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The Impact of Property Condition Disclosure Laws on Housing Prices: Evidence from an Event Study using Propensity Scores

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Abstract

We examine the impact of seller's Property Condition Disclosure Law on the residential real estate values. A disclosure law may address the information asymmetry in housing transactions shifting of risk from buyers and brokers to the sellers and raising housing prices as a result. We combine propensity score techniques from the treatment effects literature with a traditional event study approach. We assemble 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 is used to exploit 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. When we compare the results from parametric and semi-parametric event analyses, we find that the semi-parametric or the propensity score analysis generals moderately larger estimated effects of the law on housing prices.

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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It is well understood that resale markets with asymmetric information create a lemon's problem where owners have an incentive to keep products with above average quality on factors that are unobservable in the market and to sell products that have below average quality (Akerloff, 1970). Used automobile markets (Offer, 2007; Bond, 2002), resale of aircraft (Gilligan, 2004), owner-occupied housing markets, and even the market for securitized debt might suffer from this lemon's problem (Van Order, 2006; Glaeser and Kallal, 1997). A possible solution to this problem is government imposed seller disclosure and seller liability for known, undisclosed defects, which provides redress to buyers once they have taken possession of the property and have the opportunity to obtain private information concerning product quality (Fishman and Hagerty, 2003).[1]

The application of this solution to residential property markets was brought to 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.[2] Since that time, 34 state governments or courts have imposed seller disclosure requirements on the market for residential property. Several studies have documented the rationale and effect of seller disclosure laws including Zumpano and Johnson (2003) who find based on insurance claims that 76% of all suits against real estate salespeople involve the condition of the property, Nanda (2008) who finds a link between the number of disciplinary actions of against real estate agents and the adoption of disclosure laws, and finally Moore and Smolen (2000) who find that customer dissatisfaction dropped with the adoption of such laws.[3]

[1] Anti-lemon laws in the automobile industry provide an example of an alternative approach for addressing this problem (Shaffer and Ostas, 2001).
[2] 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.
[3] See Lefco (2004) for a detailed discussion of the implementation of seller disclosure laws, and Pancak, Miceli, and Sirmans (1996) for a discussion of real estate broker incentives concerning the adoption of seller disclosure laws.

This paper examines whether the adoption of seller disclosure laws over the last two decades has reduced the magnitude of the asymmetric information problem in residential property markets. Following Akerlof's (1970) 'lemons' paper, the average selling price of homes is reduced by the presence of asymmetric information as buyers adapt to the expectation that higher quality homes will be held off the market. If seller disclosure laws reduce the asymmetric information problem, housing prices should rise in response leading to an 'abnormal return' on real estate following the adoption of such laws. Accordingly, we treat the adoption of seller disclosure laws as an 'event' and test whether 'abnormal returns' can be detected following state adoption.

Specifically, this paper conducts both a traditional event study and a modified propensity score analysis to examine whether the adoption of seller disclosure laws creates temporary abnormal returns as prices adjust in response to a reduction in the lemons problem in the sale of owner-occupied housing. The event study methodology assumes that the market is efficient in assimilating new information and can detect the extent of re-pricing due to the event by examining whether returns on assets are unusually high immediately following the event, an "abnormal return". Since Fama, Fisher, Jensen, and Roll (1969), a myriad of papers in finance have applied the event study methodology including recent applications such as the effect of coal industry cartels (Bittner, 2005), the Japanese banking crisis (Miyajima and Yafeh, 2007), and the effect of corporate spin-off announcements (Veld and Veld, In Press). More specifically, in real estate, Brau and Holmes (2006) examine the effect of stock repurchase announcements by Real Estate Investment Trusts, and Fuerst (2005) examines the effect of the 9/11 attacks on the New York office market.

Event studies are most commonly applied in contexts where a market index provides a benchmark for comparing the returns from the assets that have been affected by an event. In cases where the affected assets are a large share of the market, however, a reasonable alternative is to use returns for the segment of the market that is unaffected by the event as a benchmark. This alternative benchmark illustrates the similarity between event studies and the standard treatment effects literature typically found in labor economics (Heckman and Hotz, 1989). A common concern in the treatment effects literature is systematic selection of individuals into either the treatment or control group, and in this literature propensity score models provide a standard solution for bias caused by a complex, non-linear process of selection on observables (Dehejia and Wahba, 1999; Heckman, Ichimura, and Todd, 1997 and 1998; Smith and Todd, 2000). Naturally, in the context of an event study, the traditional propensity score approach must be modified to allow for across time variation in the likelihood of the event.

