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Property Condition Disclosure Law: Does 'Seller Tell All' Matter in Property Values?

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Abstract

At the time when at least two-thirds of the US states have already mandated some form of seller's property condition disclosure statement and there is a movement in this direction nationally, this paper examines the impact of seller's property condition disclosure law on the residential real estate values, the information asymmetry in housing transactions and shift of risk from buyers and brokers to the sellers, and attempts to ascertain the factors that lead to adoption of the disclosur law. The analytical structure employs parametric panel data models, semi-parametric propensity score matching models, and an event study framework using a unique set of economic and institutional attributes for a quarterly panel of 291 US Metropolitan Statistical Areas (MSAs) and 50 US States spanning 21 years from 1984 to 2004. Exploiting the MSA level variation in house prices, the study finds that the average seller may be able to fetch a higher price (about three to four percent) for the house if she furnishes a state-mandated seller's property condition disclosure statement to the buyer.

Journal of Economic Literature Classification: C14, K11, L85, R21

Keywords: Property Condition Disclosure, Housing Price Index, Propensity Score Matching Event Study

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1 Introduction

Home buying arena has changed from the time when 'caveat emptor' or 'buyers beware' was the buzzword. Previously, the onus was placed wholly on the buyer for any defects in the property[3]. There were lawsuits against the real estate agents or the seller in the aftermath of the sales for misrepresentation or non-disclosure of material defects. The case closely resembles that of used car sales. The dealer (or the seller) has better information about the condition of the car (or the property) than the buyer can possibly have. This information asymmetry in property market was brought into public attention by the path-breaking 1984 California appellate court verdict, which made the case for requiring a seller's disclosure statement in residential real estate transactions[4].

This paper analyzes the effect of information transparency and the shift of risk from buyers and brokers to the sellers due to adoption of the law on property values. The analytical structure employs parametric dynamic panel data models, semi-parametric propensity score matching models, and an event study framework using a unique and rich set of economic and institutional attributes for a quarterly panel of 291 US Metropolitan Statistical Areas (MSAs) and 50 US States spanning 21 years from 1984 to 2004 to address the research question. Analyzing the MSA level variation in Office of Federal Housing Enterprise Oversight (OFHEO) Housing Price Indices, we find robust positive effect of the seller's property condition disclosure law on property values.

The study contributes to the literature in the following ways: First, it tests and supports the generally held claim by the brokers and scholars about the positive effect of the mandate on property values. Second, the paper provides a framework and makes the case for empirical

[3] "What is a Seller's Disclosure?" Dian Hymer, October 1, 2001. Distributed by Inman News Features.
[4] Easton v. Strassburger (152 Cal.App.3d 90, 1984) was a California Appellate Court decision that expanded the duty of realtors and the grounds for realtor negligence in selling faulty homes.

analyses for evaluating the policy statutes in the field of law and economics. Third, thirty-six US states have already enacted some form of seller's property condition disclosure law. Finding a positive effect of the law on property values along with the other favorable effects on different aspects of the residential real estate transactions and real estate business environment, the paper bolsters the recommendation of adopting disclosure laws in the states and countries, which are yet to enact such mandates. It provides of course another evidence of disclosure statement in reducing the cost of uncertainty stemming from the presence of asymmetric information.

In the past fifteen years, numerous legal proceedings have brought greater transparency in property transactions. Not all states have seller disclosure as statutory requirements, although there is a movement in this direction nationally. Almost two-thirds of the US states now require sellers to disclose property condition in a state-mandated disclosure form. California was the first state to require a seller disclosure statement, called The Real Estate Transfer Disclosure Statement (TDS). Beginning in the late 1980s and early 1990s other states initiated some form of disclosure statement. The overall format of the statement differs considerably across states. The typical disclosure form asks for information on appliances, fixtures, and structural items etc. Generally, any known material defects (regarding the items) that are not readily apparent to a buyer, but known to the seller, should be disclosed[5]. Determining what is a material defect is not always clear. Sometimes an element of subjectivity is involved. In some states, title and zoning questions appear in the disclosure form. Often natural hazards (e.g. flood or earthquake-prone area) and environmental concerns (e.g. radon, lead, or asbestos exposure) are reflected in particular state-required disclosures. For instance, earthquake hazard disclosure is required in California, but not in New York or in most of the Midwest states.

[5] Lefcoe (2004) provides an excellent discussion on many different aspects of the property condition disclosure law.

Property condition disclosure statement is not a warranty of the unit's condition[6]. It is rather a representation of the information about the property condition by the seller at the time of selling the house. Scholars argue that the seller-provided inspection is not a substitute for the seller disclosure form since many material defects may not be revealed by an inspector[7]. For example, inspectors are not supposed to inspect for rodents, or check the walls, foundation, the air-conditioning, and heating system, or know about flooding, and many other potential areas for material defects.

There have been a number of studies on the property condition disclosure law and its implications on different aspects of residential real estate market. The studies (Pancak, Miceli and Sirmans (1996), Moore and Smolen (2000), Zumpano and Johnson (2003), and Lefcoe (2004)) suggest a positive impact of the law on property values, buyer's satisfaction, broker's avoidance of risk etc. The economic implication of this requirement can be manifold. Most importantly, the seller's disclosure statement directly affects the information asymmetry in real estate transactions. It provides better transparency in property transactions, and facilitates the buyer's decision-making process.

Using data on the claims against errors and omissions insurance by the real estate licensees for five states, Zumpano and Johnson (2003) find that "… fully 76% of all suits against real estate salespeople had something to do with the condition of the property being sold"[8]. The seller's disclosure statement protects both the buyer and the seller from possible disputes in the aftermath of the transaction. It also prevents any misplaced liability on the seller and the broker who represents the seller. Thus, it can be viewed as a tool to avoid lawsuits, which are viewed as

[6] See Lefcoe (2004) pg. 212-213.
[7] See Lefcoe (2004) pg. 239.
[8] Not all states require real estate salesperson to carry Errors & Omissions (E&O) insurance coverage.

deadweight losses to some extent[9]. The disclosure statement shifts risk from the real estate buyers and brokers to the sellers. As noted by Pancak et al. (1996), brokers face a potential liability for failure to disclose by sellers, as well as their own failure to discover defects. Therefore, it makes economic sense to impose the duty of conducting inspection on brokers. However, the cost of this inspection might be incorporated in the brokerage commission. Thus, it may have impacts on the broker's commission structure[10]. It was the interest of the brokers to have a mandate in place on this issue. The National Association of Realtors (NAR), which is a major trade association of real estate agents, lobbied for the disclosure law and brought about the mandate in many states in early 1990s. There is a question about whether seller disclosure should be mandated by statute or not[11]. The most obvious argument for a statute is that it ensures widespread adherence to the mandate. The high rate of compliance is important in achieving the goal of any disclosure statement.

The literature strongly argues that the disclosure law can potentially be one of the factors behind appreciation of property values. Primarily, the positive effect comes from the buyer's satisfaction with the home she is buying. The quality assurance about what a seller is selling from the written disclosure may aid in convincing the buyer to agree on a higher bid price[12]. Based on the interviews of a group of homebuyers before the enactment of the disclosure law in Ohio, and a comparable group after the law adoption, Moore and Smolen (2000) find that the customer dissatisfaction dropped from the pre-disclosure level. In the absence of a disclosure statement (i.e.

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[9] Zumpano and Johnson (2003) conclude: "There seems to be little question that the property condition disclosure, whether mandatory or voluntary, can reduce error and omission claims against real estate licensees".

[10] The average commission for real estate brokers declined from about 6.1 percent in 1991 to about 5.1 percent in 2004. Source: "What you need to know about commission rates", Kelly A. Spors, Sept. 20, 2004; *The Wall Street Journal Online*.

[11] See Lefcoe (2004) pg. 228.

[12] Michael J. Fishman and Kathleen M. Hagerty, (2003), "Mandatory versus Voluntary Disclosure in Markets with Informed and Uninformed Customers", *Journal of Law Economics and Organization*, 19, finds that generally informed consumers pay more for higher quality products.

in the presence of asymmetric information), the rational buyers would discount the bid price due to the uncertainty associated with the property condition[13]. Following Akerlof's theory of the market for 'lemons', in the absence of asymmetric information, the average price for good quality homes would be higher than the price in the presence of asymmetric information, as the cost of uncertainty is partly eliminated (or at least reduced) by the disclosure statement[14]. Moreover, customer satisfaction is all too important from the real estate business point of view. Lefcoe (2004) rightly points out that the brokers do care about customer satisfaction due to the potential referral effect from the satisfied customers. The two major factors that possibly induced interest by realtors in switching from the regime of 'Caveat Emptor' to 'Seller Tell All' are the avoidance of risk and the customer satisfaction.

A secondary positive impact of the disclosure statement is on the quality of houses up for sale. Previously, a seller could strike a deal without fixing some of the less expensive problems with the property. In order to furnish a disclosure statement, and to avoid a possible decline in the bid price for the house, the seller may at least undertake the inexpensive repairs. This may have a positive impact on the property values[15]. However, as Lefcoe (2004) observes, the disclosure law would also prevent sellers to make a house more saleable by painting over or covering up evidence of serious defects.

