch. The share of units facing severe litigation sold in the pre-reform period was 31.8%, which rose slightly to 33.4% in the post-reform period (see table A.2).

Panel B in Table 1 provides unit prices and litigation status for luxury and non-luxury projects. Log prices of units in luxury projects are higher than those in non-luxury projects. The average buyer income in the luxury sub-market is only marginally higher than the non-luxury sub-market. This is due to the fact that homebuyers in the top income quartiles purchase units in both luxury and non-luxury projects[25] and some homebuyers in the lowest income quartiles purchase units in some luxury projects. Figure A.2 shows the income distribution of homeowners by luxury and non-luxury sub-markets. While figure A.2 shows that buyers in the luxury market are from both lowest (Q1) and highest income (Q4) quartiles, the percent of buyers from these quartiles correspond to 20% and 27%, respectively.

[25]Rich homebuyers may be purchasing second homes or buying houses for investments in non-luxury projects (see Kaul, 2015).

5 Empirical Strategy

Our identification for testing the impact of the policy relies on using data on transactions before (when buyers had no public information on the unit's litigation status) and after the policy shock (when a unit's litigation status became publicly known). Using a two-way fixed effects regression we look at whether the difference in mean log of price per square foot between litigated and non-litigated units changed post reform. We use the following specification to estimate the impact of the reform:

$$ln\ Price_{ijt} = \rho\ \text{Litigation}_{ijt} + \mu\ \text{Post} + \beta\ \mathbf{1}(\text{Litigation=1}) \times 1(\text{Post}) +$$
$$\mathbb{X}_i + \delta_j + \lambda_t + \epsilon_{ijt} \quad (1)$$

where $ln\ Price_{ijt}$ is the log of price per square foot of housing unit i in housing project j in year-quarter t in real terms. $Litigation_{ijt}$ is a dummy variable taking value 1 if unit i within project j in time t is litigated and 0 otherwise. $Post$ is a dummy variable taking value 1 for all time periods after the introduction of RERA (in May 2017) and 0 for all time periods before. $\mathbb{X}_i$ is a vector of unit and buyer characteristics. δ_j are project fixed effects that control for time-invariant project specific characteristics. We include year-quarter fixed effects (λ_t) to control for factors varying across time periods that affected all units. ϵ_{ijt} is the error term and is clustered at the project level. We assume that the error term is not correlated with the interaction term of interest.[26] To control for time-varying changes within a postcode that could impact prices, we add a postcode-specific time-trend to eq. (1).

Our coefficient of interest is β, which estimates the impact of the reform on prices of litigated units. As discussed in section 3, we expect β to be negative. The coefficient ρ estimates the impact of litigation on prices in the pre-reform period. If buyers have no information about the unit's litigation status in this period, ρ will not be statistically significantly different from zero.

There could be potential measurement errors due to lack of information on the date, month, or quarter in which the litigation started. Our litigation dummy is assigned value one starting from the year of the first legal case. Our transaction prices are for each year-quarter. Hence, errors could arise if non-litigated units (bought at a time when the project did not have litigation) are classified as litigated. This measurement error could potentially create a downward bias in the absolute value of our estimated coefficient of interest.

We augment eq. (1) for every six-month period between 2015-2020 to test for parallel trends. For this we interacted $Litigation \times Post$ with each half-year period as shown in eq. (2) and plotted the coefficients.

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[26]On 8th November, 2016, the Indian government demonetised INR 500 and 1000 currency notes in order to reduce tax evasion (see Lahiri, 2020). According to the Economic Division, Ministry of Finance (2017, p.66), "[D]emonetisation could have particularly profound impact on the real estate sector. In the past, much of the black money accumulated was ultimately used to evade taxes on property sales." Year-quarter fixed effects would control for possible impact of demonetisation on the real estate sector as a whole.

$$ln\ Price_{ijt} = \rho\ \text{Litigation}_{ijt} + \mu\ \text{Post} + \sum_{t=2015h_1}^{2020h_2} (\beta_t\ \mathbf{1}(\text{Litigation=1}) \times 1(\text{Post}))+$$

$$\mathbb{X}_i + \delta_j + \lambda_t + \epsilon_{ijt} \quad (2)$$

To examine the heterogeneous impact of RERA on litigated projects in different sub-markets we estimate eq. (1) separately for sub-markets. Based on the types of amenities provided by developers, we classify projects as luxury and non-luxury projects. We also plot coefficients for every half-year period before and after the reform separately for each sub-market.

To see whether there is a differential impact of the disclosure policy across income groups we augment eq. (1) and interact *Litigation* × *Post* with income quartiles. Based on our discussion in section 3, our hypothesis is that post reform, β will be highest (in absolute terms) for the lowest income quartile.

6 Results

6.1 Baseline results

Table 2 reports coefficient estimates for eq. (1). We find that the log of price per square foot of litigated units declined relative to non-litigated units after the reform.[27] The coefficient is -0.044 without controls (column 1) and -0.046 when including buyer and unit controls (column 2). After controlling for postcode-level time-trend, the estimated coefficient is -0.059 (column 3). The coefficient is significant in all specifications. In other words, mean per square foot price of litigated units was around 4 - 6% less than that of non-litigated units after the reform.[28] The coefficient for litigation is not statistically significant, implying that buyers did not have information about a project's litigation status before 2017. Among the control variables, the coefficient for loan-to-value is positive and significant and the coefficient for unit area is negative and significant at the 1% level; for the latter the effect size is almost close to 0.[29,30]

We show the estimated effect on price per square foot for each half-year period before and after the reform using eq. (2) in Figure 2.[31] Prior to the reform, we fail to see a difference in the

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[27]There is a growing literature highlighting considerable weaknesses in two-way fixed effects estimations in the presence of heterogeneous treatment timing and multiple time periods (Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Roth et al., 2022). In our case, while projects face litigation in different years, the two time periods (pre- and post- reform) apply uniformly to all projects. In the pre-reform period, we compare litigated units for every time period with not-yet litigated (units that did not have litigation in that period but subsequently had litigation) and never-litigated units (units that never face litigation) in that time period and assume no difference in average prices between the two groups. Post May 2017, all units are uniformly exposed to the mandatory disclosure reform at the same time.

[28]Appendix figure A.3, which plots the conditional means of log of price per square foot for litigated and non-litigated units across years, shows that prices of litigated units fell while prices of non-litigated units rose after the reform.

[29]The results do not change if we drop the loan-to-value variable in the regression.

[30]Appendix figure A.6 shows the permutation test, where 1000 permutations of the baseline regression were run with projects being assigned as having litigation at random. The figure shows the distribution of the coefficients obtained in the permutations. The vertical line represents the coefficient estimate from column (2) in table 2. We find a small probability (p-value=0.01) of getting our result if we were to permute the litigated status at random.

[31]The figure shows each coefficient for half-year periods before and after the mandatory disclosure policy. The

Table 2: **Effect of disclosure policy on log prices**

VARIABLES	(1)	(2)	(3)
	Dep var: log of price per sq ft		
Litigation	-0.025	-0.021	-0.028
	(0.034)	(0.032)	(0.037)
Post	-0.006	-0.012	-0.010
	(0.021)	(0.021)	(0.021)
Litigation x Post	-0.044**	-0.046**	-0.059***
	(0.020)	(0.019)	(0.018)
Constant	9.427***	9.410***	9.771***
	(0.020)	(0.026)	(0.049)
Observations	11,553	11,553	11,553
R-squared	0.724	0.754	0.776
Year x quarter FE	Yes	Yes	Yes
Project FE	Yes	Yes	Yes
Unit controls	No	Yes	Yes
Buyer controls	No	Yes	Yes
Year x Post code	No	No	Yes

Note: Standard errors clustered at project level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table shows the results of the introduction of RERA on price of litigated units. The dependent variable is the log of price per square foot in real terms. Post takes value 1 for all time periods after the introduction of RERA. Column (1) reports coefficients with project and year-quarter fixed effects. Column (2) reports coefficients with project and year-quarter fixed effects, unit controls, which are completion status and area in square feet, and buyer controls - which include income, occupation, gender, and loan to value for the transaction. Column (3) reports coefficients with project and year-quarter fixed effects, unit controls, buyer controls and Year x Postcode trend.
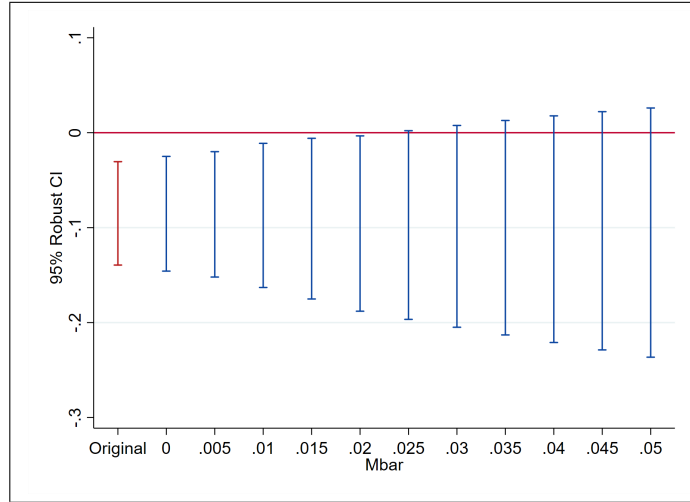
Figure 2: **Effect of disclosure policy on property prices bi-annually**
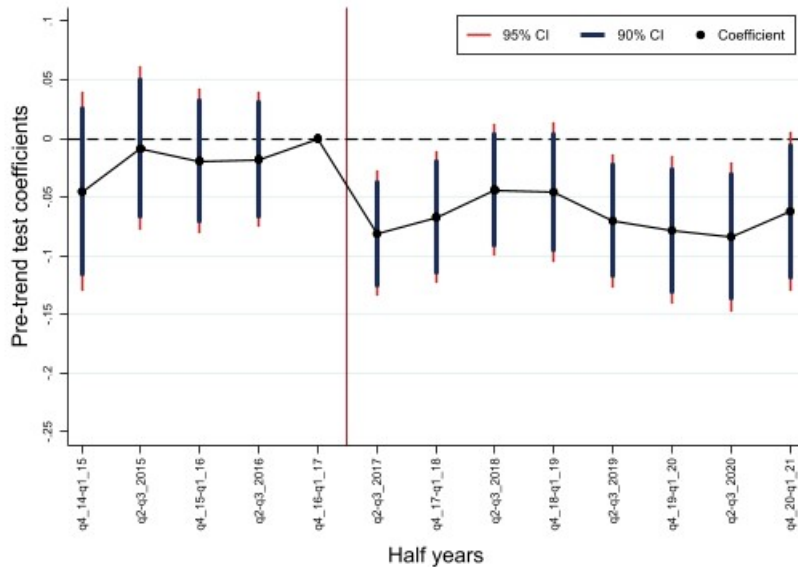
Note: The figure plots coefficients for each half-year period estimated by running eq. (2). The maroon vertical line represents the last pre-reform period before RERA

two extreme half-years do not have data for quarters Q4-2014 and Q1-2021.

