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Fear of crime and housing prices: Household reactions to sex offender registries ☆

Jaren C. Pope *

Department of Agricultural and Applied Economics (0401), Virginia Tech, Blacksburg, VA 24061, USA

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ABSTRACT

Megan's Law requires public dissemination of information from sex offender registries. Opponents to this controversial law have questioned whether households misinterpret or even use this information. One concern was that the information might simply induce a "fear of crime." This study finds evidence for both use and misinterpretation of the publicly available information on sex offenders. Using a unique dataset that tracks sex offenders in Hillsborough County, Florida, the results indicate that after a sex offender moves into a neighborhood, nearby housing prices fall by 2.3% ($3500 on average). However, once a sex offender moves out of a neighborhood, housing prices appear to immediately rebound. Surprisingly, these price impacts do not appear to differ in areas near high risk offenders labeled as "predators."

1. Introduction

The abduction of eleven-year-old Jacob Wetterling in October of 1989 led to the enactment of the "Jacob Wetterling Crimes Against Children and Sexually Violent Offender Registration Act" in 1994. This act required every state to create a sex offender registry. The brutal murder of Megan Kanka by a neighbor who was also a twice-convicted child molester was the impetus behind congress enacting the 1996 "Megan's Law." Megan's Law amended the 1994 Jacob Wetterling Act by requiring dissemination of information from the sex offender registry to the public. Currently every state has complied with the legislation and most states have websites that provide access to the sex offender registry over the internet. Accessible information typically includes a picture of the offender, information on the offence(s), whether or not the offender is classified as a "predator," and the current address of the sex offender.[1]

In passing Megan's Law and in defending it in the courts, the government and judicial system have repeatedly concluded that the public safety benefits from providing this information exceed the privacy costs to the offenders. However, this conclusion has been controversial. Opponents of the law have questioned the legality of placing a publicly viewable "scarlet letter" on a sex offender that effectively punishes the offender twice.[2] Concerns have also been raised about the possible costs resulting from the public misinterpreting the information placed on sex offender registries. For example, it is possible that the information might lead to an increased "fear of crime" cost where households' subjective evaluations of sex offense risk, widely diverge from objective measures of sex offense risk.[3] This may be especially true if households fail to recognize the different level of risk posed by an eighteen year old man listed for statutory rape with his sixteen year old girlfriend and a sixty year old man listed for molesting a four year old boy. Another example of a cost from misuse of the registry information is the possibility that the public could use the infor-

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* Fax: +1 (540) 231 7417.
E-mail address: jcpope@vt.edu.

[1] See http://www.klaaskids.com for information and links to state websites.

[2] The U.S. Supreme court has rejected the "double-jeopardy" argument by concluding that sex offenders pose a unique threat to communities. Interestingly there are no public registries for murderers or other criminals who are subsequently released from prison.

[3] See Hale (1996) for a review of the "fear of crime" literature.

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mation to perform acts of vigilantism to drive sex offenders out of neighborhoods or cause them physical harm.[4] A final concern with the registries was that the public safety benefits would fail to materialize if the public did not actively search out the information available on the registry.

This study investigates households' reactions to the information in sex offender registries through their impact on housing prices. The investigation will provide information about households' marginal willingness to pay to reduce crime risk. Given that sex offenders' addresses are posted on these registries, informed households can use this information to alter their home buying decisions. If the information on the residential locations of sex offenders reduces the prices of homes nearby in a causal way, then this would provide evidence that at least some of the public is paying attention and using the information made available by these registries to reduce crime risk. Furthermore, if there are differential impacts for high risk offenders labeled as "predators," this would provide evidence that the public is interpreting the information correctly by distinguishing between different sex offender risk types.