We can identify two broad areas, which may entail variation in house price indices. First, due to appreciation in values for the properties reported to be in good condition, the house price index should reflect a positive impact of the disclosure law. Second, disclosure may reduce the price index due to the revelation of 'lemons' in the market. This makes the case for an empirically

[13] See Lefcoe (2004) pg. 217.
[14] See "The Market for 'Lemons': Quality Uncertainty and the Market Mechanism", by George A. Akerlof, *The Quarterly Journal of Economics*, Vol. 84, No. 3 (Aug., 1970), 488-500.
[15] Lefcoe (2004) observes: "Understandably, buyers seek price reduction to offset the costs of repairing disclosed defects. By the same token, buyers pay more for homes free of defects."

testable hypothesis: What effects do state-mandated seller's disclosure statements have on residential real estate values? There have been no detailed empirical studies, to our knowledge[16]. Examining the research question helps in our understanding of the law, and indicates whether the objectives of the law are fulfilled, and the mandate should be adopted nation-wide.

Rest of the study proceeds as follows; Section 3 discusses the parametric panel estimation methods and semi-parametric approaches, Section 4 provides the description of the economic and institutional variables, Section 5 analyzes, compares, and contrasts the results from different econometric models, and finally, we conclude in Section 6.

2 Methodology

At the onset of empirical analysis of the disclosure law, we face the choice between treating the adoption of the law as a one-time shock or a persistent shock to the housing market. Since the treatment is a statute, it does not change status every period. This is especially true for the disclosure law, as it has not been repealed in any state since its inception. The effect of the shock stays over the years until it is internalized throughout the economy, which is the case as there are still quite a few states, which do not require such disclosure statement.

Moreover, there is a lag involved in the effect of the law to be felt across the state. This implies that the effect would be less pronounced in the current period of the adoption than in the future periods. The rational buyer would gradually start believing in the effectiveness of the law in bringing about the much-desired transparency in property transactions. The initial skepticism will go away as the buyer updates (reduces) the extent of discounting of the bid price due to the presence of uncertainty. Figure (1) provides a diagrammatic exposition on the slow adjustment

[16] Although Zumpano and Johnson (2003) use empirical facts to analyze the impact of the law on claims against errors and omissions insurance, no empirical modeling was conducted, and the study is limited to only five states.

(dotted line in the figure) in buyers' perception of the effectiveness of the disclosure law. In order to test the length of the slow adjustment empirically, we use specifications with different lengths of duration of the shock.

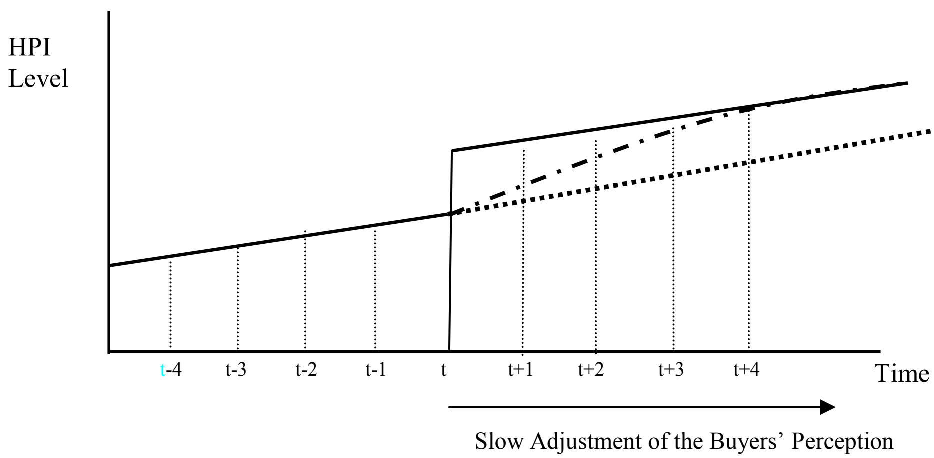

Figure 1 Movement of Housing Price Index at the level

2.1 Parametric Approaches to Ascertain the Effect on Property Values

2.1.1 Simple Panel Estimation:

In this section, we index i as MSAs, j as States, t as quarter-year, s as year, and ω_t as the quarter-year (year) fixed effect. σ_i (σ_j) is the MSA (State) fixed effect. Y_t is the outcome variable (Housing Price Index (HPI)); X_{it} is a vector of economic characteristics of the MSA; Z_{jt} is a vector of economic and institutional characteristics of the state; ε_{it} is the error term. X_{it} includes: an indicator variable for the law adoption, seasonally adjusted unemployment rate, job growth rate, percent change in per capita income, percent change in per capita Gross Metropolitan Product (or Gross State Product for state level analysis), and percent change in population[17]. Z_{jt} includes four indicator variables controlling for the political make-up of the state partisan control

[17] These economic controls are standard in the literature on housing price volatility. See Miller and Peng (2005).

(democratic control with democratic governor, democratic control with republican governor (omitted category), republican control with republican governor, and republican control with democratic governor[18]), number of real estate licensees per one thousand population, number of complaints against real estate licensees, number of disciplinary actions taken against real estate licensees, licensee supervision index[19], and mortgage rate. We include the state-level institutional characteristics to control for the fact that they might be correlated with the unobservables, which affect the housing prices directly. We do not expect these controls to have direct causality with the dependent variable.

$$\left(\frac{Y_{it} - Y_{it-1}}{Y_{it-1}}\right) \equiv y_{it} = \alpha X_{it} + \beta Z_{jt} + \varepsilon_{it} \tag{1}$$

$$y_{it} = \alpha X_{it} + \beta Z_{jt} + \omega_t + \varepsilon_{it} \tag{2}$$

$$y_{it} = \alpha X_{it} + \beta Z_{jt} + \sigma_j + \omega_t + \varepsilon_{it} \tag{3}$$

$$y_{it} = \alpha X_{it} + \beta Z_{jt} + \sigma_i + \omega_t + \varepsilon_{it} \tag{4}$$

Equation (1) is the baseline OLS regression[20]. However, there may be time period specific effects in the variation of HPI. So, In Equation (2), we allow for quarter-year fixed effects. Moreover, variation in HPI may be affected by state-specific factors. Therefore, equation (3) allows for both quarter-year and state fixed effects. Equation (4) allows for quarter-year and MSA fixed effects instead. This specification implicitly contains state effects since we drop the cross-state MSAs.

[18] See de Figueiredo and Vanden Bergh (2004) for detail discussion on these partisan control variables.

[19] The supervision index is defined as the percentage of active brokers to total active licensees. The assumption is that greater supervision can be captured by greater percentage of brokers to licensees. See Pancak and Sirmans (2005) for discussion on this control.

[20] For all parametric estimation, we report clustered standard errors. See Bertrand, Duflo, Mullainathan (2002) and Kezdi (2003) for detail discussion on estimation with robust clustered standard error.

Equations (2) through (4) do not impose any assumption about the serial correlation in error structure. However, in the current context, especially the unobservables related to institutional structure of cross-sectional units may persist over time. This warrants assumptions regarding serially correlated error structure. Therefore, in equation (5), we employ first differencing method instead of previous strategy of mean differencing to control for the cross-section fixed effects[21].

$$\Delta y_{it} = \alpha \Delta X_{it} + \beta \Delta Z_{jt} + \Delta \omega_t + \Delta \varepsilon_{it} \quad (5)$$

We estimate equations (1) through (4) with heteroscedasticity-robust standard error. However, as noted in Slottje, Millimet, and Buchanan (2005), feasible GLS is more efficient than simply using pooled OLS with robust standard errors if the error structure is well specified. Since we are leaving room for specifying the error structure in equation (5), we estimate it by iterative feasible GLS procedure. We try a few different specifications for the error structure. First, we allow a time effect, and specify the variance of the residual to be panel-specific. With this specification, we try three different explicit assumptions for the error structure: no autocorrelation, same AR(1) across panels, and panel-specific AR(1). Next, we specify the variance of the residual to be panel-specific as before, but we allow for both time and MSA effects, and impose similar assumptions about serial autocorrelation as before.

[21] See Woolridge (2002), pg. 284-285 for detail discussion on this. First differenced estimator is more efficient when error term follows a random walk instead of serially uncorrelated error structure.