Figure 3: **Sensitivity to Non-Parallel Trends**

Note: We make use of real estate projects that had transactions every year from 2015-2020. The maroon vertical line plots the confidence interval associated with the coefficient estimated in the first post-reform period.

prices of litigated and non-litigated units. In the post-reform period, the estimated coefficient is negative and statistically significantly different from zero for most periods. Our pre-trends test provides suggestive evidence that the parallel trends assumption is not violated.

We employ methods introduced by Rambachan and Roth (2023) to test the sensitivity of our baseline estimate to the presence of alternate parallel trends assumptions. These methods involve estimating bounds within which the causal effect would still hold even if the parallel trends assumption was violated. In Figure 3, we show the breakdown value of Mbar, that is, the value of Mbar beyond which we would be unable to reject a null effect. In other words, for the result in the first time period after treatment to hold, the slope of the differential trend should be no higher than 0.02 percentage points across consecutive periods.

Prior to the reform, the average per square foot price of a litigated unit was Rs.16,630. Using our preferred estimate of 5%, the reform led to a fall in the per square foot price of such units by approximately Rs.832. The mean size of litigated units sold after the reform is 850 square feet. Hence, the price of a litigated unit of average size fell by approximately Rs.707,200 after the reform.

To test whether projects with prolonged disputes see a greater decline in prices due to greater uncertainty of completion, risk of demolition, or greater possibility of information about litigation being known to buyers, we test the effect of the reform separately for different lengths of time between the first litigation and the transaction. The results, in table A.7, show that there is no effect of the reform on prices of litigated units when the litigation is very new (column 1 of table A.7). On the other hand, the disclosure had a significant impact when the project had been under litigation for between two and ten years. Surprisingly, the coefficient for the interaction term between litigation and post-reform period is not significant for projects where the disputes had been longer than ten years.

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6.2 Results by type of sub-market

The results in Table 3 show the impact of the reform for different housing sub-markets. For non-luxury projects (columns 1-2), the reform led to a decline in average prices of litigated units by around 5%. We see no impact of the reform on prices in luxury housing projects (columns 3-4). The heterogeneous results in terms of impact of the reform across sub-markets continue to hold for different classifications of luxury and non-luxury sub-markets as shown in table A.6. Interestingly, for luxury projects, we find that the coefficient for the litigation dummy, represented as ρ in eq. (1), is negative and significant; litigated units in the pre-reform period had approximately 10% lower price on average relative to non-litigated units.

Table 3: **Effect of disclosure policy on property prices by sub-markets**

VARIABLES	(1)	(2)	(3)	(4)
	Dep var: log of price per sq ft			
	Non-luxury projects		Luxury projects	
Litigation	0.016	0.013	-0.107***	-0.093***
	(0.038)	(0.036)	(0.031)	(0.032)
Post	0.001	0.003	-0.030	-0.046
	(0.025)	(0.024)	(0.040)	(0.041)
Litigation x Post	-0.053**	-0.049**	-0.022	-0.026
	(0.023)	(0.022)	(0.039)	(0.037)
Constant	9.418***	9.439***	9.903***	9.861***
	(0.023)	(0.033)	(0.046)	(0.051)
Observations	8,104	8,104	3,427	3,427
R-squared	0.731	0.763	0.695	0.730
Year x quarter FE	Yes	Yes	Yes	Yes
Project FE	Yes	Yes	Yes	Yes
Unit controls	No	Yes	No	Yes
Buyer controls	No	Yes	No	Yes

Note: Standard errors clustered at project level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table shows the impact of RERA on price of litigated units by type of project. The dependent variable is the log of price per square foot in real terms. Post takes value 1 for all time periods after the introduction of RERA. Columns (1) and (2) show the impact for non-luxury projects (classified based on types of amenities provided) and columns (3) and (4) show the impact for luxury projects. Columns (1) and (3) report coefficients with real estate project and year-quarter fixed effects and no controls. Columns (2) and (4) report coefficients with real estate project and year-quarter fixed effects, unit controls like completion status and area in square feet, and buyer controls - which include income, occupation, gender, and loan to value for the transaction.

In Figure 4, we show the coefficients for *Litigation × Post* for each half-year period before and after the reform for both luxury and non-luxury projects. The coefficients in the post-reform periods are positive and significant for some periods for non-luxury housing projects.

Testing for the sensitivity of this result to a possible violation in the parallel trends assumption, we see in figure A.5 that the slope of the differential trend should be no higher than 0.01 percentage points across consecutive periods for our result with respect to non-luxury housing to hold.

We propose two possible reasons for the heterogeneous sub-market results. First, those buying a unit in a luxury project have the resources to hire services for gathering information about any legal disputes. Second, disputes or issues facing prominent luxury housing projects are more likely to be covered by news media, thus helping disseminate crucial information to

Figure 4: **Effect of disclosure policy on property prices by project type**
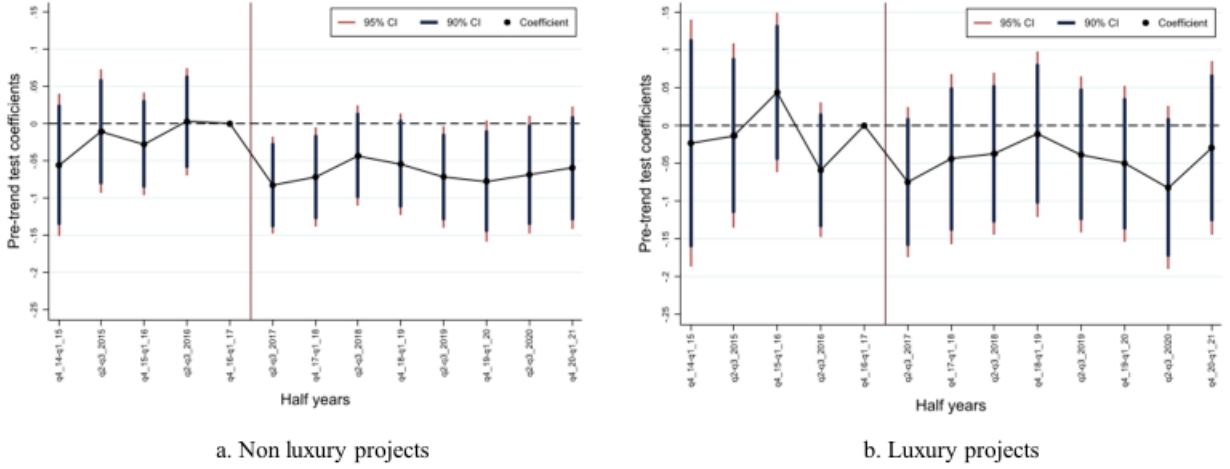
a. Non luxury projects

b. Luxury projects

Note: The figure plots coefficients for each half-year period before and after the introduction of RERA estimated by running eq. (2). The maroon vertical line represents the last pre-reform period before RERA.

potential buyers. Because information about litigation status was already known before the reform, possibly through these channels, mandatory disclosures did not affect prices of litigated units in the luxury sub-market. On the other hand, buyers of units in non-luxury projects did not know a project's litigation status until after the reform.[32] We investigate these mechanisms in section 7.

6.3 Results by income groups

We expect the impact of RERA on prices of litigated units to differ based on the income strata of buyers (see section 3). Low-income homebuyers may face high transaction costs in accessing information about the quality of the housing project before such information became publicly available due to RERA.

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[32] Acquiring information about the litigation status would have become easier for all buyers, including those in the luxury sub-market, due to the reform. However, since the price of litigated units relative to non-litigated units was already lower in the pre-reform period, we may not see a fall in prices of luxury units. Moreover, we cannot rule out the reform having an effect in this sub-market if developers of litigated projects compensate prospective buyers through non-monetary discounts such as free car parking or free upgrades to the unit.

Table 4: **Heterogeneous effect by homeowner income**

VARIABLES	(1)	(2)	(3)
	Dep var: log of price per sq ft		
Litigation x Post			
x Income Q1 (poorest)	-0.109***	-0.087***	-0.107***
	(0.022)	(0.022)	(0.021)
x Income Q2	-0.048**	-0.053**	-0.069***
	(0.022)	(0.021)	(0.020)
x Income Q3	-0.040*	-0.050**	-0.062***
	(0.023)	(0.021)	(0.020)
x Income Q4 (richest)	-0.008	-0.011	-0.019
	(0.022)	(0.020)	(0.019)
Constant	9.430***	9.421***	9.404***
	(0.020)	(0.027)	(0.034)
Observations	11,553	11,553	11,553
R-squared	0.726	0.755	0.777
Year x quarter FE	Yes	Yes	Yes
Project FE	Yes	Yes	Yes
Unit controls	No	Yes	Yes
Buyer controls	No	Yes	Yes
Year x Post code	No	No	Yes

Note: Standard errors clustered at project level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The table shows the results of the introduction of RERA on price of litigated units for each income quartile of the homebuyer. The dependent variable is the log of price per square foot in real terms. Post takes value 1 for all time periods after the introduction of RERA. Column (1) reports coefficients with real estate project and year-quarter fixed effects and without any controls. Column (2) reports coefficients with real estate project and quarter-year fixed effects, unit controls, which are completion status and area in square feet, and buyer controls - which include occupation, gender, and loan to value for the transaction. Column (3) reports coefficients with real estate project and year-quarter fixed effects, unit controls, buyer controls and Year x Postcode trend.

Our results are shown in table 4 and figure A.7. We report the effect of RERA on prices of litigated units for each income quartile group of homebuyers without unit and buyer controls in column (1). Column (2) shows the estimates after including the controls and column (3) additionally includes Year x Postcode time trend. The impact of the reform declines (in terms of the absolute value of the coefficient and level of significance) as we move from the lowest income quartile to the highest income quartile.

In all three columns, the coefficients are the largest for the lowest income quartile group and also significant at 1%. Prices of litigated units purchased by buyers belonging to the lowest income quartile fell by 9-11%. For homebuyers in the highest income quartile, we fail to see an impact of RERA on prices of litigated units.