Specifically, this paper examines whether the adoption of property disclosure laws leads to an increased rate of housing appreciation using a quarterly panel of housing price indices from the Office of Federal Housing Enterprise Oversight (OFHEO) for 291 US Metropolitan Statistical Areas (MSAs) and 50 US States spanning 21 years from 1984 to 2004. The impact of law adoption is estimated using both a standard event study based on traditional parametric panel data models, as well as semi-parametric propensity score matching model that is adapted to an event study framework by using a proportional hazard model of event occurrence or law adoption as the link function rather than a standard binary choice model of receipt of treatment. We find a robust positive effect of the seller's Property Condition Disclosure Law on property values especially for the propensity score model. The results suggest 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.

Rest of the study proceeds as follows Section 2 discusses the parametric panel estimation methods, the semi-parametric propensity score approach, and finally the standard event study. Section 3 provides the description of the economic and institutional variables, Section 4 analyzes, compares, and contrasts the results from different econometric models, and finally, we conclude in Section 5.

2 Methodology

At the onset of an empirical analysis of the impact of a law adoption, we face the choice between treating adoption as a one-time shock to the housing market or a shock that persists over several years. Unlike a disclosure that describes past financial outcomes, the adoption of a law may be followed by a period of uncertainty where individuals learn about the impact of the law over time, and the total impact of the law on asset prices may not be realized for several years. For example, rational buyers and sellers might gradually learn about the effectiveness of the law in bringing about the desired transparency in property transactions over time gaining confidence in the quality of the resulting housing units on the market (buyers) and in the fairness of prices obtained for housing units marketed with full disclosure. Figure (1) provides a diagrammatic exposition on the slow adjustment (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 or windows for duration of the shock or the period of "abnormal returns".

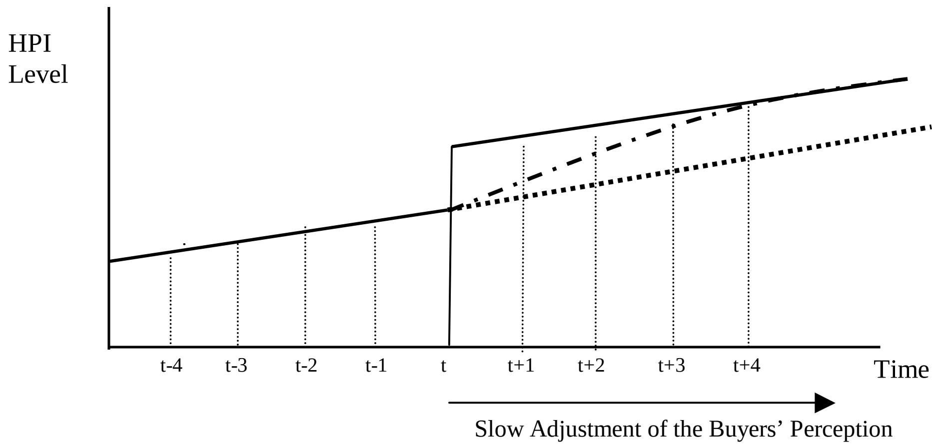

Figure 1 Movement of Housing Price Index at the level

2.1 Parametric Approaches to Ascertain the Effect on Property Values

We start with typical event study using traditional panel data procedures. Our model in equation 1 uses index i for MSA, j for state, t for quarter-year. The terms ω_t and σ_i capture the quarter-year fixed effect and the MSA fixed effect, respectively. The dependent variable is the percentage change in Y_{it} where Y_{it} is the 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, and S_{jt} is a binary variable that is one if the law has been adopted immediately preceding period t so that (S_{jt} - S_{jt-s}) takes on a value of 1 for s years (our event window) immediately following the adoption of the law.[4]

[4] The economic controls are standard in the literature on housing price volatility, see Miller and Peng (2005). We include the state-level institutional characteristics to control for the fact that such variables might be correlated with both law adoption and unobservables that correlate with housing price appreciation, but naturally we do not expect these controls to have a causal effect on housing prices.