2.1.2 Dynamic Panel Estimation:

It is a standard practice in the literature on housing price analysis to control for the feedbacks from the past levels of house prices[22]. A competent method is the Generalized Method of Moments (GMM) estimation for dynamic panel data model by Arellano and Bond (1991). As Slottje et al. (2005) argue that instead of allowing for autocorrelation in error structure, the Arellano and Bond GMM estimation explicitly allows past levels of the outcome of interest to affect current levels. First, the model sweeps away the cross-section effect by first differencing, and then uses second and higher order lags of the dependent variable as instruments for the endogenous first lagged dependent variable[23]. In the differenced model, the dependent variable (y_{it}-y_{it-1}) is correlated with (y_{it-1}-y_{it-2}) on the left hand side. However, assuming that we have a long enough time series, we could use lagged differences, (y_{it-2}-y_{it-3}) and higher order lagged differences, or the lagged levels y_{it-2}, y_{it-3}, and higher orders as instruments for (y_{it-1}-y_{it-2}). Arellano et al. and Ahn and Schmidt (1995) propose a GMM estimation suggesting that we can gain efficiency by bringing in more information by using a larger set of moment conditions. In the current context, our dependent variable is the percentage change in HPI. This implies that, to untie the correlations, we need to use further lagged dependent variables as instrument. In similar vein, we employ dynamic panel estimation in the following manner.

$$y_{it} = \sum_{L=1}^{K} \theta_L y_{it-L} + \alpha X_{it} + \beta Z_{jt} + \sigma_i + \omega_t + \varepsilon_{it} \qquad (6)$$

Where 'K' is the lag length. Equation (6) is the baseline dynamic model. In this specification, by the very nature of our dependent variable, y_{it} is correlated with y_{it-1}. By first-differencing equation (6), we obtain the following model.

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[22] Miller and Peng (2005) explain the volatility in house prices in a dynamic framework.
[23] See Greene, William (2003), pg. 307-314, and 551-555 for details on this model.

$$\Delta y_{it} = \sum_{L=1}^{K} \theta_L \Delta y_{it-L} + \alpha \Delta X_{it} + \beta \Delta Z_{jt} + \Delta \omega_t + \Delta \varepsilon_{it} \quad (7)$$

In equation (7), (y_{it}-y_{it-1}) is correlated with (y_{it-1}-y_{it-2}) and (y_{it-2}-y_{it-3}). This implies that, in order to maintain strict exogeneity in choosing instruments for the endogenous terms i.e. (y_{it-1}-y_{it-2}) and (y_{it-2}-y_{it-3}), we could use (y_{it-3}- y_{it-4}) and further lagged differences as instruments. However, the bias may still arise from the first stage OLS regression. In the first stage models, (y_{it-2}-y_{it-3}) is still correlated with (y_{it-3}-y_{it-4}) on the right-hand side. This implies that we need to modify the specifications for the first stage regression accordingly. Therefore, we should use (y_{it-3}-y_{it-4}) and onwards as instruments. This implies that our reduced form specification includes the dynamic terms (y_{it-3}-y_{it-4}) and higher ordered components. Finally, our structural estimation model is written as:

$$\Delta y_{it} = \theta_1 \Delta \hat{y}_{it-1} + \theta_2 \Delta \hat{y}_{it-2} + \alpha \Delta X_{it} + \beta \Delta Z_{jt} + \Delta \omega_t + \Delta \varepsilon_{it} \quad (8)$$

Where $\hat{y}_{it-1}$ and $\hat{y}_{it-2}$ are the predicted values from the first stage estimations. We conduct over-identification tests for the structural models. We compare Akaike Information Criterion (AIC) and Schwartz Bayesian Criterion (SC) to choose the optimal number of lags[24].

2.2 Semi-Parametric Approaches to Ascertain the Effect on Property Values

2.2.1 Propensity Score Matching Estimation

Propensity score matching method developed in Rosenbaum and Rubin (1983) provides a competing approach to analyze the effect of a treatment (in our case, adoption of disclosure law) on an outcome variable (i.e. percentage change in HPI). It is generally used in many areas of

[24] However, SC has superior large sample properties than AIC. In large sample, the SC is asymptotically consistent while the AIC is biased toward selecting an overparameterized model. See Enders (2003), pg. 69-75 for details.

applied statistics, especially in medical trials with patient data. However, in recent years, propensity score method is being increasingly used in program evaluation literature in labor economics (Dehejia and Wahba (1999), Heckman et al. (1997, 1998), and Smith and Todd (2000)). The reasons why we use the propensity score approach to compare and contrast with the parametric estimation methods are three-fold, also noted in Slottje et al. (2005): First, the propensity score approach imposes fewer assumptions about the distribution of the data. Second, it permits non-parametric interactions among all the covariates in determining the outcome (i.e. selection on observables). Third, it ascertains the mean impact of treatment on the treated within a group of 'very similar' units. Parametric approaches consider all the units to infer on the effect.

The motivation of the matching methods can be summarized as follows: In observational studies, the units are assigned to the treatment and control groups in a non-random manner[25]. Therefore, the estimates of the effect of treatment may contain biases from the selection on unobservables. Propensity score matching is based on the idea that the bias is reduced when the comparison of mean impact is performed using treated and control units, which are similar. Propensity score acts as an index on which the matching can be performed since it is generally not feasible to match on an n-dimensional vector of characteristics. A relevant application of the propensity score approach is provided by Slottje et al. (2005), which looks at the effect of logo change on franchise value and ticket sales for NFL teams. More formally, Rosenbaum and Rubin (1983) define the propensity score as the conditional probability of receiving a treatment given a vector of pre-treatment characteristics:

$$P(X_{it}) \equiv \Pr\{legal = 1 \mid X_{it}\} = E\{legal \mid X_{it}\} \tag{9}$$

Where, legal= {0, 1} is the law adoption dummy, and X_{it} is a vector of pre-treatment attributes.

[25] See Becker and Ichino (2002).

Our parameter of interest is the mean effect of treatment (MET) on the treated units, which is defined as[26]:

$$MET_{legal=1} = \frac{1}{n}\sum_{i=1}^{n} y_{1it}(X_i) - \hat{E}(y_{0it} \mid P(X_{it}), legal = 0) \qquad (10)$$

Where, y_{1it} is the percentage change in HPI with disclosure law and y_{0it} is percent change in HPI without disclosure law.

Typically, the matching algorithm (Becker and Ichino (2002)) is conducted as follows: First, we estimate a probit model to obtain the cumulative probability of adopting disclosure law. The predicted cumulative probability from the probit model is the propensity score. Then, we split the sample into five (or more) equally spaced intervals (or bins) of the propensity score. Within each bin, we test that the average propensity score of treated and control units do not differ. If it differs, we split the interval more until the condition is satisfied. Next step is to test that the average characteristics do not differ between treated and control group in each bin. This implies that the balancing property is satisfied. The balancing property ensures that for a given propensity score, exposure to treatment is random, and thus, both groups are on average observationally equivalent i.e. both groups have the same distribution of observables as well as unobservables independently of the treatment status.

Finally, we calculate the difference between average outcomes (i.e. percentage change in HPI) of the treated and the control units for each bin. For any bin 'm',

$$MET_m = \frac{1}{n_m^d}\sum y_{it}^m\Big|_{legal=1} - \frac{1}{n_m^{nd}}\sum y_{it}^m\Big|_{legal=0} \;; \quad m = 1, \ldots, M \qquad (11)$$

[26] See Todd (1999) for a discussion on this and other matching estimators.

Where, 'M' is the total number of bins, n_m^d is the number of units which adopted disclosure law i.e. the treated group, and n_m^{nd} is the number of units which have not adopted disclosure law i.e. the control group in the m$^{th.}$ bin.

The difference can be thought of as the mean impact of adopting the law in each interval. To get the aggregate effect, we add up the weighted abnormal returns to get the Average effect of Treatment on Treated (ATT). We apply interval weights based on the number of observations in the interval when adding up the mean effects.

$$ATT = \sum_{m=1}^{M} \left[\frac{n_m}{N} MET_m \right] \tag{12}$$

Where, $n_m = n_m^d + n_m^{nd}$ = number of units in the m$^{th.}$ bin.

We try three different matching methods (following Becker and Ichino (2002)) to see the robustness of the treatment effects. First, the Stratification method executes the algorithm we described before. We discard the bins where we do not find either any control or treated units. Second, the Nearest Neighbor method involves taking each treated unit and finding the control units, which is closest in terms of magnitude of the propensity score. Therefore, by construction, each treated unit should have matches, which enables us to avoid the pitfall of discarding some bins in stratification method. However, some matches would be poor in quality. Third, the Kernel Matching method gets all treated units being matched with a weighted average of all control units where weights are calculated as inverse of the Euclidean distance between the propensity scores of the two groups. Therefore, it tackles the problem of poor matches from the nearest neighbor method. Each of these methods has some advantages and disadvantages. We compare the results from all three methods.

We also try three different estimators for each of the above methods. First, we define the outcome variable to be the difference in average percentage change in HPI between treated and control units. Second, in order to control for the year-specific effect, we subtract year mean from the first estimator. Third, in line with Smith and Todd (2000), we define the outcome variable as a Difference-in-Difference (DID) in average percentage change in HPI between the two groups[27]. It is DID in the sense that we subtract a benchmark percentage change in HPI from the average percentage change in HPI. We choose the benchmark to be the percentage change in HPI from the pre-treatment time period. This strategy enables us to control for the cross section effect. We also de-mean the year effect for this estimator.