7 Mechanisms

In this section we discuss possible mechanisms for a decline in prices in response to the reform. Next, we explain two possible reasons for differential impact across luxury and non-luxury sub-markets viz. household resources and media coverage.

7.1 Why do prices decline?

Our baseline results may be attributed to actions of both buyers and sellers (developers). With the reform, high transaction costs for accessing information about a project's quality ceased to be an impediment for prospective homebuyers. Once litigation status became known to all, prospective buyers would either switch from units in litigated projects to units in non-litigated projects or would bargain for a reduction in prices of units in litigated projects. However, because we do not have data on asking price, we cannot test whether bargaining power between buyers and sellers changed due to the reform.

Developers of litigated projects could respond to the reform by lowering prices if they find that the reform led to a fall in quantities of units sold in litigated projects.[33] In order to establish developer response of lowering prices as a potential channel, we test whether sales within litigated projects declined after the reform. Plotting the raw trends of quarterly sales over time, we see a sharp difference in sales between litigated and non-litigated units in the post-reform period (see fig. A.4). We also see a similar divergence in non-luxury projects but not in luxury projects.

We use the specification in eq. (3) where the dependent variable *%Sales*$_{jt}$ is the share of quarterly sales of units in project j in year-quarter t to the total launched units in that project. *Litigation*$_{jt}$ is a dummy variable taking value 1 if project j in time t is litigated and 0 otherwise. *Post* is a dummy variable taking value 1 for all time periods after the reform and 0 for all time periods before. β is the coefficient of interest. $\mathbb{X}_{jt}$ is the project-level share of unsold inventory stock to the total launched units at the beginning of year-quarter t. λ_t and δ_j are year-quarter fixed effects and project fixed effects respectively. ϵ_{jt} is the error term and is clustered at the project level.

$$\% \ Sales_{jt} = \rho \ \text{Litigation}_{jt} + \mu \ \text{Post} + \beta \ \mathbf{1}(\text{Litigation=1}) \times 1(\text{Post}) + \mathbb{X}_{jt} +$$
$$\lambda_t + \delta_j + \epsilon_{jt} \quad (3)$$

Table 5 shows the impact of the disclosure reform on quarterly sales. We find that *%Sales* for litigated projects declined by 0.5 percentage points after the reform. This is a large effect given that the mean of percentage quarterly units sold is around 1.1 percent (see table A.5). We find that quarterly sales for non-luxury projects declined after the reform. The pre-trends test, shown in fig. 5, provides suggestive evidence that the parallel trends assumption is not violated for non-luxury projects.[34] While sales of luxury projects also fell in the post-reform

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[33]Developers may provide non-monetary incentives such as free upgrades or free car parking instead of reducing prices. In this case, we will not observe an impact on prices after the reform.

[34]We cannot test for sensitivity to the parallel trends assumption using the method given by Rambachan

Table 5: **Effect of disclosure policy on sales**

VARIABLES	(1)	(2)	(3)
	Dep var: % unit sales in a quarter		
	All	Non-Luxury	Luxury
Litigation	0.492**	0.425	0.823**
	(0.231)	(0.262)	(0.403)
Post	3.593***	3.418***	4.480***
	(0.179)	(0.193)	(0.483)
Litigation x Post	-0.462***	-0.434***	-0.600*
	(0.128)	(0.139)	(0.318)
Constant	-9.754***	-9.628***	-8.919***
	(0.370)	(0.407)	(0.780)
Observations	16,345	13,633	2,616
R-squared	0.232	0.237	0.222
Year x quarter FE	Yes	Yes	Yes
Project FE	Yes	Yes	Yes

Note: Standard errors clustered at project level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The table shows the results of the introduction of RERA on units sold in a project. The dependent variable is the % of unit sales of a project in a quarter by the total launched units. Post takes value 1 for all time periods after the introduction of RERA. The table shows results for all, non-luxury and luxury projects in columns (1), (2) and (3) respectively. All columns report coefficients with year-quarter fixed effects and project fixed effects and control for project-level unsold inventory stock from the previous quarter.

Figure 5: **Effect of disclosure policy on sales by project type**

a. Non-luxury projects

b. Luxury projects

c. All projects
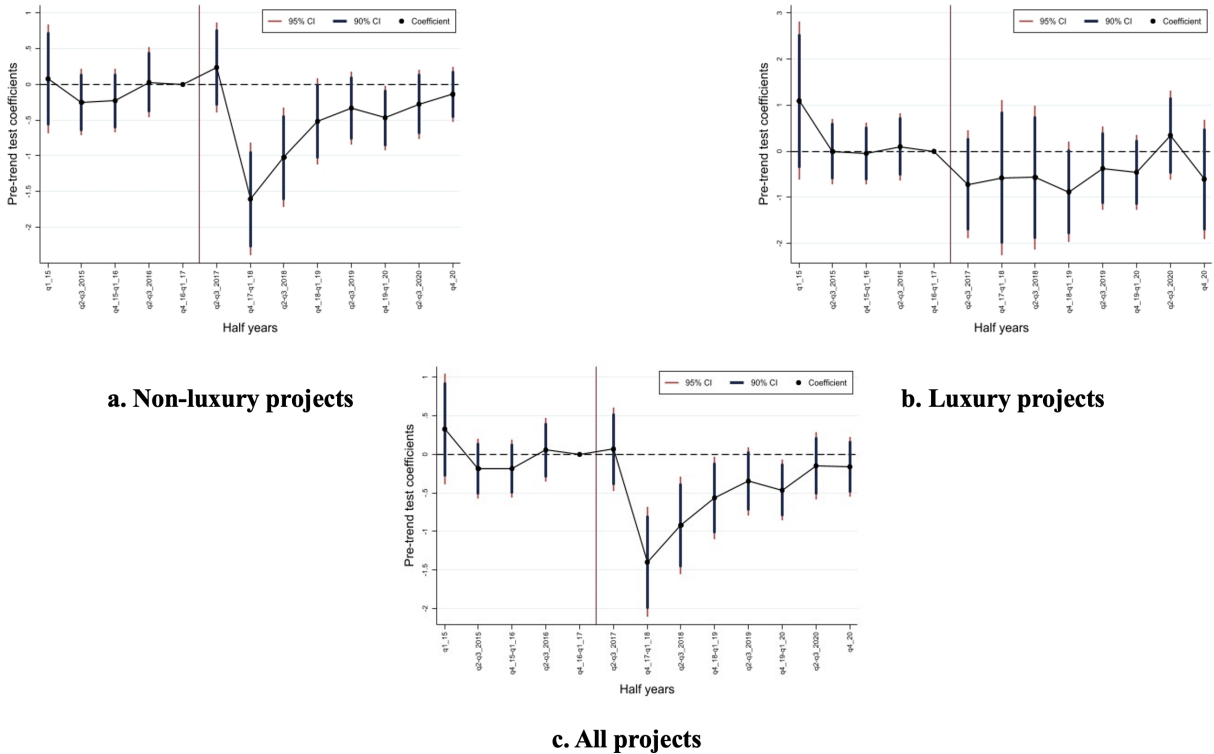
Note: The dependent variable is quarterly %sales for a project. The figure plots estimated coefficients for each half-year period before and after the introduction of RERA. The maroon vertical line represents the last pre-reform period before RERA. Figure A is for non-luxury projects, B is for luxury projects and C is for all projects.

and Roth (2023) here since it applies for the coefficient in the first time period after the 0 event period. The

period, the parallel trends assumption does not hold for this category. These results suggest that the decline in sales of litigated projects could drive developers to lower prices only in the non-luxury sub-market.

7.2 Why did the reform have differential impact across sub-markets?

In section 6.2, we showed that the reform affected prices of litigated units only in the non-luxury sub-market. Further, prices of litigated units in luxury projects were 10% lower on average in the pre-reform period. This suggests that buyers across the two sub-markets had differential access to information about the litigation status before the reform. We propose and examine the evidence for two possible channels of information. The first mechanism relates to the differential ability of households across income groups within a sub-market to expend resources for gathering information regarding the legal status of a project. Second, disputes or issues facing prominent luxury projects are more likely to be covered by news media, thus helping disseminate crucial information to potential buyers in the luxury sub-market.

Table 6: **Heterogeneous effects by income and sub-market**

VARIABLES	(1)	(2)	(2)	(3)
	Dep var: log of price per sq ft			
	Non-Luxury		Luxury	
Litigation x Post				
x Income Q1 (poorest)	-0.107***	-0.088***	-0.108**	-0.077*
	(0.025)	(0.025)	(0.044)	(0.044)
x Income Q2	-0.049*	-0.051**	-0.041	-0.044
	(0.026)	(0.025)	(0.041)	(0.039)
x Income Q3	-0.049*	-0.051**	-0.017	-0.029
	(0.026)	(0.025)	(0.042)	(0.039)
x Income Q4 (richest)	-0.022	-0.012	0.023	0.013
	(0.026)	(0.025)	(0.040)	(0.037)
Litigation	0.017	0.013	-0.110***	-0.097***
	(0.038)	(0.037)	(0.033)	(0.033)
Constant	9.421***	9.449***	10.233***	10.040***
	(0.024)	(0.033)	(0.051)	(0.081)
Observations	8,104	8,104	3,427	3,427
R-squared	0.732	0.763	0.701	0.732
Year x quarter FE	Yes	Yes	Yes	Yes
Project FE	Yes	Yes	Yes	Yes
Unit controls	No	Yes	No	Yes
Buyer controls	No	Yes	No	Yes

Note: Standard errors clustered at project level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table shows the results of the introduction of RERA on price of litigated units for each income quartile of the homebuyer in the non-luxury and luxury sub-markets. Figure A.2 shows the distribution of homeowners across income quartiles by luxury and non-luxury sub-markets. The dependent variable is the log of price per square foot in real terms. Post takes value 1 for all time periods after the introduction of RERA. Columns (1) and (3) reports coefficients with real estate project and year-quarter fixed effects and without any controls. Columns (2) and (4) reports coefficients with real estate project and quarter-year fixed effects, unit controls, which are completion status and area in square feet, and buyer controls - which include occupation, gender, and loan to value for the transaction.

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coefficient in this time period is not statistically significantly different from zero. It appears that sales of litigated units did not immediately fall after the reform but instead fell after a lag. It may be the case that sales include pre-bookings (which may have happened before the reform) as well as new bookings, and the lag is due to the inclusion of former.

Before buying real estate, buyers are recommended to seek legal advice to ensure that there are no land titling issues, litigation, and delayed approvals.[35] Incomes of households would determine the resources available for hiring "superior lawyers" (see Glaeser and Shleifer, 2003, p.406) and hence access to private information about a project's litigation status. In table 4 we showed that the reform had the highest impact on buyers in the lowest income quartile and no impact on buyers in the richest income quartile. We should see the same heterogeneity in impact across both the luxury and non-luxury sub-markets.