There have been two other studies that have investigated the relationship between sex offender locations and housing prices. The first by Larsen et al. (2003) used a single year of housing data in Montgomery County, Ohio, and the sex offenders that were listed as living in the county at the end of the year, to generate a cross-sectional estimate. They found that housing prices appeared to be reduced by approximately 17% for homes within 0.1 miles of registered "predators," but that there was only an 8% reduction in price for sex offenders without the "predator" label. The causal interpretation of these cross-sectional estimates however is questionable given the potential for omitted variable bias.[5] The second paper by Linden and Rockoff (2006), exploits both cross-sectional and temporal variation in sex offender locations and housing prices in Mecklenburg County, North Carolina. This was possible because of a file they obtained on all sex offenders living in the county as of January 1, 2005 that contained the approximate dates for when those offenders moved into their current residence. Using a difference-in-difference identification strategy that compared housing prices in areas before and after a sex offender moved in, this paper estimated that the introduction of a sex offender into a neighborhood reduced housing prices within 0.1 miles by approximately 4%.

The present study makes three important contributions beyond this previous literature. First it strengthens the causal interpretation of the impact of sex offenders on housing prices by exploiting a unique dataset that not only provides information on when sex offenders move into neighborhoods, *but also when they move out*.[6] Using housing data purchased from the property appraiser's office in Hillsborough County, Florida and a unique dataset on sex offender movements provided by the Florida Department of Law Enforcement (FDLE), an analysis of the impact of sex offender residential locations on housing prices was conducted. The identification strategy exploits the quasi-random variation that sex offender movements provide in both space and time, in a fixed effects framework. The findings suggest that the average "treated" house within a tenth of a mile of a registered sex offender living in a single family residence, sold for 2.3% less after the sex offender moved into the neighborhood. This is approximately a $3500 reduction for the average priced house in the sample. Moreover in the "reversal treatment," housing prices in the tenth of a mile area surrounding a sex offender residence appear to rebound shortly after the sex offender leaves the neighborhood suggesting that the sex offender neighborhood "move in" estimate is causal.

A second contribution of this study is that it explores whether or not households and housing prices react differently to sex offenders labeled as high risk "predators" using both cross-sectional and temporal variation. It is found that the housing price impact described above appears to be invariant to whether or not the sex offender is labeled as a "predator." These results suggest that households are indeed using the registry information but are misinterpreting this information given that household valuation of risk does not appear to be in line with objective estimates.[7] This finding is quite different from the cross-sectional result produced by Larsen et al. (2003) and has important policy implications about how the released information is affecting household welfare.

A third contribution of this study is that it analyzes if households and housing prices reacted to widespread media coverage of two high profile child abductions and murders committed by sex offenders in the spring of 2005 in Florida. These events were widely covered by the media and therefore could potentially cause a change in household reactions to offenders and thereby produce a differential impact on housing prices. Results from this study found no such impact. Again, this may have important implications for policies that mandate publicly provided information.

The results from this study not only provide empirical evidence of households' reactions to an important national law, but also add to at least three literatures in economics. First, methodologically the analysis adds to the work by Black (1999) and Chay and Greenstone (2005) who illustrate how quasi-random experiments can be used in conjunction with the hedonic model and housing data to more accurately reveal household preferences. The quasi-random experiment in this study is unique because it provides the ability to not only estimate a treatment effect on an implicit housing price, but also a "reversal treatment" effect. This is important because even in a quasi-random experiment there can be omitted variable bias and the reversal treatment provides a robustness check for the empirical evidence generated by the experiment. Second, the analysis adds to the literature that includes Thaler (1978), Cullen and Levitt (1999), Katz et al. (2001), Kuziemko and Levitt (2004), Gibbons (2004) and others, on the value of crime risk reduction. The quasi-random variation in sex offender locations provides an opportunity to break the endogeneity that has plagued this literature for one specific type of crime. Finally, the analysis adds to a literature on the impact of public information disclosure programs that includes Ippolito and Ippolito (1984), Mathios (2000) and Jin and Leslie (2003). This study also concludes that public information disclosure programs can influence household behavior.

The remainder of the study will proceed as follows. Section 2 provides background on how households might perceive the information on residential locations of sex offenders. Section 3 gives some background on the study area and describes the data used in

[4] Since the registries have been available there have only been a few cases of lethal vigilantism. However, other forms of harassment in an attempt to drive offenders from neighborhoods appear to be more common.