2.2.2 Event Study Analysis

In the propensity score matching estimation, the control unit may come from any of the periods in the sample. However, it may be desirable to find a matched control from the disclosure year or from the vicinity of that time period. To address this concern, we restrict the control unit to be obtained within one year of the law adoption. This is done in an event study approach where we calculate the abnormal return (AR) for each of the time periods in the event window (which we specify to be 4 years or 16 quarters before and after the event i.e. law adoption). One important advantage of the event study framework is that it allows us to focus on the disclosure law adoption date (i.e. the event date) to infer on the impact. Typically, the event study methodology is extensively used in analyzing the impact of earnings announcements (or, other information shocks) on the stock prices[28]. There are also quite a few studies, which address the issues related to corporate laws in this framework[29].

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[27] Smith and Todd (2000) requires the matches to come from the same labor market while evaluating employment programs.

[28] See Campbell, Lo, and McKinley (1997), chapter 4 for an excellent discussion on the methodology.

[29] See Bhagat and Romano (2001) for excellent discussions on the issues and methodologies.

Generally, the ARs are obtained as the deviation of the treatment unit's outcome from a market index or a benchmark at each event dates. AR is the sample average abnormal return for the specified date in event time. In the current context, the ARs are obtained as the deviation of the treatment unit's HPI growth rate from the control unit's HPI growth rate at each event dates, which are lined up as different states adopted the law at different dates. The control units are obtained by matching on the estimated propensity scores[30]. We apply the restriction of obtaining matches within one year of the event date. Cumulative abnormal return (CAR) is calculated as the sample average cumulative abnormal return for quarter -16 to the specified quarter.

2.3 A Model of Law Adoption

In many studies of analysis of statutes, the statute is generally assumed exogenous. However, one might argue that many different legal, economic as well as special interest group activities gradually give rise to a situation when government enacts a law after much deliberation on the subject. Following Kiefer (1988), and de Figueiredo and Vanden Bergh (2004) we formulate a proportional hazard model in discrete time framework to ascertain what factors are instrumental in adopting a property condition disclosure law.

$$\lambda(t) = \lambda_0(t)\exp(x'\beta) \tag{13}$$

In the model in equation (13), first part is a function of duration time, called the baseline hazard, and the second part is a function of explanatory variables other than time. The time is separated from the explanatory variables so that the hazard is obtained by shifting the baseline hazard as the explanatory variables change (i.e. for all the cross section units the hazard is proportional to the baseline hazard function). One popular form of the model is the logit estimation where each unit contributes several terms to a logit likelihood function, one term for each period for which the

[30] See Appendix A for detail exposition on the event study procedure employed in this paper.

unit was at risk of leaving the treatment stage[31]. The baseline hazard can be specified by allowing the intercept to be different for logit formulations of each time-period (i.e. by including a dummy variable for each representative period) or by including a function of time. We assume that once a law is adopted, it will remain; and eliminate the observations after the disclosure law has been adopted. This censoring of the data is reasonable given that no state has ever repealed property condition disclosure law. The hazard function can be represented by a standard normal cumulative distribution function. Therefore, we could estimate the model after conditioning on the event not yet having occurred using a standard logit specification.

3 Data Description

The study uses information on economic variables and institutional variables for 291 MSAs in 50 US States from 1984 to 2004. For MSA level analysis, we utilize the quarterly information i.e. 24,444 observations. The state level analysis is based on yearly information i.e. 1,050 observations. Office of Management and Budget (OMB) has changed the definition of MSAs a few times during the study period. Since, OFHEO uses 2003 MSA definition to compute the housing price index; we use 2003 MSA definition for our analysis. Since, our treatment variable is the adoption of disclosure law, which is state-mandated, we discard the MSAs, which cross the state boundaries, and we discard the consolidated MSAs.

To our knowledge, 36 states have already mandated some form of disclosure statement. We obtained the effective dates of the mandate from official statements for different states[32]. To estimate the housing price changes, we use the repeat sales quarterly Housing Price Index (HPI), reported by the OFHEO. We use quarterly percentage change in HPI in MSA level analysis. For yearly analysis, we take the average quarterly rate of change for the year. This is the case with the

[31] See Kennedy (1998), pg. 259-261 for a simple discussion on this structure.
[32] Pancak et al. (1996) lists the states, which adopted the disclosure law until 1996.

propensity score matching analyses. One important advantage of the time period is that on average, we can observe the treated units sufficiently before and after the adoption of the disclosure law for most of the states. In our sample, California, being the first state, adopted the law in 1987, while the majority of other 35 states adopted the law in 1990s.

3.1 Economic Variables:

We use labor market characteristics like the seasonally adjusted unemployment rate and the job growth rate, which are provided by the Bureau of Labor Statistics (BLS). In order to comply with 2003 MSA definition, we use county labor market information to aggregate up to the MSA level. Other economic variables include percentage change in per capita income, percentage change in per capita Gross Metropolitan Product (GMP) and Gross State Product (GSP), single-family 30-year average mortgage rate for states, and population growth rate. Broadly these variables characterize the economic make-up of the state or the MSA. Data on these controls are obtained from the Bureau of Economic Analysis (BEA) except for GMP. GMP data is not publicly available. We compute MSA share of GSP to use it as a proxy for GMP[33]. United States Conference of Mayors and the National Association of Counties publish GMP data from 1997. Comparing with the United States Conference of Mayors and the National Association of Counties' GMP data, we find that our proxy is close to the official estimates. Moreover, we are interested in the variation in per capita GMP. Economic variables except labor market controls are available on a yearly basis. We interpolate these variables to the quarterly level[34].

[33] Proxy GMP=GSP*(MSA population/State population).

[34] Since linear interpolation takes two yearly values and fits a straight line while projecting the data in between, it is generally less accurate than other polynomial based methods. So, we apply a cubic spline interpolation method, which uses the data point value along with the first and the second derivatives at each surrounding point to interpolate. When we compare the results with interpolated quarterly data with the actual yearly data, the qualitative results do not differ.

3.2 Institutional Variables:

Numerous lawsuits against the real estate licensees made the case for adoption of disclosure laws. Potentially the legal activities are governed by the institutional characteristics of the state. Statistics from the *Digest of Real Estate Licensing Laws and Current Issues* (reports from 1985 to 2005) compiled by the Association of Real Estate Licensing Law Officials (ARELLO) provide a rich set of characteristics that are closely associated with the institutional backdrop of the disclosure law. For example, the number of complaints against real estate licensees indicates the broad dissatisfaction about the licensee service. Similarly, the number of disciplinary actions taken against the licensees provides information about how the monitoring authority performs its duty[35]. Other institutional controls include number of active brokers, associate brokers, and salespersons in each state and the broker supervision. It was the concerted movement and lobbying on the part of realtor's association, which brought the law in most states. To have a sense of how organized the real estate agents are in different states, we include the number of active brokers, associate brokers, and salespersons in each state in our analysis. Ideally, the percentage of licensees who are associated with some trade organizations like NAR could serve as an excellent indicator of the lobbying effort. However, it is hard to obtain this information across the states for a long time series that we are considering in this study. We also include a measure of the extent of broker supervision in our analysis. Pancak and Sirmans (2005) expect that "greater supervision would prevent intentional and unintentional wrong doing on the part of salespersons, and therefore decrease findings of misconduct". These variables broadly characterize the institutional make-up of the real estate market. We also include a control for

[35] When disciplinary actions figure is missing or zero, we take the average of the figures within 1-year range. When total disciplinary actions figure is missing in ARELLO reports, if available, we take the sum of the figures under different categories of disciplinary actions, or, we take the sum of the actions by consent and number of formal hearing as total number of disciplinary actions (this is the case until 1986). Then we take sum of disciplinary action and formal hearing from column of complaints resulting in some actions. Both of these are expected to provide the number of complaints having enough substance to attract legal attention. This is typically the case with Arizona and Hawaii for 1984 to 1986.

partisan control in the state legislation. Following de Figueiredo and Vanden Bergh (2004), we include an indicator variable for democratic and republican control. In order to fully exploit the information on political make-up of the state general assembly, we use detail partisan control variables rather than a simple blue/red category. Above all else, the political process decides on enacting a regulation. We use democratic control with republican governor as the omitted category. The information on partisan control for each general election cycle is obtained from National Conference of State Legislatures (NCSL).

Table (1) reports the summary statistics of the above controls for the treated and the control units. Few important observations can be made from the summary statistics of the two groups. Both at the MSA level as well as the state level, average percentage change in HPI is slightly higher (1.13 percent against 1.01 percent for MSAs, and 1.24 percent against 1 percent for States) for the treated group than for the control group. Unemployment rate and other economic controls are generally, on average, higher for the control units. Remarkably, average number of disciplinary actions (about 110 against 43) and average number of complaints (about 869 against 793) are higher for the states, which adopted disclosure law. Generally, a higher number of disciplinary actions and complaints against the licensees suggest that these controls are important in capturing the dissatisfaction of the consumers, and also due to high volume of complaints, regulators might be inclined to a state-mandated disclosure requirement. On average, control units tend to have greater broker supervision (50 percent against 48 percent) than the treated units. This supports the hypothesis that greater broker supervision ensures less mistakes and greater awareness of the market practices among salespersons, which, in turn, tend to reduce the dissatisfaction among the homeowners. The disclosure states tend to have higher number of active licensees. Interestingly, the treated states are more likely to be under republican control than under democratic control.