Table 6 shows that the reform had a null impact (and the smallest coefficient in absolute terms) on prices of units purchased by buyers in richest income quartile in both sub-markets.[36] In contrast, the lowest income quartile witnessed a decline in prices of litigated units of a magnitude of around 11% in both sub-markets (see columns (1) and (3)). Finally, prices for homebuyers in the non-luxury sub-market who belonged to the second and third income quartiles fell after the reform but there was no effect for these income quartiles in the luxury sub-market. This indicates that individual resources may not be the sole channel and that other factors that affect the two sub-markets differently could also be playing a role.

Table 7: **Impact of negative media coverage on prices before disclosure policy**

VARIABLES	(1)	(3)	(4)
	Dep var: log of price per sq ft		
	All	Non-luxury	Luxury
Media dummy	0.011	-0.043	0.160
	(0.085)	(0.076)	(0.114)
Constant	10.063***	9.707***	10.077***
	(0.041)	(0.058)	(0.082)
Observations	1,320	841	479
R-squared	0.835	0.840	0.829
Year x quarter FE	Yes	Yes	Yes
Project FE	Yes	Yes	Yes
Unit controls	Yes	Yes	Yes
Buyer controls	Yes	Yes	Yes

Note: Standard errors clustered at project level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The table shows the results of media coverage on price of units. We restrict the sample to only those transactions that take place before the introduction of RERA and if the project has litigation against it. The dependent variable is the log of price per square foot in real terms. Media dummy take the value 1 if a project has a media article mentioning litigation or some other negative news against it. Column (1) reports results for all projects. Column (2) reports results for non-luxury projects. Column (3) reports results for luxury projects. All columns report coefficients with report coefficients year-quarter fixed effects, project fixed effects, unit controls, which are completion status and area in square feet, and buyer controls - which include occupation, gender, and loan to value for the transaction.

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[35] For example, Economic Times (2015) advises homebuyers to "*hire the services of a legal advisor and discuss all property-related documents with him before signing on the dotted line with the builder.*"

[36] The income quartiles are based on distribution of incomes of homebuyers for the full sample.

We test whether the heterogeneous effects are driven by differences in media coverage of legal disputes facing projects in the two sub-markets. For this, we searched for news stories online that reported legal disputes faced by projects in the years before the reform (2014-17).[37] We created a dummy variable taking value 1 for all units in litigated projects that were covered by the news media and 0 for units in litigated projects that received no such coverage.[38] In our sample, 37% and 6% of all litigated units were impacted by media coverage in the luxury and non-luxury sub-market, respectively. We test if units in litigated projects that got media attention had lower prices on average than units in litigated projects that had no media coverage. The results, presented in table 7, show no impact of media attention on prices of litigated units. We fail to find evidence of media coverage as a channel for disseminating information about the litigation status of a luxury project to homebuyers.

8 Robustness checks

8.1 Supplementary price data

Our results could be biased if transaction prices reported in the mortgage dataset have selection issues.[39] To address this, we use transaction prices from a different dataset as described in section 4.1.[40] We matched 39,009 transactions to 620 projects in the RERA dataset.

We estimate the baseline regression eq. (1) using this dataset. Our results, presented in table 8, show that the reform led to a decline in prices of litigated units overall and for non-luxury projects (columns (1) and (2)). The pre-trends tests, shown in fig. 6, suggest that the parallel trends assumption holds. Consistent with the results in table 3, we see no impact of the reform on prices of litigated units in the luxury sub-market (column (3)).

The *Litigation* x *Post* coefficient for columns (1) and (2) is higher than our baseline results in tables 2 and 3. The difference in estimates could be due to inaccuracies in reporting transaction values in either dataset. Anagol et al. (2022) use PropEquity data to show that transactions values for units having mortgages from private sector banks see much less under-reporting compared to transaction values of units with mortgages from public banks.[41] Hence, we believe

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[37]We looked for news articles in published in English for each litigated project using the Google search engine. The first round of searching involved typing out the name of the project in the Google 'News' tab, and going through all news articles found. A date cutoff of May, 2017, was set so only those articles that were published before 1st May, 2017 (i.e. the beginning of the reform), were recorded. The next round of search was similarly carried out by typing out the name of the developer and going through all news articles that come up. Different combinations of keywords—such as 'court', 'litigation', 'problem', 'delay'—were also used along with the developer and project names to ensure that we sift through all possible news articles available on that project/ developer. Articles that mentioned adverse events that had the potential to negatively affect the reputation of the project/ developer were recorded.

[38]The media dummy switches on to 1 beginning from the date of the article's publication, and remains 1 thereafter. In case multiple articles were found, the dummy turns on to 1 on the date of publication of the earliest article.

[39]This could be in the form of unobserved changes in the composition of buyers who were given a mortgage if the private bank was able to verify the litigation status of a project after the reform.

[40]This dataset provides details of transaction prices, unit sizes (in square feet), and project-level identifiers. Using project-level identifiers, we match transactions from this dataset to a smaller set of projects from the RERA dataset.

[41]Unfortunately, we cannot distinguish between transactions having mortgages from public sector banks and private sector banks in the data we have from PropEquity.

Table 8: **Effect of disclosure policy using alternate property price data**

VARIABLES	(1)	(2)	(3)
	Dep var: log of price per sq ft		
	All	Non luxury	Luxury
Litigation	-0.102***	-0.085*	-0.135***
	(0.030)	(0.046)	(0.032)
Post	0.061**	0.053*	0.054
	(0.024)	(0.027)	(0.039)
Litigation x Post	-0.123***	-0.140***	-0.047
	(0.032)	(0.039)	(0.050)
Constant	10.049***	10.039***	9.618***
	(0.045)	(0.051)	(0.024)
Observations	39,009	27,928	11,002
R-squared	0.668	0.681	0.596
Year x quarter FE	Yes	Yes	Yes
Project FE	Yes	Yes	Yes
Unit controls	Yes	Yes	Yes

Note: Standard errors clustered at project level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The table shows the results of the introduction of RERA on prices of litigated units using data from PropEquity. The dependent variable is the log of price per square foot in real terms. Post takes value 1 for all time periods after the introduction of RERA. Column (1) reports results for all projects, column (2) reports results for non-luxury projects and column (3) reports results for luxury projects. All columns report coefficients with year-quarter fixed effects, project fixed effects and units controls (size of the unit).

that the coefficient estimates in tables 2 and 3 using the dataset from the private sector bank are more accurate than estimates from the data of all public and private sector banks transactions.

Figure 6: **Effect of disclosure policy using alternate property prices data**
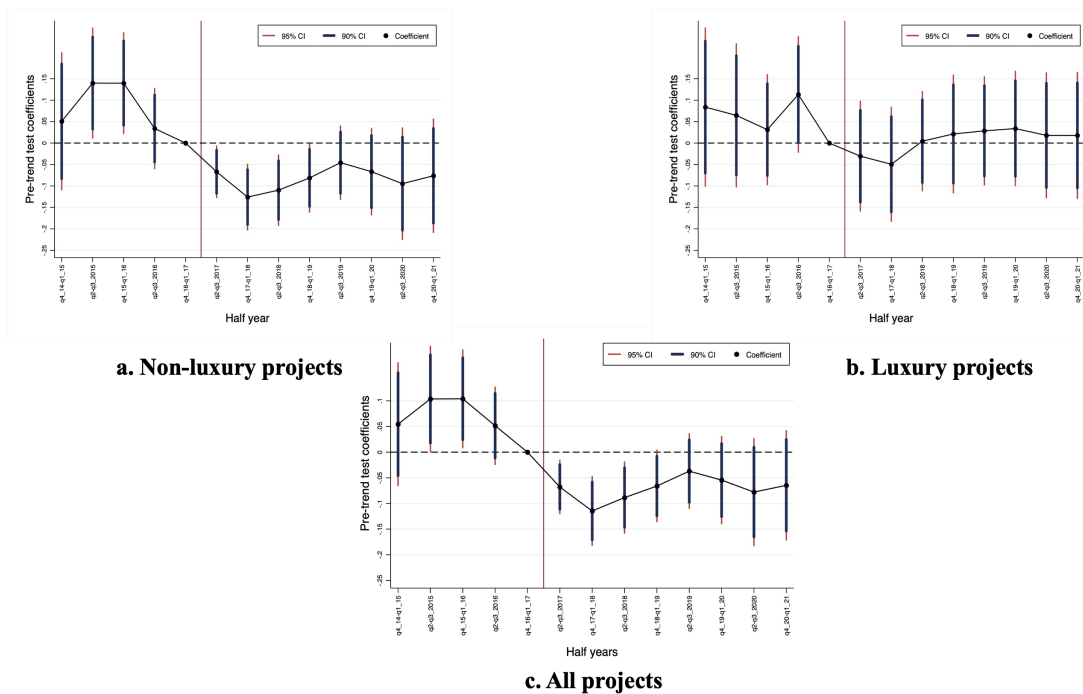
a. **Non-luxury projects**

b. **Luxury projects**

c. **All projects**

Note: The figure plots coefficients for each half-year period before and after the introduction of RERA estimated by running eq. (2). The maroon vertical line represents the last pre-reform period before RERA. Figure A is for non-luxury projects, B is for luxury projects and C is for all projects.

8.2 Testing for violations of SUTVA

Stable Unit Treatment Value Assumption (SUTVA), which is a key condition for our identification to be causal, entails that litigated units have no spillover effects on the outcomes of non-litigated units. This could be violated if information about litigation against a project by a developer causes reputation loss that spills over to other projects built by her.[42] Our sample includes projects with and without litigation built by the same developer. Thus, our coefficient estimates could underestimate the true effect of RERA. To address this we drop units in non-litigated projects built by a developer who had other litigated projects. A total of 381 units in 35 projects built by 18 developers who had other projects facing legal disputes were dropped.