$$\left(\frac{Y_{it} - Y_{it-1}}{Y_{it-1}}\right) \equiv y_{it} = \alpha X_{it} + \beta Z_{jt} + \delta(S_{jt} - S_{jt-s}) + \sigma_i + \omega_t + \varepsilon_{it} \qquad (1)$$

In this model, the quarter-year fixed effects capture housing price appreciation over time and act as the market benchmark for the event study. The parameter δ captures quarter-by-quarter whether housing price appreciation during a metropolitan area's event window differs systematically from housing price appreciation during other periods. This specification acts as an event study with or without the MSA fixed effects. However, the MSA fixed effects assure that models are identified based on difference in appreciation rates within an MSA after controlling for national housing appreciation rates in that quarter. As such, the model controls for observable and unobservable time-invariant differences across states that might influence both the likelihood of ever adopting the law and metropolitan specific housing price appreciation rates.

While standard errors are robust to serial correlation, the specification in equation (1) does not impose any specific assumptions about serial correlation in error structure, and in the current context, the unobservables especially those related to institutional structure may persist over time. Accordingly, we consider alternative GLS specification in which we eliminate MSA fixed effects via first differencing and allow the first difference error structure to follow the following possible patterns: no autocorrelation, same AR(1) across MSA's, and MSA-specific AR(1).

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

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). Propensity score methods have been extensively applied in program evaluation literature within labor economics (Dehejia and Wahba, 1999; Heckman, Ichimura, and Todd, 1997 and 1998; 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): (1) the propensity score approach imposes fewer assumptions about the distribution of the data, it permits non-parametric interactions among all the covariates in determining the outcome (i.e. selection on observables), it ascertains the mean impact of treatment on the treated within a group of 'very similar' units. As opposed to parametric approaches that consider all the units to infer an effect size.

The motivation of propensity score matching methods can be summarized as follows: In observational studies, the units are assigned to treatment and control groups in a highly non-linear manner. Therefore, even in models that include parametric controls for all variables that influence selection, treatment effect estimates will be biased by the unknown, non-linear selection process. 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 on the observables that influence selection. The 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 (Becker and Ichino, 2002). 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:

$$P(Z_j) \equiv \Pr[S_j = 1] = E\left[S_i \mid Z_j\right] \tag{2}$$

where $S_j = \{0, 1\}$ is the treatment dummy, and X_j is a vector of pre-treatment attributes.

Our parameter of interest is the mean effect of treatment (MET) on the treated units defined as:[5]

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

$$MET_{S=1} = \frac{1}{N_{S_j=1}} \sum_{\forall i \in S_j=1} y_j(X_j) - E[y_j \mid P(\gamma Z_j), S_j = 0] \qquad (3)$$

where y_j is the outcome of interest and the expected value of y_j for S_j=0 is estimated based on y_k for k's that have the same likelihood of treatment as k and for which S_k=0.

As described in Becker and Ichino (2002), the first step in obtaining estimates of treatment effects using a propensity score model is to estimate the likelihood of treatment and predict the likelihood of treatment for each observation typically using a standard discrete choice model like the Logit or Probit often referred to as the link function. However, in our context, the property condition disclosure law is adopted by different states at different points in time with events (law adoptions) occurring at various times rather than simply a single treatment. The probability for law adoption varies across states as well as time i.e. a disparate treatment exists. The fundamental assumption in logit and probit models that the probability approaches zero and one at the same rate is not tenable since the law has been adopted by different states in different time. According to Clarke, Courchane, and Roy (2005), use of a symmetric link function (i.e. inverse of the cumulative distribution function) like logit or probit would extend bias when such disparate treatment exists and is not capable of capturing the variations over time. A good alternative for our problem is the complimentary log-log model that incorporates an asymmetric link function.[6] {comment - Please eliminate the use of the word disparate treatment – it is confusing Jargon and no one will no what you are talking about}

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[6] The hazard function can be represented by a standard normal cumulative distribution function.

$$\log it(P_1(w_j;\beta)) = \log[P_1(w_j;\beta)/(1-P_1(w_j;\beta))] = w_j'\beta$$

In equation (5), the underlying assumption is that the probability $P_1(w_j;\beta)$ approaches zero and one at the same rate i.e. the link is symmetric. However, as explained before, a disparate treatment exists. An alternative is complimentary log-log or the proportional hazard link function, which is:

$-\log(-\log(1-P_1(w_j;\beta))) = w_j'\beta$ or, $P_1(w_j;\beta) = 1-\exp(-\exp(w_j'\beta))$ with $P_1(w_j;\beta)$ approaching one faster than zero. See Clarke, Courchane, and Roy (2005).