4 Empirical Results

We discussed the slow adjustment process of the legal shock in the methodology section. To get a sense of how 'slow' is the adjustment process we use equation (4) i.e. the regression model that allows for MSA and time effects, and specify the length of legal dummy to be single quarter, four quarters, eight quarters and, up to thirty-six quarters or nine years. Since different states adopted the law in different times, there are a different number of states with disclosure law associated with different lengths of adjustment. Therefore, we try two ways to test the robustness of the outcome. First, we keep the sample size same for all the lengths. Next, we adjust the sample size as we increase the length. In Figure (2), we plot the estimates on legal dummy variable from different specifications in terms of lengths of law adjustment.

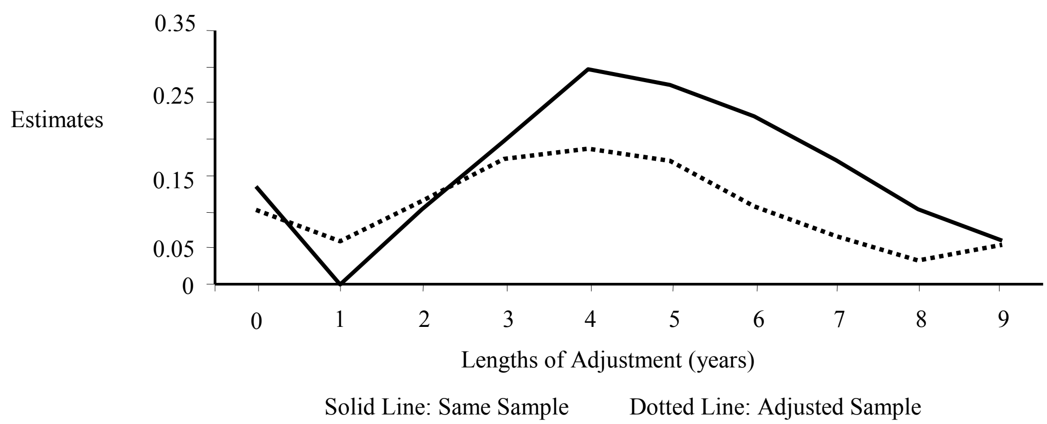

Figure 2 Plot of the Estimates

The analysis reveals significant effects when we assume long-term persistence in the shock. The effect is most pronounced in 4 to 6 years of windows. This is quite consistent with the theoretical hypothesis in Figure (1). Figure (2) also reveals that the estimate is almost zero when we specify the length as 8 to 9 years. However, to get the actual effect size, we need to multiply the estimates with the corresponding number of quarters that we specify as the lengths of adjustment.

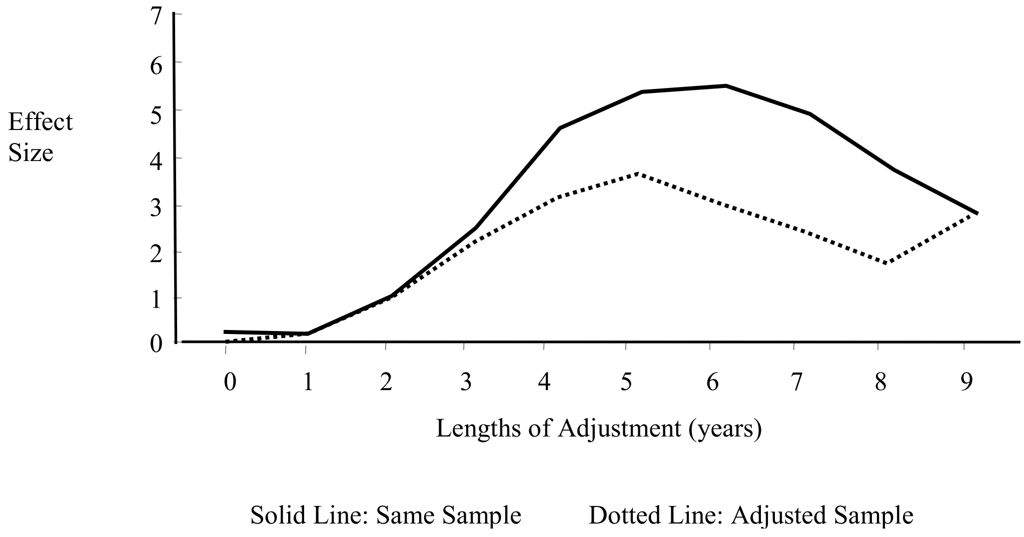

Figure 3 Plot of the Actual Effect Sizes

For example, in figure (2), the effect is about 0.182 per quarter for the model with 4 years (or 16 quarters) of length of persistence where we use the adjusted sample. Therefore the actual effect is 2.91 (= 0.182*16). We plot the actual effect size in figure (3). Figure (3) reveals that the effect size decreases gradually and is not zero in 8 to 9 years of adjustment lengths. It suggests that the effect of the law on property values is generally spread over about four to six years. Therefore, we argue for treating adoption of the law as a shock to the housing market that stays for four years on average, and take it forward for rest of the analyses.

4.1 Parametric Results

Results for equations (1) through (5) are reported in Table (2) for MSA level analysis. Table (2) reports the simple panel data estimation results. Column (1) reports the OLS estimates while Columns (2), (3), (4), and (5) report the estimates after controlling for the time fixed effect, both time and state fixed effects, both time and MSA fixed effects, and first-differenced model respectively. After allowing for MSA effects, coefficient on the indicator variable for adoption of law suggests a positive effect (ranging from 1.7 to 4.6 percent on the HPI growth rate) on property values.

In figure (2), the effect is about 0.182 per quarter for the model with 4 years (or 16 quarters) of length of persistence, where we use the adjusted sample. Therefore the actual effect is 2.91 (= 0.182*16). We plot the actual effect size in figure (3). In table (2) column (4), we find the same effect as we use the 4 year time period for effect of the law.

Comparing across the columns (3), (4), and (5) reveals positive impact of the disclosure law on house prices. As expected, compared to the mean-differencing approach, first-differencing reports substantially lower adjusted R-squared. The joint significance tests for time and cross-section effects suggest the importance of controlling for these effects. The estimates on economic and institutional variables are moderately robust across the specifications except in the first-differencing method. Complete democratic control seems to have positive effect on the property values. The main finding from Table (2) is the positive impact of adopting the disclosure law on property values at the MSA level.

Results from feasible GLS procedure are reported in Table (3). As discussed in Section 2, feasible GLS procedure provides improvement (in terms of efficiency gain) over pooled regressions in Table (2) when we specify the error structure. Table (3) reveals robust positive impact of adoption of law on house prices. The overall magnitude of the estimates (effect size ranging from 2.6 to 4 percent) is similar to the effect size in Table (2). The rationale behind explicit assumptions about serial correlation is established in Table (3).

Table (4) reports the dynamic panel estimation results. Results for four different structural equations are reported. The first model uses third through seventh lagged dependent variables as instruments for first lagged HPI. Column (2) reports the structural model with estimates using fifth through ninth lagged dependent variables as instruments for first lagged HPI. Columns (3) and (4) report the similar structural models but with the further lags (seventh through eleventh and ninth through thirteenth) as instruments. We estimated all the models including lagged labor force variables and find that they do not affect the quantitative and qualitative results. Therefore, we report the results without the lagged labor force variables.

For the instrumental variable approach of Table (4) to eliminate the bias from an endogenous dependent variable, we need to choose proper instruments. By construction, there are correlations between the dependent variable, and first as well as second order lagged dependent variables in the reduced form equation. However, there may still be correlation between the dependent variable and third (or higher) order lagged dependent variable due to possible persistence in the house price generating process. Therefore, we use only longer lags to provide consistent estimates in the presence of persistence in the data. Due to better large sample properties, we use Schwartz Bayesian Criterion (SC) to choose the optimal number of lags. SC suggests using five lags in the reduced form equations. The broad qualitative results still hold in the dynamic framework. Our variable of interest – adoption of law – is positive, although not statistically significant, across different sets of instruments, and the effect size is smaller than the range of magnitude that we find in Tables (2) and (3). Except for column (2), the over-identification tests reject the validity of the instruments. Therefore, we avoid inferring the results from this analysis. Probably, a better model specification may bring about the effect and significance that is similar to the estimates in Tables (2) and (3.3).

4.2 Semi-Parametric Results

Table (5) reveals the results from semi-parametric propensity score matching analysis. It presents three different estimation strategies with three different estimators for the outcome variable for one model specification (as in Table (5) column (4)) for estimating the propensity scores. This analysis is done with yearly data at the MSA level– i.e. information about 291 MSAs for 21 years[36]. As discussed in Section (2), each of these estimation strategies provides some quantitative and qualitative gains over each other. We try to see the robustness of the effects across these methods. For each of the methods, we look at the effects with three different estimators: first, a simple average difference in percentage change in HPI that does not control for cross-section and time effects; second, an average difference in percentage change in HPI after controlling for the year effect; and third, we pull out the cross-section effect by subtracting the percentage change in HPI from a benchmark year from the second estimator. In general, table (6) reveals strongly positive and fairly robust impact of property condition disclosure law on house prices across different methods, and model assumptions. As we discussed before, *Kernel* matching estimator provides some useful advantages over other two methods. Column (3) from *Kernel* matching method reveals about 0.21 per quarter or 3.36 percent (which is, 0.21*16= 3.36, for 16 quarters) significant and positive effect on the HPI growth rate.