Table 9: **Results without non-litigated projects by developers who had litigated projects**

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
			Dep var: log of price per sq ft			
	All		Non luxury		Luxury	
Litigation	-0.026	-0.022	0.018	0.015	-0.107***	-0.093***
	(0.035)	(0.033)	(0.039)	(0.037)	(0.031)	(0.032)
Post	-0.003	-0.008	0.005	0.009	-0.031	-0.047
	(0.022)	(0.022)	(0.026)	(0.025)	(0.042)	(0.042)
Litigation x Post	-0.051**	-0.053***	-0.062***	-0.058**	-0.021	-0.025
	(0.021)	(0.019)	(0.024)	(0.022)	(0.040)	(0.038)
Constant	9.420***	9.403***	9.410***	9.430***	9.902***	9.859***
	(0.021)	(0.027)	(0.025)	(0.035)	(0.048)	(0.054)
Observations	11,172	11,172	7,790	7,790	3,360	3,360
R-squared	0.721	0.751	0.727	0.760	0.695	0.730
Year x quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Project FE	Yes	Yes	Yes	Yes	Yes	Yes
Unit controls	No	Yes	No	Yes	No	Yes
Buyer controls	No	Yes	No	Yes	No	Yes

Note: Standard errors clustered at project level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table shows the results of the introduction of RERA on price of litigated units. The dependent variable is the log of price per square foot in real terms. Post takes value 1 for all time periods after the introduction of RERA. Columns (1) and (2) report the estimates for the full sample, columns (3) and (4) report the results for the sample of non-luxury projects and columns (5) and (6) report the results for luxury projects. Columns (1), (3) and (5) report coefficients with real estate project and year-quarter fixed effects and without any controls. Columns (2), (4) and (6) report coefficients with real estate project and year-quarter fixed effects, unit controls, which are completion status and area in square feet, and buyer controls - which include income, occupation, gender, and loan to value for the transaction.

The results in Table 9 show that in the full sample (columns 1 and 2) and in the sample of non-luxury projects (columns 3 and 4), coefficient estimates are slightly higher (in absolute terms) than the estimates in table 2 and table 3 respectively. On average, RERA led to a 5.1-6.2% decline in prices of litigated units.

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[42]Spillover effects of reputation loss have been documented in literature in the automobile industry (Bachmann et al., 2023), dairy industry (Bai et al., 2022), toy industry (Freedman et al., 2012), among others. Recent newspaper articles have also highlighted the issues of reputation in the real estate industry in Mumbai (Moneycontrol, 2021).

8.3 Matching procedures

The presence of unobserved covariates (such as neighborhood-specific factors) that impact selection into litigation and prices differently in the pre- and post-reform period could bias our results. As a robustness test, we matched litigated units and non-litigated units on distance. Here, we restricted the sample to the pre-reform period and identified all non-litigated units within 500 meters distance of a litigated unit. We estimate the effect of RERA for this matched sample using eq. (1). We also estimate the effect of RERA after weighting each observation with the propensity score weights estimated for a matched sample using project size in the pre-reform period.

Our results are reported in table 10. Columns (1) and (2) show coefficient estimates for using matching on distance for all projects and non-luxury projects respectively. The coefficient is negative and significant throughout and is between 10-12% . Using propensity score weights, we find that disclosure reform led to around 4-6% decline in property prices for the non-luxury sub market and has no impact on the luxury sub-market (columns 4-5).

Table 10: **Matching procedures**

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
			Dep var: log of price per sq ft			
	Matched on distance (500 mts)			PSM (Size)		
	All	Non luxury	Luxury	All	Non luxury	Luxury
Litigation	0.088***	0.297***	0.292**	-0.033	-0.004	-0.047
	(0.024)	(0.024)	(0.086)	(0.032)	(0.039)	(0.041)
Post	0.032	0.058*	-0.166**	-0.027	-0.007	-0.029
	(0.032)	(0.035)	(0.057)	(0.028)	(0.027)	(0.066)
Litigation x Post	-0.106***	-0.117***	-0.127	-0.039*	-0.054**	-0.017
	(0.028)	(0.029)	(0.073)	(0.020)	(0.026)	(0.046)
Constant	9.738***	9.697***	9.426***	9.802***	9.514***	9.838***
	(0.048)	(0.049)	(0.106)	(0.050)	(0.047)	(0.065)
Observations	4,082	2,401	291	6,117	4,014	2,346
R-squared	0.682	0.709	0.682	0.755	0.775	0.698
Year x quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Project FE	Yes	Yes	Yes	Yes	Yes	Yes
Unit controls	Yes	Yes	Yes	Yes	Yes	Yes
Buyer controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors clustered at project level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table shows the results of the introduction of RERA on price of litigated units after matching on distance and propensity score matching on project size. The dependent variable is the log of price per square foot in real terms. Post takes value 1 for all time periods after the introduction of RERA. Columns (1), (2) and (3) report coefficients for matching on distance for all projects, non-luxury projects, and luxury projects respectively. Columns (4), (5) and (6) report the results after propensity score matching on size for all, non-luxury, and luxury projects respectively. All columns report coefficients with real estate project and year-quarter effects, unit controls, which are completion status and area in square feet, and buyer controls - which include occupation, gender, and loan to value for the transaction.

8.4 Alternate explanations: severity of litigation

Our results of differential impact of the reform across housing sub-markets could be on account of differences in the severity of litigation rather than differences in access to information for homeowners across the sub-markets. For instance, the price fall after the reform for non-luxury projects seen in table 3 may be due to more severe litigation faced by these projects. To address this, we compare the impact of the reform across sub-markets separately by severity of litigation. We define severity of litigation in two ways – the level of courts (upper and lower) in which disputes are being heard and the number of cases against the project.

Table 11: **Heterogeneous effects of disclosure policy by type of court**

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Dep var: log of price per sq ft					
	All		Non Luxury		Luxury	
	Upper	Lower	Upper	Lower	Upper	Lower
Litigation	-0.031	0.023	0.004	0.045	-0.101***	-0.053
	(0.040)	(0.042)	(0.046)	(0.054)	(0.035)	(0.058)
Post	-0.021	-0.024	-0.007	-0.011	-0.050	-0.060
	(0.023)	(0.025)	(0.025)	(0.027)	(0.045)	(0.058)
Litigation x Post	-0.052**	-0.036	-0.050**	-0.046	-0.037	0.002
	(0.021)	(0.026)	(0.026)	(0.033)	(0.040)	(0.040)
Constant	9.397***	9.440***	9.427***	9.490***	9.865***	9.517***
	(0.027)	(0.040)	(0.037)	(0.039)	(0.052)	(0.062)
Observations	9,806	7,739	6,836	5,907	2,948	1,816
R-squared	0.755	0.741	0.764	0.760	0.729	0.698
Year x quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Project FE	Yes	Yes	Yes	Yes	Yes	Yes
Unit controls	Yes	Yes	Yes	Yes	Yes	Yes
Buyer controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors clustered at project level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The table shows the impact of RERA on price of litigated units by type of court for all units, luxury units and non luxury units. The dependent variable is the log of price per square foot in real terms. Post takes value 1 for all time periods after the introduction of RERA. Columns (1), (3), and (5) show the impact for cases in upper courts and columns (2), (4) and (6) show the impact for cases in lower courts. All columns report coefficients with real estate project and year-quarter fixed effects, unit controls like completion status and area in square feet, and buyer controls - which include income, occupation, gender, and loan to value for the transaction.

A dispute will reach the upper court (that is, the Bombay High Court and the Supreme Court of India) if litigants are not satisfied with the verdict in the lower court. In addition, cases may directly be taken to the Bombay High Court if they are above a monetary value of INR 10 million. Litigants expend more resources and time when disputes are heard in an upper court. Thus, we consider cases that are in these upper courts to be more severe. We classify projects with at least one legal dispute being heard in an upper court as having severe litigation whereas projects with cases being heard only in the lower court are considered to have less severe litigation. For instance, if a project has two cases, with one case in an upper court and one case in a lower court, it is classified as having severe litigation. If a project has two cases and both are in a lower court, it is classified as having less severe litigation. Of the total units affected by litigation in our sample, 68.6% had cases in the upper courts, that is, were affected by severe litigation.

Table 12: **Heterogeneous effects of disclosure policy by number of cases**

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Non-luxury		Luxury	
VARIABLES	One	Two or more	One	Two or more	One	Two or more
Litigation	-0.030	-0.027	0.014	0.012	-0.096***	-0.184***
	(0.037)	(0.063)	(0.049)	(0.039)	(0.034)	(0.042)
Post	-0.030	-0.019	-0.014	-0.007	-0.074	-0.046
	(0.025)	(0.023)	(0.027)	(0.025)	(0.062)	(0.043)
Litigation x Post	-0.037	-0.051**	-0.052**	-0.044	0.022	-0.034
	(0.023)	(0.023)	(0.025)	(0.032)	(0.043)	(0.039)
Constant	9.419***	9.414***	9.461***	9.448***	9.741***	9.961***
	(0.037)	(0.029)	(0.036)	(0.040)	(0.045)	(0.043)
Observations	8,044	9,501	6,271	6,472	1,751	3,013
R-squared	0.748	0.752	0.767	0.758	0.687	0.734
Year x quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Project FE	Yes	Yes	Yes	Yes	Yes	Yes
Unit controls	Yes	Yes	Yes	Yes	Yes	Yes
Buyer controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors clustered at project level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The table shows the results of the introduction of RERA on price of litigated units by number of cases for all units, non-luxury units, and luxury units. The dependent variable is the log of price per square foot in real terms. Post takes value 1 for all time periods after the introduction of RERA. All columns reports coefficients with project and year-quarter fixed effects, unit controls, and buyer controls.

Our second measure of severity takes into account the number of cases against a project. Units in projects with multiple disputes are defined as having severe litigation and those in projects with a single dispute are defined as having less severe litigation. Of the total units with litigation, 63% had more than one dispute.

Using eq. (1), we estimate the impact of the reform on prices of units having severe and less severe litigation separately. The results using the upper court and lower court classification for severity are shown in table 11. The reform led to a 5% fall in price of units facing a severe form of litigation (column (1)) but had no effect on the prices of units facing less severe litigation (column (2)). Comparing non-luxury and luxury sub-markets, our results for upper courts (columns (3) and (5)) are consistent with table 3.[43] In other words, the differential impact of the reform across the sub-markets (and thus differences in access to information for homeowners) continues to hold for severe litigation.

In table 12, we see that the reform led to a decline in prices of litigated units with multiple disputes (column (2)). Consistent with results in table 3, the reform had no effect on prices of litigated units in the luxury sub-market regardless of the number of disputes. In the non-luxury sub-market, prices of litigated units fell after the reform but the effect is not statistically significant for units with multiple disputes. Overall, the heterogeneous effects across sub-markets is not due to differences in severity of litigation as defined by the number of disputes.

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[43]While the type of court is a useful proxy for severity of litigation, it may be the case that new litigation for new projects starts at lower courts while longstanding litigation finds its way to the upper courts. Here, comparing effects across different courts within the same year-quarter could be comparing older projects, with longstanding litigation, to newer projects for which litigation is just starting. Even so, for examining the heterogeneous effects across sub-markets, we are interested in comparing the effects of severe litigation for luxury and non-luxury projects. In this case, we are effectively comparing older projects in both sub-markets.