[5] For example, if sex offenders tend to locate in low income areas that suffer from disamenities not controlled for in the cross-sectional regression, then this would bias the estimate for the impact of sex offenders on housing prices.

[6] In most "natural experiments" the analyst does not have the luxury to witness a reversal in the treatments that have naturally occurred. As long as housing near a previous sex offender residence is not stigmatized, then one would expect housing prices that had been depressed while the offender lived in the neighborhood to immediately rebound after the sex offender moved out of the neighborhood. Since the information made available on the sex offender registries does not provide the previous addresses of offenders (unlike the archived data acquired for this study), it is difficult to imagine that a stigma effect would exist. Therefore, finding that housing prices rebound when they receive the "reversal treatment" would strengthen the causal interpretation of any housing price decline after a sex offender moved into a neighborhood.

[7] It should be noted that this estimate is somewhat noisy and therefore some caution should be exercised in interpreting the result.

the analysis. Section 4 outlines the identification strategy for the hedonic price regressions used to determine the impact of sex offender locations on housing prices. Section 5 presents the results and Section 6 concludes the study.

2. Background on sex offender risks and perceptions

It is difficult to define variables to be used in an empirical analysis that represent the "sex offender risk" attribute of housing for which households care about. The way that households perceive the residential locations of sex offenders is important to how these variables should be defined. The sex offender attribute is likely a combination of objective risk, households' subjective risk and other emotional or fear-based responses that households may have. This section reviews evidence of the objective risks of sex offenders and then discusses how households may interpret information in the sex offender registries as sex offender risks.

Objective risks of sex offenders are difficult to calculate. However, there is some evidence that convicted sex offenders have a substantial probability of re-offending. Using rates of reconviction for various types of sex offenders from the largest dataset of its kind (4724 offenders), Hanson et al. (2003) found that sexual recidivism rates are approximately 14% after five years, 20% after 10 years, and 30–40% after 20 years. However, the Hanson study admits that these observed recidivism rates may substantially underestimate the actual rates because some sex offenders re-offend and are not caught. They argue based on what they view as plausible assumptions that the true recidivism rates are likely 10–15% higher than the observed rates suggesting that on average approximately half of sex offenders re-offend within 20 years.[8] In related work, Hanson and Bussiere (1998) have shown that certain characteristics are strong predictors of recidivism risk. For example sexual interest in children, deviant sexual preferences, prior sexual offenses, and whether or not the victims were strangers are some of the strongest predictors of re-offense. These predictors are often used by states to label sex offenders who are more likely to re-offend as "predators."

Other evidence suggests that sex offenders are more likely to commit their offenses locally than other types of criminals. Using statistics of a nationally representative survey conducted with prison inmates by the United States Department of Justice, Larsen et al. (2003) report that 85.1% of sex offenders committed their offense in the same city in which they resided at the time of their arrest. This is compared to 80.2% for other criminal offenses. Furthermore they report that 64.9% of sex offenders committed their offense in their own neighborhoods compared to 44.6% for other criminal offenses. Although these statistics are rather vague in the definition of neighborhood and the type of offenses used to calculate the statistics, they are suggestive that sex offenders are apt to commit their offenses locally. Given sex offenders likelihood for re-offending on a local scale, the policy rationale for a registry was that providing this information to households would enable them to self-protect by warning their children and increasing neighborhood vigilance.

How households perceive the residential locations of sex offenders is certainly related to these objective risks, but likely also has an emotional component as well. Stewardship of children is a widely recognized social responsibility. A primary reason for the public support of Megan's law no doubt stems from this shared commitment and the emotional response of the lay public to crimes against children. This emotional response has made it politically feasible to pass laws requiring registration of sex offenders and public dissemination of the information. Feelings of shock, revulsion, fear, and finally outrage likely describe the emotional reaction that society has towards highly publicized heinous sex crimes. These same feelings may be important drivers of households' micro-level responses to sex offender information such as their response to the news that a registered sex offender lives next door.