Table (6) reports the results from an event study analysis at the MSA level. We calculate the cumulative abnormal returns for 33 quarters i.e. 16 quarters before and after the event date. The analysis suggests about 2.6 percent increase in house prices due to adoption of the property condition disclosure law. On average, the event date abnormal return is positive. Almost 50 percent of the abnormal return estimates are positive on the event date and on other dates in the event window. The percentage of positive abnormal returns is slightly higher in the post-event time periods than in the pre-event dates. The plot of CARs in Figure (4) reveals that the effect of

[36] While conducting the yearly analysis, we test alternative specifications for the timing of the law adoption. Since we know the effective day of the mandate, we could assign the corresponding year as the adoption year. However, one could argue that if the effective date falls in last two quarters of the year, bulk of home sales has already taken place. So, the effectiveness of the mandate really starts from next year. We tried both the specifications. The qualitative and quantitative results are robust to this concern.

the law increases gradually in the event window and supports the hypothesis that the initial skepticism about the effectiveness of the law gradually goes away and the buyers offer higher bid prices for the houses disclosed to be in good condition.

4.3 Robustness and Comparisons

The important finding of robust positive impact of the law on house prices in Tables (5) and (6) warrants comparison with the parametric results. Comparing with columns (2) and (3) in Table (2) and columns (3) and (6) in Table (3), the effect size is larger from the semi-parametric analysis (about 3.3 percent compared to 2.7-2.9 percent). We get about 2.6 percent effect size from the event study analysis. A pertinent question is which approach we should prefer. As pointed out in Slottje et al. (2005), matching estimators come with a few advantages. First, there are fewer assumptions about the distribution of the data. Second, it allows for non-parametric interactions among all the covariates in determining the outcome (i.e. selection on observables). Third, it compares within a group of 'very similar' units. Parametric approaches consider all the units to infer on the effect. This is also true for the event study approach. Moreover, the event study approach allows us to focus on the event date effect. The usual critique of the matching estimation technique regarding smaller sample sizes is not pronounced in the current context as we have many observations for both the treated and the control units. This suggests that we should prefer the semi-parametric estimators for the purpose of inference.

4.4 Factors Explaining Law Adoption

Table (7) reports results from four different model specifications for the proportional hazard model of disclosure law adoption. This table is identical to Table (2), which is our baseline framework in Chapter (2). The analysis is done with the state-level data (1,050 observations). We use the pre-disclosure average number of disciplinary actions taken against the licensees, licensee supervision index, and number of licensees as controls for pre-treatment characteristics[37]. Essentially, we assume that these institutional characteristics are exclusive to the housing market. We still use the economic variables as time-varying attributes since they are not directly associated with the institutional environment of the housing market. The columns are distinguished by the inclusion of lagged percentage change in HPI. It seems that inclusion of the second lagged percentage change in HPI matters in this set-up. We also allow the intercepts to differ across the census divisions. Most importantly, as hypothesized, average number of disciplinary actions seems to determine whether the state would adopt the law. Greater number of disciplinary actions conveys a signal in favor of a state mandate (robust significant positive impact across the columns). The greater the degree of broker supervision, the lower is the state's likelihood of adopting the law (robust significant negative impact across the columns). This is in line with the postulate that greater broker supervision, by ensuring less mistakes and greater awareness of the market practices among salespersons, tends to reduce dissatisfaction among the homeowners, which, in turn, lowers the number of lawsuits that signals the movement towards adopting the law. Interestingly, as observed in Table (1), republican control tends to favor (although not statistically significant) the adoption of property condition disclosure law, that promote transparency in housing transactions.

5 Conclusion

The study examines the impacts of seller's property condition disclosure mandate on the residential real estate values. We analyze the effect of information transparency and the shift of risk from buyers and brokers to the sellers due to adoption of the law on property values. The analytical structure employs parametric dynamic panel data models, semi-parametric propensity score matching models, and an event study framework using a rich set of economic and

[37] Due to missing information, we use earliest available data for Indiana, Montana, and New York. However, we still use information from pre-disclosure period of these states.

institutional variables for a quarterly panel of 291 US Metropolitan Statistical Areas (MSAs) and a yearly panel of 50 US States spanning 21 years from 1984 to 2004 to address the research question.

Analyzing the MSA level variation in Housing Price Indices, we find positive effect (about three to four percent) of the seller's property condition disclosure law on property values, and the effect is spread over about four years. We suggest using semi-parametric approaches due to absence of any *a priori* distributional assumption, and comparison based on similar units. The results suggest that the average seller may be able to fetch a higher price for the house if she furnishes a state-mandated seller's property condition disclosure statement to the buyer. The state-mandated disclosure requirement ensures widespread compliance. The plausible reasons behind this premium could be the buyer's greater confidence in the quality of the house she is acquiring, and the higher quality of the houses up for sale. The Property Condition Disclosure Law brings about the much-desired transparency in housing transactions, which increases the prospective homeowners' confidence. The finding is consistent with the generally held postulate by real estate agents and scholars about the favorable impact of the law on average house prices.

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Table 1 Summary Statistics

	Disclosure Mandate			No Disclosure Mandate		
Variable	**N**	**Mean**	**Std. Dev.**	**N**	**Mean**	**Std. Dev.**
291 Metropolitan Statistical Areas Characteristics: 1984Q1—2004Q4: 24,444 Observations						
%Change in HPI[38]	17,189	1.127	2.186	4,615	1.012	2.046
%Unemployment Rate	19,068	8.660	9.227	5,376	10.254	15.976
%Job Growth Rate	19,068	0.443	4.081	5,376	0.556	2.352
%Per Capita Income Change	19,068	5.619	3.103	5,376	6.207	2.943
%Per Capita GMP Growth Rate	19,068	1.142	0.741	5,376	1.128	0.657
%Population Growth Rate	19,068	0.292	0.387	5,376	0.373	0.412
50 States Characteristics: 1984—2004: 1,050 Observations						
%Change in HPI	735	1.243	1.329	315	1.008	0.789
%Unemployment Rate	735	5.514	1.685	315	5.628	1.879
%Job Growth Rate	735	1.513	1.895	315	1.682	1.976
%Per Capita Income Change	735	1.392	0.647	315	1.417	0.777
%Per Capita GSP Growth Rate	735	4.884	3.432	315	4.707	3.147
%Population Growth Rate	735	1.042	1.084	315	1.012	1.210
%Mortgage Rate	735	8.432	1.788	315	8.433	1.763
Number of Real Estate Licensees/1000 population	735	6.479	3.735	315	5.199	2.430
No. of Complaints	735	868.650	1482.715	315	793.365	2671.183
No. of Disciplinary Actions	735	109.686	261.837	315	42.768	53.779
Licensee Supervision Index	735	47.785	26.494	315	50.191	24.878
Democratic Control Democratic Governor	735	0.214	0.410	315	0.270	0.444
Democratic Control Republican Governor	735	0.223	0.417	315	0.209	0.407
Republican Control Republican Governor	735	0.284	0.451	315	0.269	0.444
Republican Control Democratic Governor	735	0.246	0.431	315	0.238	0.426

[38] The number of observations differs for HPI due to missing information for some MSAs in early years.

Table 2 Parametric: OLS and Fixed Effect Analysis: MSA

(Dependent Variable: Percent Change in HPI from previous quarter)