Specifically, we will adopt discrete time proportional hazard model as our link function[7] and estimate the likelihood of law adoption using the following model (Kiefer, 1988; de Figueiredo and Vanden Bergh, 2004)

$$\lambda_j(t) = \lambda_0(t)\exp(\gamma Z_{jt}) \tag{4}$$

where $\lambda_i(t)$ represents the hazard of law adoption for a state that has not yet adopted the law and the probability of a state adopting the law in one period conditional on not having adopted it previously is

$$\Pr[S_{jt} = 1 \mid \gamma Z_{jt}, S_{jt-1} = 0] = E\lfloor S_{jt} - S_{jt-1} \mid \gamma Z_{jt} \rfloor \tag{5}$$

The estimated probability of a given quarter-year t being in state j's event window is simply

$$E[S_{jt} - S_{jt-s} \mid \hat{\gamma} Z_{jt}, \hat{\gamma} Z_{jt-1}, \ldots, \hat{\gamma} Z_{jt-s}] = \sum_{u=1}^{s} \Pr[S_{jt-s+u} = 1 \mid \gamma Z_{jt-s+u}, S_{jt-s+u-1} = 0] \tag{6}$$

which is used as the propensity score for every quarter-year in each state or in the MSA's for each the state.

We employ a Kernel Matching method where each treatment observation or MSA-quarter year is 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 units.[8] The Kernal Matching method efficiently uses all information to form a control or benchmark for each treatment observation, which is important when some treatment observations have few control or comparison observations with similar propensity scores. Finally, we verify that for each treatment observation the weighted average of the observed state attributes X_{jt} for control

[7] Obviously, many complex extensions exist to the traditional proportional Hazard model. However, propensity score estimation is very robust to misspecification in the link function and so is typically conducted with very simple link functions such as the proportional hazard model in our case and logit or probit in traditional applications (Dehejia and Wahba, 1999).

[8] Traditionally, the score is used to divide the sample into equally spaced intervals or bins, and within each bin a test is conducted for whether the average propensity score of treated and control units do not differ statistically. If it differs, the interval split again until the condition is satisfied. With Kernel Matching all treated are matched with a weighted average of all controls with weights that are inversely proportional to the distance between the propensity scores of treated and controls.

observations does not differ statistically observation from the state attributes for the treatment observation. The estimation is carried out in the common support region. Common support refers to overlapping distributions of their characteristics. The major advantage of propensity score approaches compared to traditional regression-based methods is that regression hides the common support problem, because it does not quantify the similarities (or dissimilarities) between the two groups.

Finally, we calculate the average difference between outcomes (i.e. percentage change in HPI) of the treatment/event window MSA-quarter years and the weighted average of control quarter year HPI as described in equation (3).

2.3 Adapting the Event Study Methodology

An event study aims to estimate the "abnormal return" that occurs during an event window that immediately follows the event - the difference between the expected price of an asset based on benchmark appreciation rates following the event and the observed price in the end of the event window. The following scheme outlines the modified event study procedure:

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 – 34 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 Model using equation (6). 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:

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

.

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

.

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

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

Step-4: Calculation of Cumulative Abnormal Return (**CAR**): It 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 - $CAR = \left[AR_i^{-1} + AR_i^{0} + AR_i^{+1}\right]$.

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 the MSA level analysis, we utilize the quarterly information i.e. 24,444 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, 34 states have already mandated some form of disclosure statement. We obtained the effective dates of the mandate from official statements for each state.[9] 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 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 33 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 State Product (GSP), and the population growth rate, which are obtained from the Bureau of Economic Analysis (BEA). We also include single-family 30-year average mortgage rates for states. Per capita Gross Metropolitan Product (GMP) data is not publicly available. We compute MSA share of GSP to use it as a proxy for GMP[10]. United States Conference of Mayors and the National Association of Counties publish GMP data from 1997.

[9] Pancak et al. (1996) lists the states, which adopted the disclosure law until 1996.