9 Conclusion

We examine the impact of a policy reform in the state of Maharashtra that mandated public disclosure of litigation status of housing projects on house prices. Using unit-level data on prices and litigation status of housing projects, we find that the reform led to a 4-6% decline in per square foot prices of litigated units relative to non-litigated units. The decline in prices could be a result of a developer response to the reform since the reform led to a fall in sales of units in litigated projects.

We find that public disclosure in the housing market resolves information asymmetry primarily in the non-luxury housing sub-market and for low-income buyers. A related finding is that in the pre-reform period, litigated units in luxury projects had lower prices on average relative to non-litigated units. This indicates that buyers in the luxury housing sub-market had better access to information prior to the reform.

This could be driven by two channels. First, buyers of luxury units had the means and resources to invest in collecting accurate information. Second, they had access to better information through media, which is more likely to report disputes facing prominent luxury housing projects. We find suggestive evidence for household resources but not for media reporting in explaining these results.

Our results show that low-income buyers paid too high a price for lemons. With disclosure, the true quality was revealed and prices of litigated housing fell. Disclosure forces developers to bear the burden of the cost of litigation. If litigation signals legitimate issues of quality, disclosure creates incentives for developers to incur costs for improving quality and therefore, improves housing quality and increases prices without affecting supply. While most Indian states have set up a real estate regulatory authority, many do not require developers to report ongoing legal disputes on the regulators' website (see table A.8). We present a strong case for mandating disclosure of litigation in order to alleviate information asymmetry for potential homebuyers in these states.

However, in so far as litigation is a result of poor land titling, approval delays, communities stopping development (also known as NIMBYs), or parties suing developers for frivolous reasons (Gandhi et al., 2021), it cannot be completely avoided. Litigation also increases uncertainty and delays project completion due to lengthy resolution times. Delays have been documented in literature to reduce housing supply (see Mayer and Somerville, 2000) resulting in an overall increase in the house prices and unaffordability (Glaeser and Gyourko, 2018). Thus, if litigation cannot be altogether avoided, disclosure could have unintended effects. Since developers would bear the cost of litigation, it could lead to developers exiting the sector and new developers being dissuaded from entering it, thus affecting supply. Therefore, our results have implications for broader cross-cutting reforms for strengthening property rights and contract enforcement in order to improve the functioning of land and real estate markets in developing countries.

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Appendix

Table A.1: Variable descriptions and datasets

Variable	Description	Dataset	Obs
Housing unit level attributes			
Unit price	Price in Indian rupees, as reported by the buyer on the Approval Date for units transacted between 2015-2020	Bank mortgage dataset	11,553
Area in sq ft	Total built-up area of the unit in square feet.	Bank mortgage dataset	11,553
Unit price per sq ft	Unit price/unit area in sq ft	Calculated by authors	11,553
Unit price(alternate)	Price in Indian rupees as reported on sales deed registered with the government	Propequity	39,009
Unit size(alternate)	Area of unit in square feet	Propequity	39,009
Log price per sq ft in real terms(alternate)	Log of unit price per square foot in real terms	Calculated by authors	39,009
Price per sq ft in real terms	Unit price per sq ft converted from nominal to real terms using Maharashtra urban consumer price index	Calculated by authors	11,553
Log price per sq ft in real terms	Log of unit price per square foot in real terms	Calculated by authors	11,553
Project completion	Units are either 'completed' or 'under construction', depending on their stage of completion on the Approval Date. Determined by physical visits and evaluation by the bank.	Bank mortgage dataset	11,553
Project postcode	The postal code of the area in which the property is located.	Bank mortgage dataset	11,553
Loan attributes			
Loan amount	Total loan amount in Indian Rupees	Bank mortgage dataset	11,553
Loan to value ratio	Ratio of value of loan to the unit price	Bank mortgage dataset	11,553
Approval date	The date (dd-mm-yyyy) on which the loan is approved by the bank.	Bank mortgage dataset	11,553
Buyer attributes			
Annual income	Buyer's annual income in Indian rupees	Bank mortgage dataset	11,553
Real income	Buyer's annual income in Indian rupees converted from nominal to real terms using the Maharashtra urban consumer price index	Calculated by authors	11,553
Occupation	Buyer's occupation	Bank mortgage dataset	11,553
Gender	Buyer's gender (male/female)	Bank mortgage dataset	11,553
Project level attributes			
Litigation	Whether project is under litigation -yes/no	Maharashtra RERA	2,953
Upper/Lower court	Name of court where a dispute is being heard	Maharashtra RERA	2,953
Amenity	List of amenities self-reported by developers	Maharashtra RERA	2,953
Developer	Name of the real estate developer of the project	Maharashtra RERA	2,953
Units sold	Number of units sold every quarter	Propequity	682
Launched units	Total number of units for sale	Propequity	682

Table A.2: Share of units with litigation pre- and post-reform

	Pre-reform	Post-reform
Share of litigated units (%)	45.2	49.1
Share of units with a case in upper courts (%)	31.8	33.4
Share of units with cases in lower courts (%)	13.4	15.7

Note: The table presents the share of transacted units by litigation status and severity of litigation in the pre- and post- reform period. Units with at least one dispute in the upper courts (Bombay High Court and Supreme Court of India) are classified as facing severe litigation. The second row presents the share of total units facing litigation in the upper courts. Units with all disputes only in the lower courts are classified as facing less severe litigation.

Table A.3: Summary statistics for alternate property prices

	All		Litigated units		Non litigated units	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
All Projects						
Unit price per sq ft. (INR)	13987.4	7012.4	14822.1	6797.7	13157.5	7123.4
Log of price per sq ft	9.4	0.5	9.5	0.4	9.4	0.5
Unit area (sq. ft.)	831.4	407.0	916.3	433.1	747.0	360.0
N	39009		19447		19562	
Non luxury Projects						
Unit price per sq ft. (INR)	13966.0	6659.0	14914.7	6561.3	13174.5	6636.6
Log of price per sq ft	9.4	0.5	9.5	0.4	9.4	0.6
Unit area (sq. ft.)	817.9	389.2	905.0	427.6	745.2	337.1
N	27928		12702		15226	
Luxury Projects						
Unit price per sq ft. (INR)	14068.2	7844.3	14643.7	7217.5	13161.3	8665.1
Log of price per sq ft	9.5	0.4	9.5	0.3	9.4	0.4
Unit area (sq. ft.)	868.2	447.2	938.5	442.4	757.5	432.1
N	11002		6731		4271	

Note: The table presents summary statistics of the price and size data from PropEquity data. We were able to match this to 620 RERA registered projects.

Table A.4: Summary statistics: Type of court

	Mean	Std dev
Upper courts		
Unit price per sq. ft. (INR)	16573	6602
Log of price per sq. ft.	9.65	0.35
Unit area (sq. ft.)	930	570
Buyer's annual income (INR million)	4.37	16.25
Loan to value ratio	0.59	0.20
Obs	3,814	
Lower courts		
Unit price per sq. ft. (INR)	14410	5858
Log of price per sq. ft.	9.52	0.35
Unit area (sq. ft.)	744	450
Buyer's annual income (INR million)	2.48	3.99
Loan to value ratio	0.61	0.21
Obs	1,747	

Note: Data spans years 2015 to 2020. Unit price and buyer's income are in real terms. Loan to value ratio is the ratio of the total loan amount and the value of the unit. Units with at least one dispute in the upper courts (Bombay High Court and Supreme Court of India) are classified as facing severe litigation. Units with all disputes only in the lower courts are classified as facing less severe litigation.

Table A.5: Summary statistics for sales data

Variables	All		Litigated projects		Non lit. projects	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
All projects						
Launched units	108.5	123.6	149.2	129.1	91.1	117.0
Units sold	1.2	3.9	1.7	4.7	1.0	3.5
% of units sold	1.1	3.0	1.2	2.9	1.1	3.1
% of unsold inventory	40.7	16.2	40.1	15.8	40.9	16.4
N	16345.0		4890.0		11455.0	
Non luxury Projects						
	All		**Litigated projects**		**Non lit. projects**	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
Launched units	101.1	115.8	143.6	122.9	84.8	108.5
Units sold	1.1	3.6	1.7	4.7	0.9	3.0
% of units sold	1.1	3.1	1.2	2.9	1.1	3.1
% of unsold inventory	40.3	16.4	39.6	16.0	40.6	16.5
N	13633		3794		9839	
Luxury Projects						
	All		**Litigated projects**		**Non lit. projects**	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
Launched units	148.3	154.2	171.1	147.7	132.5	156.6
Units sold	1.7	5.4	1.9	4.9	1.6	5.7
% of units sold	1.2	3.0	1.2	2.9	1.1	3.0
% of unsold inventory	42.1	15.7	41.6	15.3	42.4	15.9
N	2616		1072		1544	

Note: The table presents sales by quarter for real estate projects for quarters from 2015-2020. Launched units is the number of units in the project. This is not time variant. Units sold is at project and quarter level. % of units sold and unsold inventory (quarterly) as percent of launched units.

Table A.6: Alternative classifications of luxury and non-luxury sub-markets

VARIABLES	(1) Non luxury	(2) Luxury Pool	(3) Non luxury	(4) Luxury Pool & Gym	(5) Non luxury	(6) Luxury Pool or gym
Litigation	-0.014	-0.067*	-0.023	-0.063	-0.012	-0.063*
	(0.037)	(0.038)	(0.037)	(0.046)	(0.037)	(0.036)
Post	-0.019	0.016	-0.020	0.005	-0.006	-0.021
	(0.022)	(0.063)	(0.023)	(0.054)	(0.023)	(0.051)
Litigation x Post	-0.047**	0.002	-0.043**	0.024	-0.049**	-0.016
	(0.020)	(0.041)	(0.022)	(0.040)	(0.021)	(0.044)
Constant	9.437***	9.815***	9.443***	9.760***	9.438***	9.820***
	(0.031)	(0.069)	(0.030)	(0.063)	(0.032)	(0.062)
Observations	9,483	2,028	9,883	1,628	8,643	2,868
R-squared	0.762	0.666	0.756	0.707	0.761	0.729

Note: Standard errors clustered at project level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table shows the results of the introduction of RERA on price of litigated units. The dependent variable is the log of price per square foot in real terms. Post takes value 1 for all time periods after the introduction of RERA. In columns (1)-(2) we classify luxury projects as having a swimming pool. In columns (3)-(4) we classify luxury projects as having both a swimming pool and gymnasium. In columns (5)-(6) we classify luxury projects as having either a swimming pool or gym. All columns report coefficients with project and year-quarter fixed effects, unit controls, and buyer controls.