Regressors	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Law Adoption	0.107** (0.054)	0.288* (0.057)	0.210* (0.058)	0.182* (0.055)	0.144 (0.109)
Mortgage Rate	-0.130* (0.016)	0.353* (0.115)	0.768* (0.133)	0.771* (0.134)	0.709** (0.346)
%Unemployment	-0.006* (0.002)	-0.009* (0.003)	-0.006** (0.002)	-0.003 (0.002)	-0.006 (0.011)
%Job Growth	0.004 (0.006)	0.003 (0.006)	0.004 (0.006)	0.003 (0.006)	0.002 (0.004)
%Per Capita Income Change	0.062* (0.008)	0.072* (0.011)	0.076* (0.011)	0.078* (0.011)	0.026 (0.017)
%Per Capita GMP Growth Rate	0.104* (0.038)	0.115* (0.039)	0.081** (0.040)	0.079** (0.039)	-0.094** (0.039)
%Population Growth Rate	0.573* (0.085)	0.624* (0.096)	0.798* (0.093)	1.369* (0.114)	0.835* (0.303)
Democratic Control Democratic Governor	-0.016 (0.062)	0.050 (0.064)	0.230* (0.071)	0.210* (0.070)	-0.305 (0.176)
Republican Control Republican Governor	-0.075 (0.062)	-0.034 (0.055)	0.077 (0.063)	0.094 (0.061)	0.044 (0.184)
Democratic Control Republican Governor	-0.184* (0.056)	-0.081 (0.052)	-0.003 (0.074)	-0.012 (0.073)	-0.201 (0.217)
Number of Real Estate Licensees/1000 population	0.007 (0.007)	0.011 (0.008)	-0.042** (0.016)	-0.044** (0.017)	0.064*** (0.033)
% Disciplinary Action taken / number of complaints	0.004* (0.001)	0.002 (0.001)	-0.0003 (0.001)	-0.0001 (0.001)	0.003 (0.003)
Licensee Supervision Index	0.001 (0.001)	-0.008* (0.001)	-0.011* (0.003)	-0.011* (0.002)	-0.003 (0.008)
Fixed Effects?	None	Time Mean Difference	Time State Mean Difference	Time MSA Mean Diff	Time MSA First Difference
Joint Significance of Time Effects		F (83, 290) =35.48 (Pr= 0.00)	F (83, 290) = 34.34 (Pr= 0.00)	F (83, 290) = 33.68 (Pr= 0.00)	F (82, 290) = 32.79 (Pr= 0.00)
Joint Significance of Cross-Section Effects			F (48, 290) = 21.58 (Pr= 0.00)	F (60, 290) =1.2e+05 (Pr= 0.00)	
Adj. R^2	0.035	0.109	0.129	0.144	0.002
N	19,577	19,577	19,577	19,577	19,067

NOTES: Clustered (on MSAs) standard errors are reported within parentheses. '*', '**', and '***' imply 1 percent, 5 percent and 10 percent significance level. We include the state-level institutional controls in these regressions due to the possibility that although they are not directly associated with the house prices, they may be correlated with the unobservables directly associated with the house prices. Including fixed effects may not be able to fully mitigate the bias.

Table 3 Parametric: Feasible GLS Procedure: MSA

(Dependent Variable: Percent Change in HPI from previous quarter)

Regressors	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Law Adoption	0.265* (0.034)	0.251* (0.031)	0.165* (0.030)	0.207*** (0.115)	0.191** (0.081)	0.168** (0.080)
Fixed Effects?	Time	Time	Time	Time, MSA First Difference	Time, MSA First Difference	Time, MSA First Difference
Panel Heteroscedasticity?	Yes	Yes	Yes	Yes	Yes	Yes
Error Structure?	No AR	Same AR(1) Across Panels	Panel Specific AR(1)	No AR	Same AR(1) Across Panels	Panel Specific AR(1)
N	19,577	19,577	19,576	19,067	19,067	19,066

NOTES: All the specifications include the all the regressors reported in Table 2. Only the coefficient of law adoption dummy variable is reported here. We employ iterative feasible Generalized Least Squares procedure. We test the hypothesis that the error term may follow different auto-regressive processes across MSAs and for each MSA. Robust Standard errors are reported within parentheses. '*', '**', and '***' imply 1 percent, 5 percent and 10 percent significance level.

Table 4 Parametric: Dynamic Panel Estimation: MSA

(Dependent Variable: Percent Change in HPI from previous quarter)

Regressors	**(1)**	**(2)**	**(3)**	**(4)**
Law Adoption	0.125 (0.108)	0.062 (0.111)	0.102 (0.117)	0.074 (0.114)
HPI-rate_Lag1	-0.008 (0.065)	0.264 (0.441)	-0.203 (0.278)	-0.546 (0.396)
Fixed Effects?	Time, MSA, First Difference	Time, MSA, First Difference	Time, MSA, First Difference	Time, MSA, First Difference
Over-Identification Test	$\chi^2(4)=150.097$ (Pr~0.00)	$\chi^2(4)=2.275$ (Pr~0.70)	$\chi^2(4)=6.442$ (Pr~0.11)	$\chi^2(4)=4.592$ (Pr~0.32)
Adj. R^2	0.001	0.001	0.001	0.001
N	17,096	16,568	16,049	15,527

NOTES: All the specifications include the all the regressors reported in Table 2. Only the coefficients of law adoption dummy variable and HPI lags are reported here. Clustered Standard errors are reported within parentheses. ‘*’, ‘**’, and ‘***’ imply 1 percent, 5 percent and 10 percent significance level. Labor force variables include unemployment rate and job growth rate. Lagged labor force variables do not seem to matter, so we do not include them in these models. Column (1) contains HPI-rate_Lags 3-7. Column (2) contains HPI-rate_Lags 5-9. Column (3) contains HPI-rate_Lags 7-11. Column (4) contains HPI-rate_Lags 9-13. F-tests reject the null hypotheses of equal intercept across time and cross-sections for all the models. The over-identifying restrictions for validity of instruments are not rejected for the model in column (2). See Wooldridge (2002), pg. 123-124 for a discussion on heteroscedasticity-robust version of the over-identification test.

Table 5 Semi-Parametric: Average Treatment Effect: Propensity Score Matching Estimation: MSA

Stratification Estimators			Nearest Neighbor Estimators			Kernel Matching Estimators		
(1) Average Difference	**(2)** Average Difference Year FE	**(3)** DID-Benchmark	**(1)** Average Difference	**(2)** Average Difference Year FE	**(3)** DID-Benchmark	**(1)** Average Difference	**(2)** Average Difference Year FE	**(3)** DID-Benchmark
0.055 (0.044)	0.158* (0.047)	0.166 (0.117)	0.069 (0.070)	0.173* (0.069)	0.195 (0.171)	0.099* (0.038)	0.206* (0.033)	0.219* (0.079)

NOTES: Treatment is the law adoption. Outcome is the percent change in average quarterly HPI from the previous year to current year. All the parametric models for estimating propensity scores include the controls as in Table (7) column (4). Bootstrapped standard errors are reported in parentheses. '*', '**', and '***' imply 1 percent, 5 percent and 10 percent significance level. Estimator- (1) is defined as Difference in Average HPI rate between treated and control groups. Estimator- (2) is obtained from estimator- (1) after controlling for the year effect. Estimator- (3) is defined as Difference-in-Difference in Average HPI rate after controlling for year effect between treated and control groups, relative to a HPI rate from a year before the disclosure law adoption as benchmark. Since there are some MSAs, which have missing HPI rate in early years of the sample period, we use earliest available HPI rate as the benchmark. However, we make sure that the benchmark is from a year prior to adoption of the disclosure law. This leaves us with 286 MSAs for the analysis. For Stratification estimators, we first estimate a probit model to obtain the cumulative probability of adopting disclosure law. The predicted cumulative probability from the probit model is the propensity score. Then, we split the sample into five (or more) equally spaced intervals (or bins) of the propensity score. Within each bin, we test that the average propensity score of treated and control units do not differ. If it differs, we split the interval more until the condition is satisfied. Next step is to test that the average characteristics do not differ between treated and control group in each bin. This implies that the balancing property is satisfied. The balancing property could not be satisfied with MSA level data for few bins. We discard those unbalanced bins. This is similar to discarding the bins where we do not find either any treated or control units. Discarding these bins does not affect the results. The Nearest Neighbor estimators take each treated unit and find the control unit, which is closest in terms of magnitude of the propensity score. Therefore, by construction, each treated unit should have matches, which enables us to avoid the pitfall of discarding some bins in stratification method. However, some matches would be poor in quality. The Kernel Matching estimators get all treated units matched with a weighted average of all control units, where weights are computed as inverse of the Euclidean distance between the propensity scores of the two groups. Therefore, it tackles the problem of poor matches from the nearest neighbor method.

Table 6 An Event Study of the Adoption of Disclosure Law: MSA

Event Date/ Quarter	Abnormal Return (AR)	Positive ARs %	33-Quarter CAR	25-Quarter CAR	17-Quarter CAR	9-Quarter CAR
-16	0.606* (0.251)	52	0.606			
-15	-0.111 (0.309)	44	0.495			
-14	0.047 (0.285)	54	0.542			
-13	0.525** (0.273)	52	1.067			
-12	-0.046 (0.250)	42	1.021	-0.046		
-11	0.090 (0.199)	51	1.111	0.044		
-10	-0.049 (0.201)	50	1.062	-0.005		
-9	0.182 (0.178)	52	1.244	0.177		
-8	0.367*** (0.207)	55	1.611	0.544	0.367	
-7	-0.255 (0.165)	43	1.356	0.289	0.112	
-6	0.095 (0.167)	51	1.451	0.384	0.207	
-5	-0.225 (0.167)	43	1.226	0.159	-0.018	
-4	-0.118 (0.170)	48	1.108	0.041	-0.135	-0.118
-3	0.255 (0.163)	52	1.363	0.296	0.119	0.137
-2	0.007 (0.161)	43	1.370	0.303	0.126	0.144
-1	-0.279 (0.163)	46	1.090	0.023	-0.153	-0.136
0	**0.256** (0.141)**	**50**	**1.346**	**0.279**	**0.103**	**0.120**
1	0.053 (0.126)	46	1.401	0.333	0.156	0.174
2	-0.271 (0.153)	44	1.128	0.061	-0.115	-0.098
3	-0.101 (0.178)	46	1.029	-0.038	-0.215	-0.197
4	-0.140 (0.178)	44	0.888	-0.179	-0.355	-0.338
5	0.164 (0.159)	52	1.052	-0.015	-0.192	
6	-0.008 (0.149)	49	1.044	-0.023	-0.199	
7	0.390* (0.157)	57	1.434	0.367	0.191	
8	0.111 (0.131)	50	1.545	0.478	0.302	
9	0.001 (0.156)	50	1.545	0.478		
10	0.224*** (0.135)	60	1.769	0.702		
11	0.028 (0.140)	48	1.797	0.730		
12	-0.044 (0.127)	49	1.753	0.686		
13	0.240*** (0.141)	50	1.993			
14	0.352* (0.119)	55	2.345			
15	-0.132 (0.126)	47	2.213			
16	0.086 (0.134)	57	2.299			