Some evidence of emotional and risk based responses is provided by a study conducted by Beck and Travis (2004). They examined the relationship between "fear of victimization" and receiving a sex offender notification. Surveys were conducted with heads of households that had received notification that a sex offender lived on the adjacent property (their treated group) and other nearby households that had not received any such notification (their control group). They found that households that had received a notification were more likely to fear sexual victimization for themselves and for their family members than households not living near an offender and not having received a notification. This result was strongest when the survey respondent was female or had less education.

The Beck and Travis study highlights the importance of households' *perceptions* on living near a sex offender. These perceptions are certainly influenced by subjective risk calculations and emotion. Thus the object of choice or attribute conveyed by a home's location to a buyer—proximity to a sex offender—is complex. It is more difficult to quantify the emotional response that some people may feel upon learning of a sex offender in their neighborhood. These reactions seem likely to contribute to an informed buyer's perceptions of neighborhood quality.

Without direct evidence of household reactions to publicly available sex offender information for the study area, the subsequent empirical analysis uses distance to a sex offender's residence as a proxy for the subjective risk and neighborhood perception responses of households. Distance is likely highly correlated with households' emotional responses to being "near" an offender and may also reflect some elements of risk. It is difficult *a priori* to determine what households' perceptions of "near" might be. However, the results presented later in this study suggest that only housing transactions that took place within a tenth of a mile *after a sex offender moved into* a neighborhood are influenced by the residential location of sex offenders. If households are indeed fully informed, then this is what the data reveals as households' perceptions of "near."[9]

3. Study area and data

3.1. The study area: Hillsborough County, Florida

A primary consideration in choosing a study area was finding a state that archived information on the locations of sex offenders *over time* and could provide it for this research. Florida is one of the only states that could fulfill this requirement. Most state registries update their websites to keep the posted information current, but the update destroys some of the historical information about the offender such as their previous residential addresses. Florida was also the first state to list sex offenders on a publicly accessible website beginning October 14, 1997.[10] In addition to the website,

[8] Also see Doren (1998) for a discussion of why recidivism base rates are generally underestimated.

[9] If a significant fraction of households are uninformed, then a tenth of a mile is likely to be an understatement of "near." Thus the results presented later on in this study should be interpreted as a lower bound.

[10] This website is maintained by the Florida Department of Law Enforcement (FDLE) and can be accessed at http://www.fdle.state.fl.us/. Posted information includes a picture of the offender, the offenders designation (predator or offender), name, supervision status, date of birth, race, sex, hair and eye color, weight and height, aliases, any scars/marks/tattoos, address information, adjudication and date, crime description and limited victim information.

Fig. 1. Location of Hillsborough County, Florida.

the same information was made available through a 24 hour a day hotline.[11] Therefore, the residential locations of sex offenders have been publicly available and easily accessible for a substantial period of time.

Once Florida was determined to be the state in which the empirical analysis would be conducted, the next task was to select a county. The selection criteria included: (i) a county with a large number of houses, (ii) a county with a large number of sex offenders, and (iii) a county where housing prices, attributes and geographic locations of housing could be acquired. Hillsborough County, Florida was found to meet these criteria.[12] Based on the 2000 census, the county has approximately 1 million people. The largest city in the county is Tampa with approximately 300,000 people. Fig. 1 shows the location of Hillsborough County in relation to the rest of Florida. There are a large number of housing transactions that have occurred in the county over the relevant time period. In addition, there have been a significant number of sex offenders that have lived in this county since the sex offender registry was developed.

3.2. Data used in the analysis

There are two primary sources of data used in the analysis. The first is a dataset on all single-family housing transactions occurring between October 1996 and April 2006 in Hillsborough County. Data on sales prices and property characteristics were purchased from the Hillsborough County Property Appraiser's Office. The data was screened to drop "unqualified" sales and outlying observations.[13] A detailed GIS parcel map was also acquired from the appraiser's office. From this map the centroid of each parcel was calculated and this geographic reference point was linked to each of the houses in the database. This process allows information from the sex offender dataset described later, to be spatially merged to the housing dataset.