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

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. The economic variables with the exception of the labor market controls are available on a yearly basis. We interpolate these variables to the quarterly level[11]. Broadly, these variables characterize the economic make-up of each metropolitan area.

3.2 Institutional Variables:

Numerous lawsuits against the real estate licensees made the case for adoption of disclosure laws. Presumably the legal activities are influenced 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 level of dissatisfaction with licensee provided service. Similarly, the number of disciplinary actions taken against the licensees provides information about how the monitoring authority performs its duty.[12] Other institutional controls include number of active brokers, associate brokers, and salespersons in each state and the broker supervision. In fact, it was the concerted

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[11] 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.

[12] 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.

movement and lobbying on the part of realtor's association, which moved law onto the agenda of in many state legislatures. In order to control for the organization of real estate agents in different states, we include the number of active brokers, associate brokers, and salespersons in each state in our analysis.[13] 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 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 detailed partisan control variables rather than a simple blue/red category. We used indicator variables capturing Democratic-Control-Republican-Governor, Democratic-Control-Democratic-Governor, Republican-Control-Republican-Governor, Republican-Control-Democratic-Governor. 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. A few important observations can be made based on the summary statistics. 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 1.22 percent against 1.06 percent for States) for the treated group than for the control group. Unemployment rate and other economic controls are generally higher for the control units. Remarkably, average number of disciplinary actions (about 134 against 51) and average number of complaints (about 860 against 737) are higher for the states,

[13] Ideally, the percentage of licensees who are associated with some trade organizations like National Association of Realtors (NAR) could serve as an excellent indicator of the lobbying effort. However, it is hard to obtain this information across the states for the long time series required for this study.

which adopted disclosure law, but this may arise in part from high levels preceding adoption. Generally, the higher number of disciplinary actions and complaints against the licensees in treatment states 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 (51 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, but this may result from the important role of the industry in lobbying for seller disclosure laws as opposed to traditional consumer protection groups.

4 Empirical Results

We discussed the slow adjustment process of the legal shock in the section (2). To get a sense of the "speed" of the adjustment process we use equation (1) 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. We try two ways to test the robustness of the outcome. First, we keep the sample size same for all the lengths limiting our sample to states that adopted the law at least nine years prior to the end of our sample period. Next, we adjust the sample size as we increase the length so that shorter event windows allow the inclusion of states that adopted the law closer to the end of our sample. In figure (2), we plot the estimates on legal dummy variable from different specifications in terms of lengths of law adjustment.

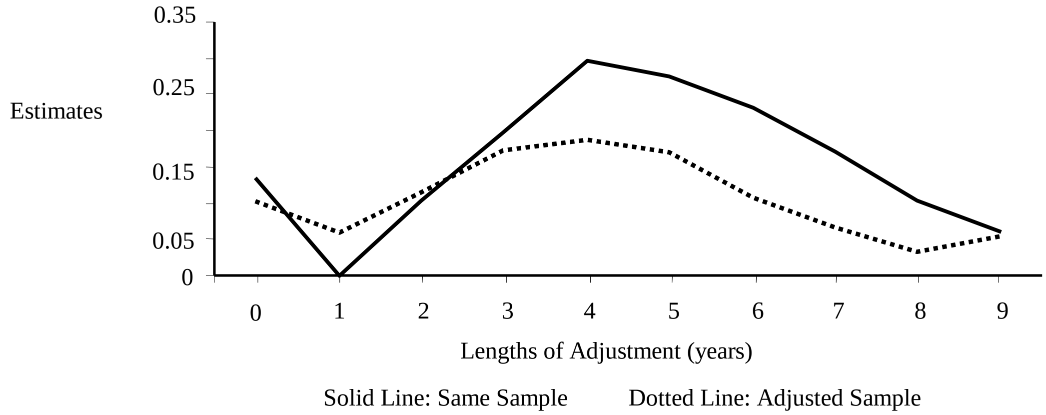

Figure 2 Plot of the Estimates

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) appears to suggest that the estimate is almost zero when we specify the length as 8 to 9 years, however, figure 2 is based on quarterly appreciation rates. In order to get the actual effect size of the event, we need to multiply the estimates in figure 2 with the corresponding number of quarters that we specify as the event window.