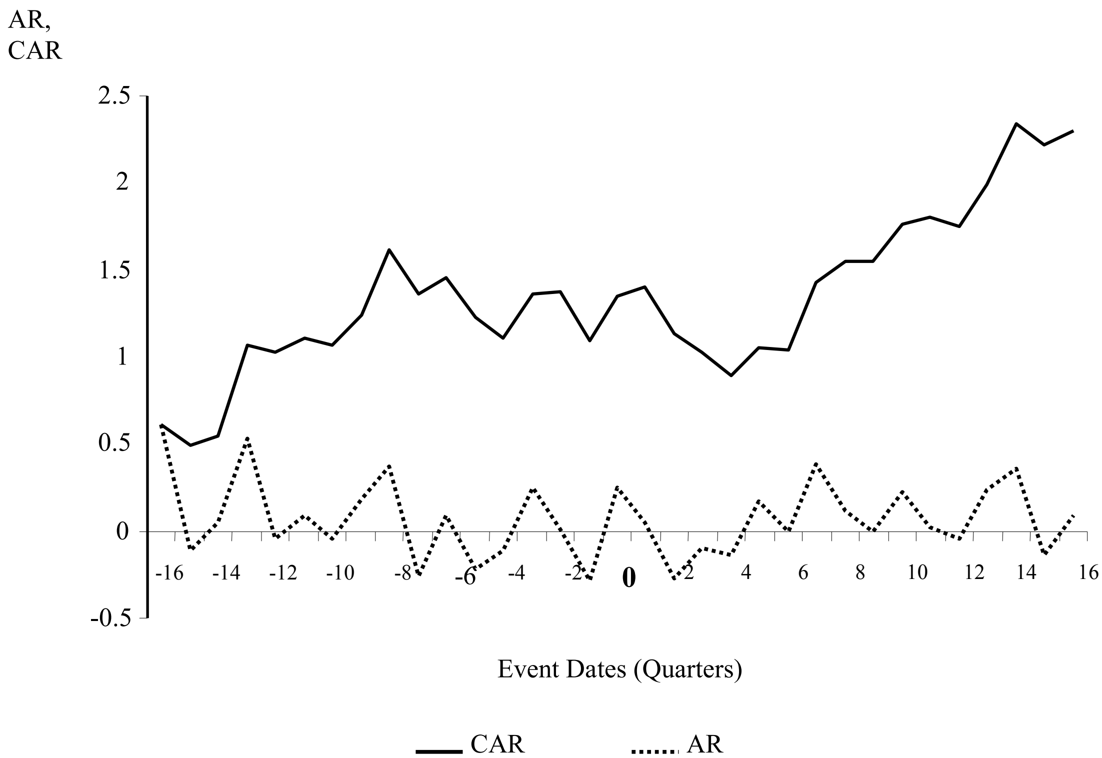

Figure 4 Plot of Cumulative Abnormal Return for Adoption of Disclosure Law

Table 7 Proportional Hazard Model of Law Adoption

(Dependent Variable: Law Adoption Dummy)

Regressors	(1)	(2)	(3)	(4)
Time-Invariant Avg. No. of Disciplinary Actions relative to avg. no. of complaints	0.006 (0.004)	0.006 (0.004)	0.007*** (0.004)	0.007*** (0.004)
Time-Invariant Licensee Supervision Index	-0.008** (0.004)	-0.008** (0.004)	-0.007*** (0.004)	-0.007*** (0.004)
Time-Invariant Number of Real Estate Licensees/1000 population	-0.022 (0.029)	-0.024 (0.028)	-0.023 (0.028)	-0.025 (0.028)
Democratic Control Democratic Governor	-0.124 (0.269)	-0.126 (0.268)	-0.093 (0.269)	-0.101 (0.268)
Republican Control Republican Governor	0.011 (0.226)	0.009 (0.227)	0.058 (0.233)	0.058 (0.233)
Democratic Control Republican Governor	0.071 (0.295)	0.071 (0.294)	0.107 (0.295)	0.105 (0.295)
Mortgage Rate	-0.374* (0.140)	-0.368** (0.145)	-0.365** (0.148)	-0.358** (0.147)
% Unemployment	-0.093 (0.077)	-0.115 (0.077)	-0.125 (0.081)	-0.135*** (0.080)
% Job Growth	0.173** (0.072)	0.169** (0.071)	0.171** (0.072)	0.170** (0.072)
%Per Capita Income Change	-0.262*** (0.151)	-0.246 (0.153)	-0.279*** (0.151)	-0.266*** (0.151)
%Per Capita GSP Growth Rate	-0.031 (0.027)	-0.025 (0.027)	-0.020 (0.028)	-0.019 (0.028)
%Population Growth Rate	0.091 (0.109)	0.103 (0.108)	0.126 (0.112)	0.128 (0.110)
HPI-rate_Lag-1		-0.085 (0.071)		-0.052 (0.070)
HPI-rate_Lag-2			-0.148** (0.069)	-0.141** (0.062)
Fixed Effect?	Census Division	Census Division	Census Division	Census Division
Joint Significance of Census Division Effects	$\chi^2(8)=22.32$ (Pr~0.00)	$\chi^2(8)=23.81$ (Pr~0.00)	$\chi^2(8)=27.75$ (Pr~0.00)	$\chi^2(8)=28.00$ (Pr~0.00)
Adj. R^2	0.231	0.222	0.212	0.213
N	728	678	628	628

NOTES: Models include a cubic function of time as the baseline hazard specification. Standard errors are reported within parentheses. '*', '**', and '***' imply 1 percent, 5 percent and 10 percent significance level. This analysis is done with all the states from 1984 to 2004.

Appendix A:
Event Study Procedure:

Following event study procedure is employed in this paper.

Event:	Adoption of the property condition disclosure law
Outcome Variable:	quarterly HPI growth rate
Event Window:	16 quarters before and 16 quarters after the adoption of the law.
Sample:	MSAs in 50 US states – 36 states adopted the law.
Notations:	Event time = 0;
	Pre-event time periods = -1,…, -16; Post-event time periods = +1,…, +16
	HPI growth rate for treated MSA= h^T
	HPI growth rate for control MSA= h^C
	Abnormal Return = *AR*
	Cumulative Abnormal Return = *CAR*
	MSAs = *k*; Treated MSAs = *i*; Control MSAs = *j*; $i, j \in k$

Event Time-line:

---------|--------|--------|--------|--------|---------|----------|---------|---------|-----------

-16 -12 -8 -4 ***0*** *+4 +8 +12 +16*

Step-1: Estimating Propensity Score: Logit Model

$$P(X_{kt}) \equiv \Pr\{legal = 1 \mid X_{kt}\} = E\{legal \mid X_{kt}\} \equiv \text{Propensity Scores}$$

Where, *legal*= {0, 1} is the law adoption dummy, and X_{it} is vector of MSA (*k*) economic characteristics and includes state-level institutional characteristics. Propensity score is the conditional probabilities of adopting the disclosure law. The estimated propensity score is obtained for each MSA in each quarter-year.

Step-2: For each treated MSA in respective event date, we find the closest match from the group of control MSAs in terms of the estimated propensity score. So, the HPI growth rate of matched control MSA would be the benchmark from which we calculate the deviations of the actual return or HPI growth rate of the treated MSA for each time period in the event window.

Step-3: Calculating the Abnormal Returns (***AR)***

For a given treated MSA, *i*, and a matched control MSA, *j*, we obtain:

$$AR_i^{-16} = \left(h_{i,-16}^T - h_{j,-16}^C\right)$$

.

.

$$AR_i^{0} = \left(h_{i,0}^T - h_{j,0}^C\right)$$

.

.

$$AR_i^{+16} = \left(h_{i,+16}^T - h_{j,+16}^C\right) \tag{14}$$

We calculate the average (across treated MSAs) abnormal returns for each event date.

Step-4: Calculation of Cumulative Abnormal Return (***CAR***)

CAR is calculated as the cumulative aggregation of the average ARs. For example, for a three period CAR (i.e. within one period of the event date), we obtain,

$$CAR = \left[AR_i^{-1} + AR_i^{0} + AR_i^{+1}\right] \tag{15}$$