Table A.7: Years since litigation against project

VARIABLES	(1)	(2)	(3)	(4)
	Dep var: log of price per sq ft			
	Time since litigation (years)			
	1	2 to 5	6 to 10	>10
Litigation	-0.038	-0.024	0.321***	0.228***
	(0.035)	(0.048)	(0.035)	(0.040)
Post	-0.038	-0.025	-0.026	-0.043
	(0.027)	(0.026)	(0.024)	(0.027)
Litigation x Post	0.018	-0.059***	-0.046**	-0.017
	(0.038)	(0.023)	(0.022)	(0.040)
Constant	9.427***	9.435***	9.416***	9.394***
	(0.039)	(0.036)	(0.039)	(0.034)
Observations	6,618	8,012	7,650	7,249
R-squared	0.749	0.762	0.749	0.732

Note: Standard errors clustered at project level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The table shows the results of the introduction of RERA on price of litigated units by differing length of litigation. The dependent variable is the log of price per square foot in real terms. Post takes value 1 for all time periods after the introduction of RERA. All columns reports coefficients with project and year-quarter fixed effects, unit controls, and buyer controls.

Table A.8: Do state RERA websites provide litigation details on projects[44][1]

Yes	No	Information not found
Chhattisgarh[2]	Andaman and Nicobar Islands	Arunachal Pradesh
Dadra and Nagar Haveli & Daman and Diu	Andhra Pradesh[4]	Chandigarh
Kerala	Assam[4]	Jammu and Kashmir
Maharashtra	Bihar	Ladakh
Punjab[3,4]	Delhi	Lakshadweep
Rajasthan	Goa[5]	Manipur
Telangana[4]	Gujarat	Meghalaya
	Haryana[6]	Mizoram
	Himachal	Nagaland
	Jharkhand	Sikkim
	Karnataka[6]	
	Madhya Pradesh	
	Odisha	
	Puducherry	
	Tamil Nadu	
	Tripura	
	Uttar Pradesh	
	Uttarakhand	
	West Bengal	

Note: 'Yes' implies that the state/union territory has a RERA portal and explicitly states if the current project is involved in litigation or not. 'No' means that the state has a RERA portal but does not provide information on litigation. And 'Information not found' implies that the state has either not set up a RERA website, or no list of registered projects can be found. Some union territories are covered by different states' RERA websites- Chandigarh is covered by Punjab's website, Dadra and Nagar Haveli and Daman and Diu is covered by Maharashtra's website, and Andaman and Nicobar Islands is covered by Tamil Nadu's website. Haryana has set up two RERA portals, one for HRERA Panchkula, the other for HRERA Gurugram.

1. As checked on 30th March, 2023.
2. Has a separate section on litigation, but consistently says 'no data found'.
3. Some projects have no litigation details.
4. Gives some litigation details on the promoter and/or their past projects.
5. Gives litigation information on very few projects as a separate attached document.
6. Gives details on only RERA complaints.

Figure A.1: Litigation details published on RERA website

Project

|			
Project Name (Mention as per Sanctioned Plan)	Shri Sai Vishram CHSL	Project Status	On-Going Project
Proposed Date of Completion	31/12/2019	Revised Proposed Date of Completion	31/12/2019
Extended Date of Completion	30/12/2022		
Litigations related to the project ?	Yes	Project Type	Residential
Are there any Promoter(Land Owner/ Investor) (as defined by MahaRERA Order) in the project ?	No		
File/reference no. of planning authority			
Plot Bearing No / CTS no / Survey Number/Final Plot no.	772/A, Survey No.138, Village Eksar, Taluka Borivali, Mumbai 400068	Boundaries East	Gokul CHS Ltd
Boundaries West	60 Feet wide road	Boundaries North	Kandarpada Gaothan
Boundaries South	Sai Niketan CHS Ltd	State/UT	MAHARASHTRA
Division	Konkan	District	Mumbai Suburban

Litigations Details

Project Name	Court Name	Case Number	Case Type	Preventive/Injunction/Interim Order is Passed?	Petition Name	Other Petition Details	Year	Present Status	Documents
Palais Royale	Supreme Court of India	TP 1595	Civil	No	Other	Transfer Petition	2017	Pending	NA
Palais Royale	Bombay High Court	PIL 50	Civil	No	Suit	NA	2017	Pending	NA
Palais Royale	Bombay High Court	Commercial Summary Suit 462	Civil	No	Suit	NA	2017	Pending	NA
Palais Royale	National Company Law Tribunal	1472	Civil	No	Suit	NA	2017	Pending	NA
Palais Royale	National Company Law Tribunal	1519	Civil	No	Suit	NA	2017	Pending	NA
Palais Royale	Bombay High Court	CP/594	Civil	No	Suit	NA	2015	Pending	NA
Palais Royale	Bombay High Court	CP/1066	Civil	No	Suit	NA	2015	Pending	NA
Palais	Bombay High	CP/1039-2015	Civil	Yes	Suit	NA	2015	Pending	◉ View

Figure A.2: Income distribution of buyers in sub-market
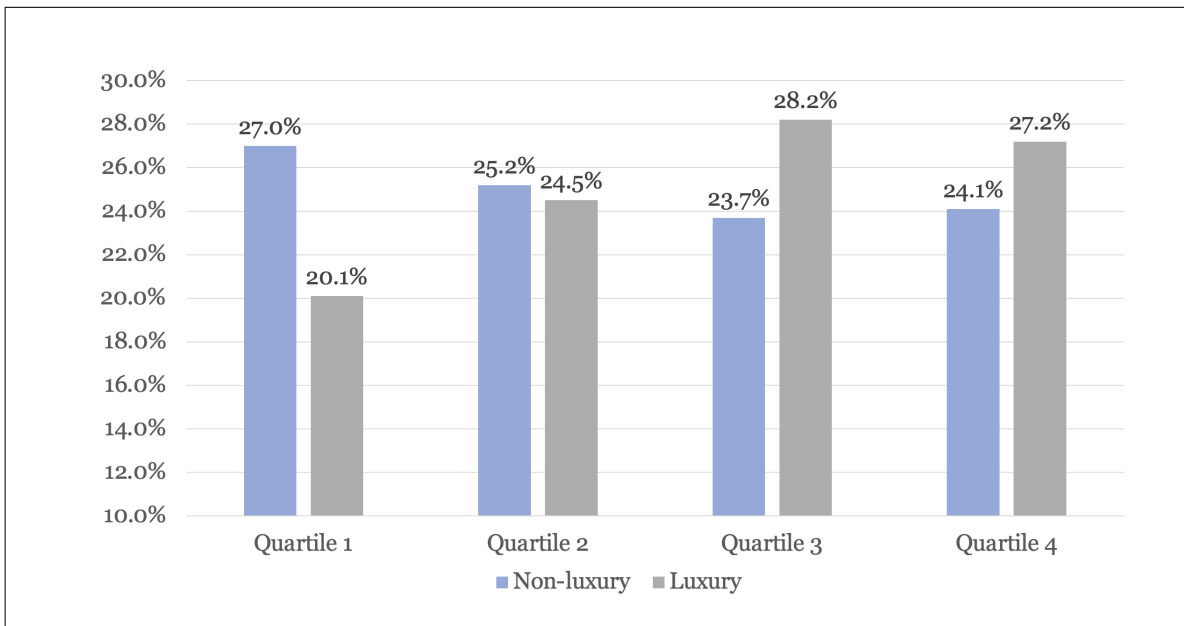
Note: Figure shows the distribution of buyers in non-luxury and luxury sub-market by income quartiles.

Figure A.3: Conditional Mean of log real price per square foot
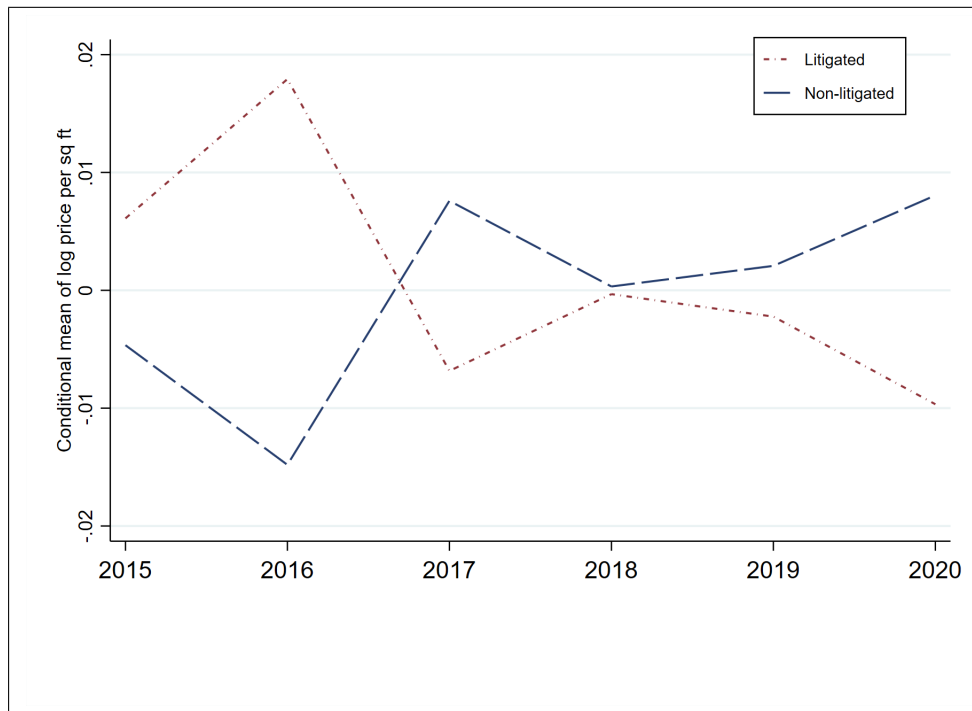
Note: Computed using data on prices of litigated and non-litigated units.