In addition to the sale prices, the appraiser's database also provides a set of important structural control variables that can be used in the analysis. These include information on the age of the house, the acreage of the lot on which the house is built, the number of bedrooms, the number of fixtures, the source for heating and cooling of the house, the architectural type, and the "effective" area of the house. This last variable takes the gross area of the house and multiplies each one of the sub areas by an "effective rate factor" that takes into account if it is a heated area and the area "type." This adjustment provides a more comprehensive measure of house size than heated square footage. Panel A in Table 1 provides summary statistics for the single family transactions in Hillsborough County from 1996–2006.

The second dataset is the publicly available information on the sex offenders that have resided in Hillsborough County since the registry was made publicly available. The Florida Department of Law Enforcement provided this archived information from November 1997 through April 2006. These data include addresses, latitude and longitude coordinates of the residence, whether or not the offender was listed as a "predator," gender of the offender, and the date when the offender listed the address as his/her new residence. Sex offenders since 1997 have been required to register their new address with the FDLE within 2–10 days of moving to a new residence. Using the dates when a new address was listed for an offender in the archive, approximate move in and move out dates were derived. Table 2, panel A, provides summary statistics for the 2824 sex offenders that resided at some point between 1997 and 2006 in Hillsborough County, Florida. Approximately 10% of these sex offenders were listed as "predators,"[14] 76% were white and 98% were male. Offenses that warrant listing on the registry include those that commit sexual offenses against either children or adults. However, the FDLE did not provide the offense-specific information for the offenders in this dataset for this research although the information is available on the internet.

To identify the causal impact of sex offender's residential locations on nearby housing prices, it was necessary to drop some of the sex offender location observations before this information was linked to the housing dataset. All sex offender residential locations where the offender did not live in the residence for at least 6 months were dropped.[15] Panel B in Table 2 gives summary statistics of the sex offenders that lived for at least six months in their residence. Using the addresses provided by the FDLE for this screened sample of sex offenders, it was then possible to match approximately 85% to a parcel in the housing dataset.[16] The housing dataset provides information on what types of build-

[11] This number is 1-888-FL-PREDATOR. Florida legislation also requires county sheriff's offices to provide additional public disclosure as "deemed necessary."

[12] The analysis was first attempted in Alachua County, Florida because the housing data in that county was available from a previous project. However, upon matching the housing data to the sex offender information, it was determined that there were an insufficient number of housing transactions and sex offender locations for the identification strategy employed by this study.

[13] House sales below $5700 and above $2,900,000 which correspond with the approximate 1st and 99th percentiles were dropped. Unqualified sales are those that involve one of the following: multiple parcels, a court order, developer, an addition, business Inc. or personal property Inc., improvement incomplete, agricultural sale, death certificate, bank sale, government subsidized, or distressed sale.

[14] In the state of Florida the "predator" designation is issued by the court for certain types of heinous offenses, or if an offender re-offends for certain types of offenses. The "predator" designation signifies a greater risk that the offender will re-offend. See the FDLE website and their bulletin entitled "2004 guidelines to Florida Sex Offender Laws" for more details.

[15] Sex offenders that live in a location on a temporary basis complicate an identification strategy since there is often not enough housing sales bracketing these short time periods, and because temporary sex offender locations are often in areas where multiple offenders reside.

[16] This was an involved process. It was found that the latitude and longitude coordinates provided by the FDLE did not provide sufficient spatial resolution for the empirical analysis. As a result, the sex offender information had to be matched to the exact parcel to ensure locational accuracy. First using a GIS software called Arcview and an electronic road file, the addresses of both the sex offenders and the parcels were geocoded. Using the lat/longs from the geocoding as a unique id, approximately 70% of the sex offenders were matched directly to their corresponding parcel layer. The other 30% were assigned to the nearest 25 parcel centroids. Further matches were determined by hand, comparing the sex offender address with these nearest parcel addresses. Approximately half of the remaining 30% were either matched to the correct parcel or a "suitably correct" parcel which consisted of the exact same address but the number being within 5 street address units. So an