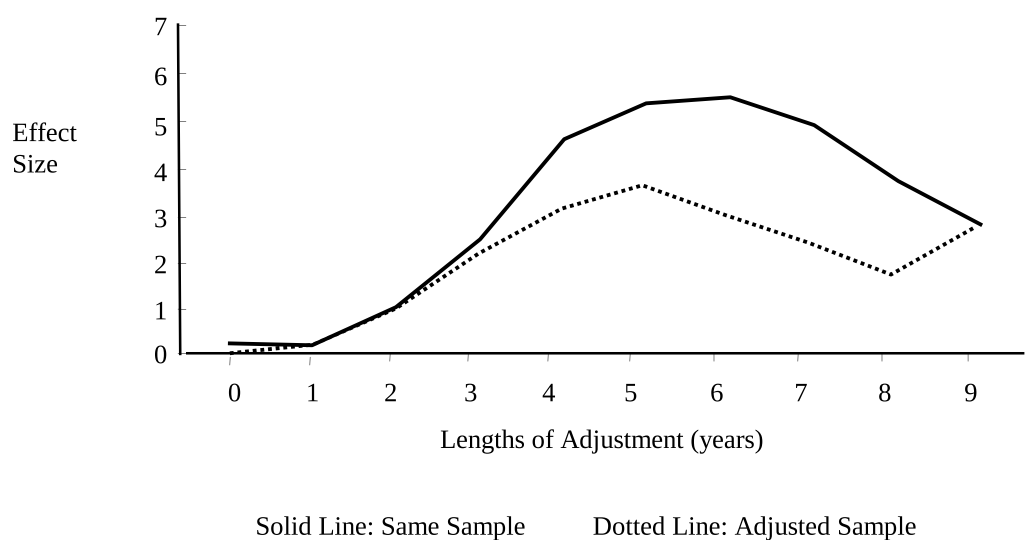

Figure 3 Plot of the Actual Effect Sizes

For example, in figure (2), the effect is about 0.167 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 2.67 (= 0.167*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 treat the adoption of the law as an event that creates "abnormal returns" for four years following the event.[14]

4.1 Parametric Results

Results for equations (1) are reported in table (2) for the MSA level analysis. Column (1) reports estimates that only control for quarter-year fixed effects while columns (2) and (3) report the estimates after including state and MSA fixed effects respectively. The estimated impact of law adoption is fairly stable across the three models. The estimate of 0.167 differential appreciation rates from column (3) is the figure used in figure (2) for an event window of 4 years. Multiplying this effect by 16 quarters yields a 2.67 (= 0.167*16) percent increase in asset values during the event window relative to the market benchmark.

Results from feasible GLS procedure are reported in table (3). As discussed before, feasible GLS procedure provides improvement (in terms of efficiency gain) over pooled regressions when we specify the error structure. The estimated effect size in table (3) column (3) is nearly identical to the 2.67 percent increase in asset values from column (3) in Table (2).

[14] Similar results are obtained using a six year event window.

4.2 Propensity Score Estimates

Propensity scores are obtained from a proportional hazard model with economic and institutional variables as described in Nanda (2008). Virtually all our data is annual so that there is almost no variation in propensity scores across quarters. Therefore, this analysis is done with yearly data at the MSA level i.e. information about 291 MSAs for 21 years although annual appreciation rates are divided by four to be comparable to quarterly rates used in Tables (2) and (3) [15]. We look at the effects with three different estimators: (1) a simple average difference in percentage change in HPI that does not control for cross-section and time effects; (2) an average difference in percentage change in HPI after controlling for the year effect; and (3) a differences in differences estimate based on removing year and MSA fixed effects. Estimator (1) is simply the difference in Average HPI rate between the treatment and control groups, state quarter-years that fall in or outside of each states event window. Estimator (2) is obtained from estimator (1) after controlling for the year effect so that the outcome variable is a quarter-year's deviation from average appreciation in that quarter-year across the entire sample. Estimator (3) is defined as difference between average HPI deviation for a quarter year (after controlling for year affects) between treated and control groups compared to the difference between the treated and control groups' average HPI deviation from a year before the disclosure law was adopted, where the year before adoption provides a market benchmark. Since there are some MSAs, which have missing values on the HPI 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.

[15] 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. Therefore, 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.