Figure A.4: Sales of units by litigation status of projects

a. Non-luxury projects

b. Luxury projects
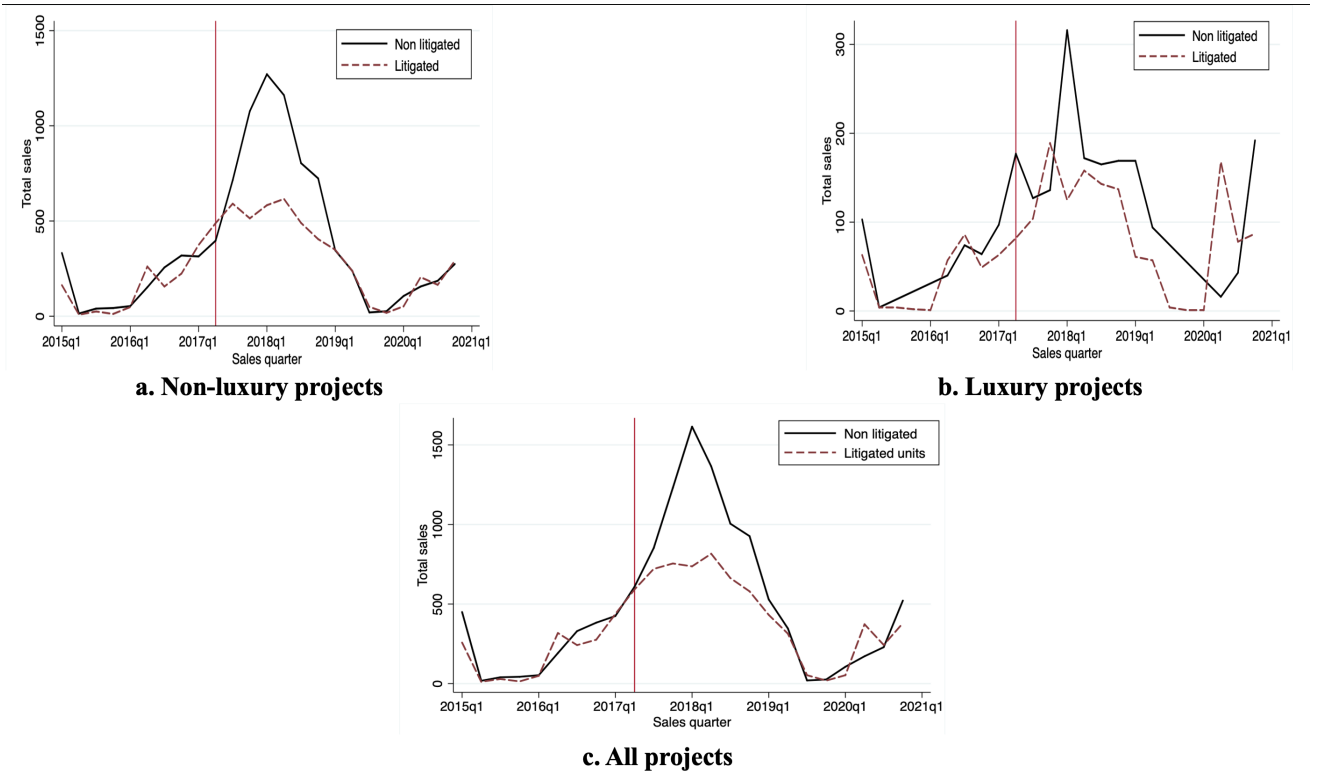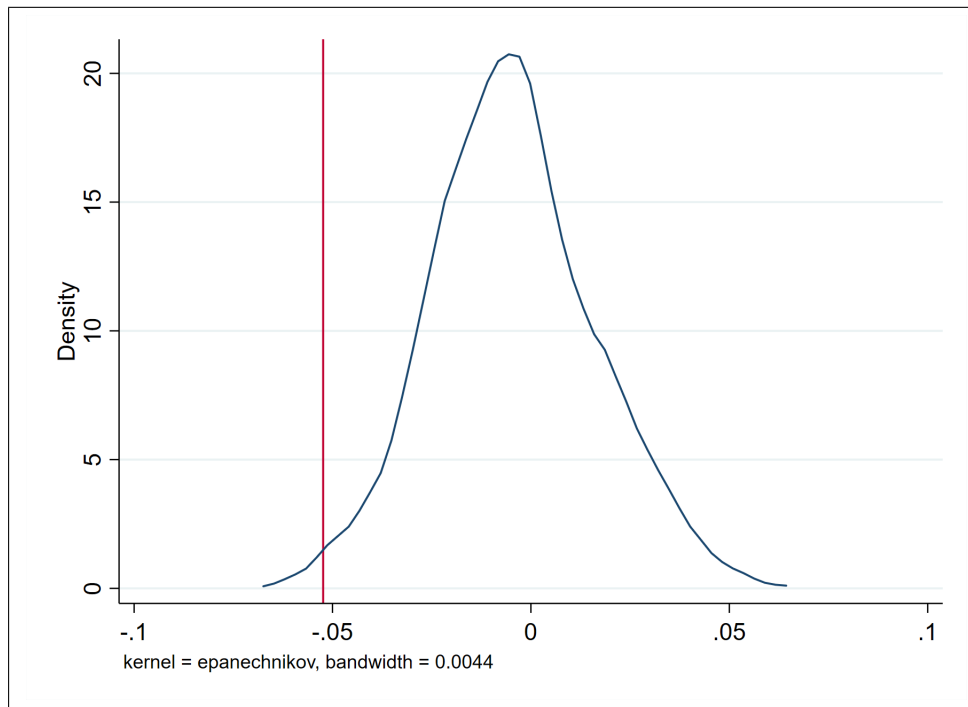

c. All projects

Note: The figure plots mean of quarterly sales of units by litigation status of project. Figure A is for non-luxury projects, B is for luxury projects and C is for all projects.

Figure A.5: Sensitivity to Non-Parallel Trends for Non Luxury and Luxury Projects

a. Non luxury projects   b. Luxury projects
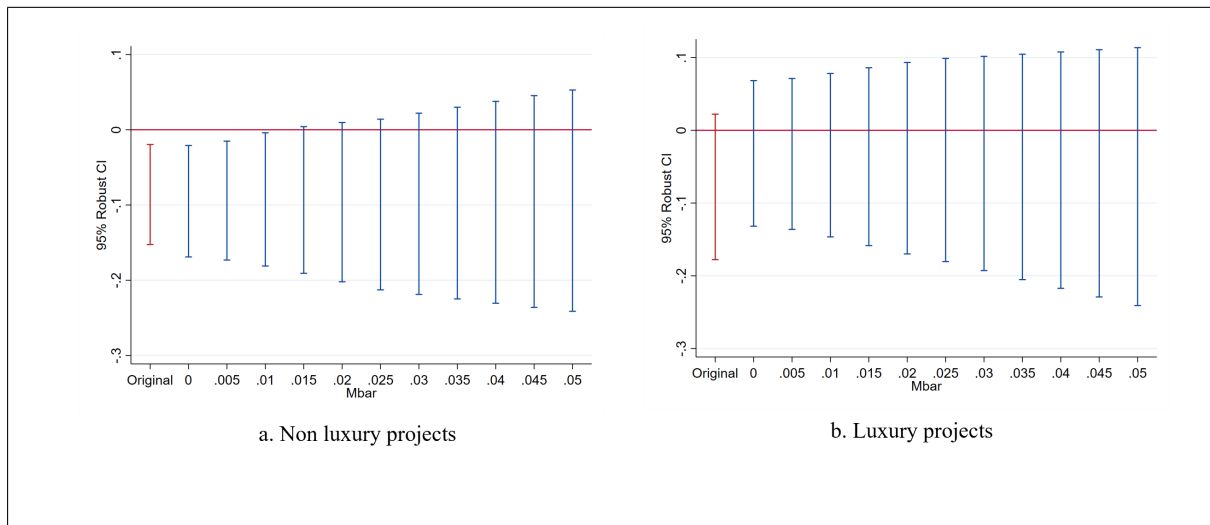
Note: We test the sensitivity of our baseline estimate to the presence of alternate parallel trends assumptions using methods by Rambachan and Roth (2023). We make use of real estate projects that had transactions every year from 2015-2020. The maroon vertical line plots the confidence interval associated with the coefficient estimated in the first post-reform period.

Figure A.6: Permutation distribution

kernel = epanechnikov, bandwidth = 0.0044

Note: The figure shows the distribution of the coefficients obtained in the permutation test, where 1000 permutations of the baseline regression were run with projects being assigned as having litigation at random. The vertical line represents the coefficient estimate from column (2) in table 2.

Figure A.7: Effect of disclosure policy on property prices by income quartiles
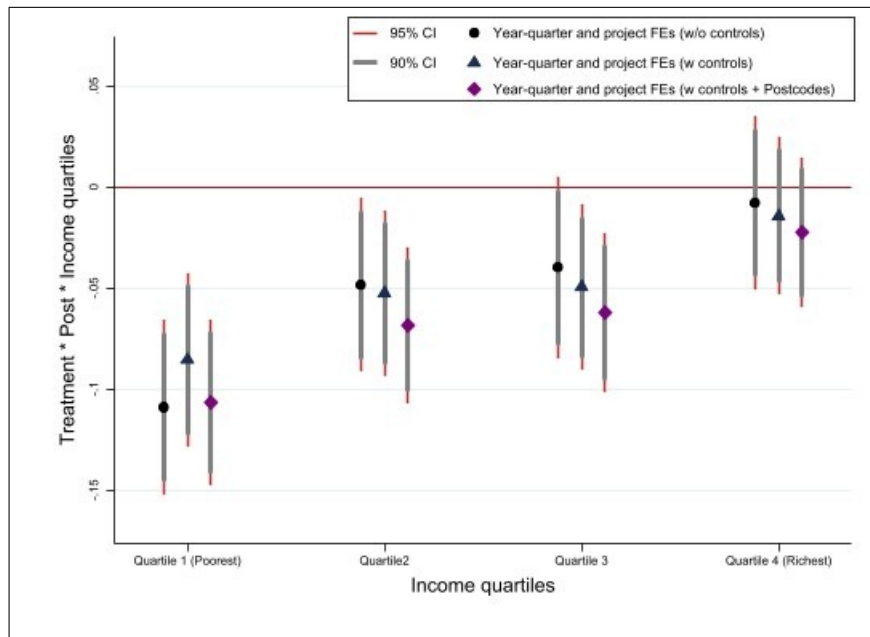
Note: Standard errors clustered at project level in parentheses. The figure shows the results of the introduction of RERA on price of litigated units for each income quartile of the homebuyer. The regression results are reported in table 4. The dependent variable is the log of price per square foot in real terms. We run three specifications. The first specification reports coefficients with real estate project and year-quarter fixed effects and without any controls. The second specification reports coefficients with real estate project and year-quarter fixed effects, unit controls, which are completion status and area in square feet, and buyer controls - which include occupation, gender, and loan to value for the transaction. The third specification reports coefficients with real estate project and year-quarter fixed effects, unit controls, buyer controls and Year x Postcode trend.