Table 1
Summary statistics of housing variables used in analysis

Variable	Description	Mean	Median	Standard deviation	Minimum	Maximum	Observations
Panel A: All single family transactions from 1996–2006							
price	sale price of property	172,183.80	143,900.00	114,771.90	5700.00	2,863,299.00	189,491
lprice	ln(price)	11.89	11.88	0.56	8.65	14.87	189,491
age	age of property in years	19.37	13.00	20.71	0.00	106.00	189,491
acreage	acreage of lot	0.30	0.19	0.83	0.00	123.97	189,491
utsBR	number of bedrooms	3.21	3.00	0.85	0.00	12.00	189,488
utsBR_2	utsBR squared	11.00	9.00	5.61	0.00	144.00	189,488
utsBT	number of fixtures	6.40	6.00	2.13	0.00	24.00	189,491
eff_ar	the "effective" square footage	2065.35	1953.00	780.07	105.00	10,749.00	189,491
AC_dum2	has central AC	0.98	1.00	0.15	0.00	1.00	189,491
onetenth_mile	within 0.1 miles of S.O.	0.16	0.00	0.37	0.00	1.00	189,491
twotenth_mile	between 0.1 and 0.2 miles of S.O.	0.20	0.00	0.40	0.00	1.00	189,491
Panel B (housing subset #1): Transactions in areas with no S.O.'s or within 0.3 miles of non-transient S.O.'s							
price	sale price of property	198,977.80	166,000.00	124,532.70	5700.00	2,863,299.00	118,558
lprice	ln(price)	12.06	12.02	0.52	8.65	14.87	118,558
age	age of property in years	13.39	7.00	16.62	0.00	106.00	118,558
acreage	acreage of lot	0.35	0.20	1.00	0.00	123.97	118,558
utsBR	number of bedrooms	3.42	3.00	0.80	0.00	12.00	118,557
utsBR_2	utsBR squared	12.33	9.00	5.59	0.00	144.00	118,557
utsBT	number of fixtures	6.99	6.00	2.04	0.00	24.00	118,558
eff_ar	the "effective" square footage	2302.65	2171.00	771.22	191.00	8687.00	118,558
AC_dum2	has central AC	0.99	1.00	0.10	0.00	1.00	118,558
onetenth_mile	within 0.1 miles of S.O.	0.03	0.00	0.17	0.00	1.00	118,558
twotenth_mile	between 0.1 and 0.2 miles of S.O.	0.07	0.00	0.26	0.00	1.00	118,558
Panel C (housing subset #2): Transactions in areas within 0.3 miles of non-transient S.O.'s							
price	sale price of property	155,574.20	138,500.00	79,968.85	11,500.00	880,000.20	5924
lprice	ln(price)	11.85	11.84	0.46	9.35	13.69	5924
age	age of property in years	15.80	10.00	17.73	0.00	101.00	5924
acreage	acreage of lot	0.24	0.19	0.38	0.00	18.87	5924
utsBR	number of bedrooms	3.26	3.00	0.79	0.00	9.00	5923
utsBR_2	utsBR squared	11.27	9.00	5.46	0.00	81.00	5923
utsBT	number of fixtures	6.47	6.00	1.81	0.00	15.00	5924
eff_ar	the "effective" square footage	2041.95	1910.00	689.66	394.00	6453.00	5924
AC_dum2	has central AC	0.99	1.00	0.10	0.00	1.00	5924
onetenth_mile	within 0.1 miles of S.O.	0.15	0.00	0.36	0.00	1.00	5924
twotenth_mile	between 0.1 and 0.2 miles of S.O.	0.36	0.00	0.48	0.00	1.00	5924
onetenth_mile_post	within 0.1 miles and transacted after S.O. enters	0.07	0.00	0.25	0.00	1.00	5924
twotenth_mile_post	between 0.1 and 0.2 miles and transacted after S.O. enters	0.19	0.00	0.39	0.00	1.00	5924

Notes: Year dummies, block group dummies, sex offender area dummies, fuel and heat dummies and architecture type dummies are not included in the summary statistics above.

ings and activities occur on each parcel. All sex offenders from the sample who were not capable of being linked to a parcel designated as "single-family-residential" were dropped from the sample. Including only sex offenders on single-family-residential parcels that lived in the residence for at least six months, reduces the likelihood of multiple sex offender treatments (the halfway house affect) or some of the unobserved impacts from trailer courts, apartments, etc. that can affect residential neighborhoods.