Table (4) reveals the results from semi-parametric propensity score matching analysis with kernel matching. The estimated effect size falls between 1.59 and 3.50 percent. Column (3) of table (4) reveals about 3.50 percent (i.e. 0.219*16= 3.50, for 16 quarters) significant and positive effect at the MSA level, which is higher than 2.67 percent that we found in column (3) of tables (2) and (3). The first column does not involve any controls for differentials in the national rates of housing price appreciation during periods shortly after the adoption of laws in many states and other periods of time. Therefore, in our view, the results in columns (2) and (3) which fall between 3.30 and 3.50 percent are our best estimates that are most directly comparable to the parametric estimates in Table (2).

4.3 Analysis of Abnormal Returns

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 as laid out in section (2.3). The abnormal returns or 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. 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 16th quarter to the specified quarter.

Table (5) 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.

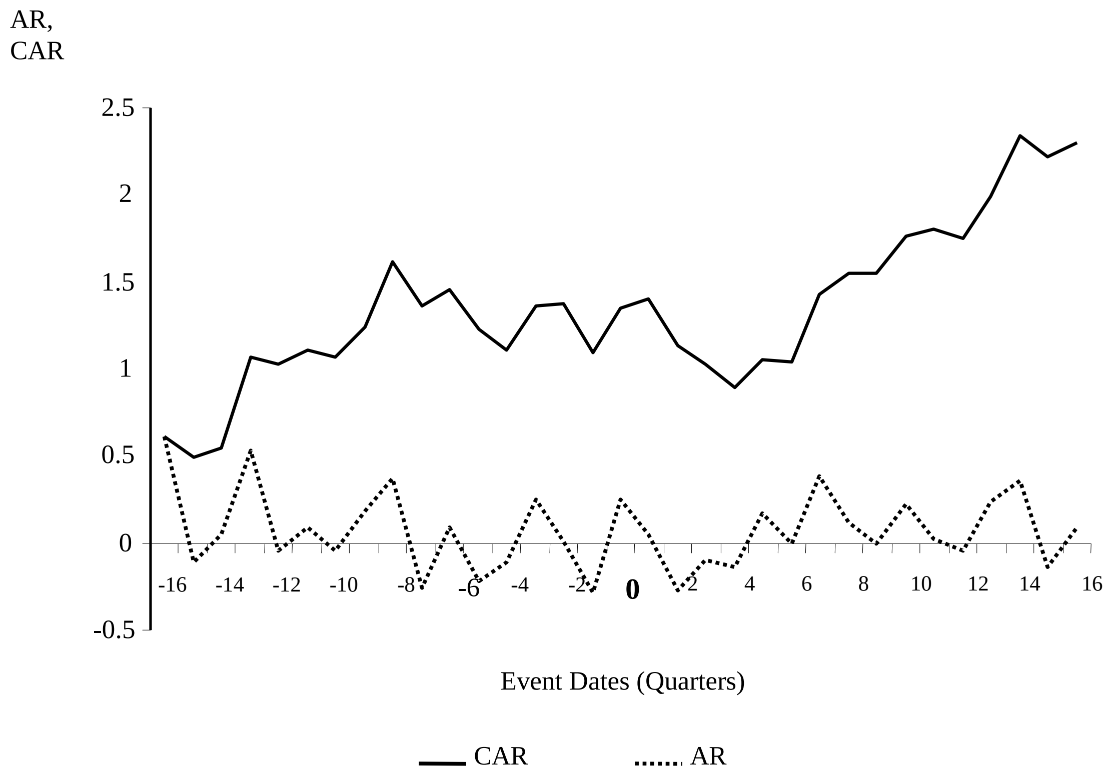

Figure 4 Plot of Cumulative Abnormal Return for Adoption of Disclosure Law

The plot of CARs in figure (4) reveals that the effect of 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.

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 panel data models, semi-parametric propensity score matching models, and an event study framework using a rich set of economic and 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 a positive effect 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. Moreover, we show that compared to parametric event study, the propensity score effects are somewhat larger in size. The results suggest 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. 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

Variable	Disclosure Mandate N	Disclosure Mandate Mean	Disclosure Mandate Std. Dev.	No Disclosure Mandate N	No Disclosure Mandate Mean	No Disclosure Mandate Std. Dev.
291 Metropolitan Statistical Areas Characteristics: 1984Q1—2004Q4: 24,444 Observations						
%Change in HPI[16]	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	714	1.225	1.299	336	1.059	0.937
%Unemployment Rate	714	5.532	1.682	336	5.583	1.875
%Job Growth Rate	714	1.536	1.891	336	1.622	1.983
%Per Capita Income Change	714	1.391	0.644	336	1.416	0.774
%Per Capita GSP Growth Rate	714	4.861	3.434	336	4.766	3.163
%Population Growth Rate	714	1.058	1.093	336	0.979	1.185
%Mortgage Rate	714	8.432	1.784	336	8.434	1.773
Number of Real Estate Licensees/1000 population	714	6.144	2.810	336	5.991	4.511
No. of Complaints	714	860.112	1465.934	336	737.382	2465.497
No. of Disciplinary Actions	714	134.243	267.121	336	50.767	53.126
Licensee Supervision Index	714	47.555	26.202	336	50.529	25.585
Democratic Control Democratic Governor	714	0.225	0.418	336	0.277	0.448
Democratic Control Republican Governor	714	0.224	0.417	336	0.241	0.428
Republican Control Republican Governor	714	0.293	0.455	336	0.259	0.438
Republican Control Democratic Governor	714	0.258	0.437	336	0.223	0.417

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

Table 2 Parametric Event Study: Fixed Effect Analysis at the MSA Level

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

Regressors	**(1)**	**(2)**	**(3)**
Law Adoption	0.251* (0.056)	0.196* (0.057)	0.167* (0.054)
Mortgage Rate	0.327* (0.112)	0.751* (0.130)	0.758* (0.131)
%Unemployment	-0.009* (0.003)	-0.006** (0.002)	-0.003 (0.003)
%Job Growth	0.003 (0.006)	0.003 (0.006)	0.003 (0.006)
%Per Capita Income Change	0.073* (0.011)	0.077* (0.011)	0.081* (0.011)
%Per Capita GMP Growth Rate	0.115* (0.039)	0.082** (0.040)	0.081** (0.039)
%Population Growth Rate	0.611* (0.096)	0.782* (0.092)	1.351* (0.112)
Democratic Control Democratic Governor	0.051 (0.064)	0.241* (0.069)	0.221* (0.069)
Republican Control Republican Governor	-0.029 (0.054)	0.084 (0.061)	0.101 (0.060)
Democratic Control Republican Governor	-0.073 (0.052)	0.018 (0.071)	-0.011 (0.070)
Number of Real Estate Licensees/1000 population	0.009 (0.007)	-0.042* (0.016)	-0.044* (0.017)
% Disciplinary Action taken / number of complaints	-0.062 (0.061)	0.076 (0.088)	0.073 (0.088)
Licensee Supervision Index	-0.008* (0.001)	-0.009* (0.002)	-0.009* (0.002)
Fixed Effects	Time Only	Time & State	Time & MSA
Joint Significance of Time Effects	F (83, 290) =36.95 (Pr= 0.00)	F (83, 290) = 36.67 (Pr= 0.00)	F (83, 290) = 35.69 (Pr= 0.00)
Joint Significance of Cross-Section Effects		F (48, 290) = 20.72 (Pr= 0.00)	F (60, 290) =88236.11 (Pr= 0.00)
Adj. R^2	0.107	0.128	0.143
N	19,994	19,994	19,944

Table 3 Parametric: Feasible GLS Procedure: MSA

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

Regressors	**(1)**	**(2)**	**(3)**
Law Adoption	0.184 (0.116)	0.191** (0.082)	0.167** (0.080)
Fixed Effects	Time & MSA	Time & MSA	Time & MSA
Panel Heteroscedasticity	Yes	Yes	Yes
Error Structure	No AR	Same AR(1) Across Panels	Panel Specific AR(1)
N	19,491	19,491	19,490

Table 4 Semi-Parametric Event Study: Average Treatment Effect

Propensity Score Matching Estimation

Kernel Matching Estimators

	(1) Average Difference	**(2)** Average Difference Year FE	**(3)** DID-Benchmark
Treatment Effect	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 (year-over-year change). Bootstrapped standard errors are reported in parentheses. ‘*’, ‘**’, and ‘***’ imply 1 percent, 5 percent and 10 percent significance level.

Table 5 An Improvised Event Study of the Adoption of Disclosure Law

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