Using the sex offender location information and the parcel map, the distances from each parcel centroid where a housing transaction occurred, to the nearest 25 sex offender locations were calculated. Using this "distance to sex offender locations" information with the full housing transaction dataset, two subsets of housing data were created. Housing subset #1 was created by dropping all housing locations that occurred within 0.15 miles *of more than one offender location* at any time between 1997–2006. In other words this sample is composed of the houses that were never "near" a sex offender location and houses that were only "near" an offender's location once. Panel B in Table 1 provides summary statistics for housing subset #1. Housing subset #2 was created by dropping all housing locations in housing subset #1 that were further than 0.3 miles from a sex offender location. Panel C in Table 1 provides summary statistics for housing subset #2, panel C in Table 2 provides summary statistics of the 322 sex offenders that are relevant to both of the housing subsets, and Fig. 2 shows their residential locations in relation to Hillsborough County. When compared with panel A of Table 2 it can be seen that there is a lower fraction of offenders listed as "predators" (6%) and a higher fraction of white offenders in this sub-sample. Further discussion on the reasoning behind the creation of these housing subsets and their usefulness in identifying the impact of sex offenders on housing prices will be delayed until Section 4.2.

Using the timing of when these sex offenders moved in and out of their residence in relation to the timing of the housing sale as well as the linear distance between sex offender residences and neighboring housing, four key variables were created: (i) *one_tenth_mile* is a dummy variable equal to 1 for houses that sold within one tenth of a mile of a sex offender the year before or the year after the sex offender moved into the neighborhood, (ii) *two_tenth_mile* is a dummy variable equal to 1 for houses that sold within two tenths of a mile of a sex offender the year before or the year after the sex offender moved into the neighborhood, (iii) *one_tenth_post* is a dummy variable equal to 1 for houses that sold within one tenth of a mile of a sex offender the year *after* the sex offender moved into the neighborhood, and

address of 301 Oak Lane could be matched to 303 or 305 Oak Lane if 301 did not exist, but it could not be matched to 307 Oak Lane.

Table 2
Sex offender summary statistics

Variable	Mean	Median	Standard deviation	Minimum	Maximum	Observations
Panel A: All S.O.'s living in Hillsborough sometime between 1996 and 2006						
Offender	0.90	1	0.31	0	1	2824
Predator	0.10	0	0.31	0	1	2824
White	0.76	1	0.43	0	1	2824
Black	0.24	0	0.43	0	1	2824
Other	0.00	0	0.05	0	1	2824
Male	0.98	1	0.13	0	1	2824
Female	0.02	0	0.13	0	1	2824
stay_length	291.70	100	499.86	0	3139	11,472
Panel B: S.O.'s living in a single family residence for at least 6 months						
Offender	0.91	1	0.28	0	1	1130
Predator	0.09	0	0.28	0	1	1130
White	0.74	1	0.44	0	1	1130
Black	0.26	0	0.44	0	1	1130
Other	0.00	0	0.05	0	1	1130
Male	0.98	1	0.14	0	1	1130
Female	0.02	0	0.14	0	1	1130
stay_lengt	826.20	504.5	746.74	183	3139	1660
Panel C: S.O.'s living in a single family residence for at least 1 year and not within 0.3 miles of any other offender						
Offender	0.94	1	0.24	0	1	322
Predator	0.06	0	0.24	0	1	322
White	0.87	1	0.34	0	1	322
Black	0.13	0	0.33	0	1	322
Other	0.00	0	0.06	0	1	322
Male	0.98	1	0.16	0	1	322
Female	0.02	0	0.16	0	1	322
stay_length	1137.89	820.5	828.64	366	3139	322

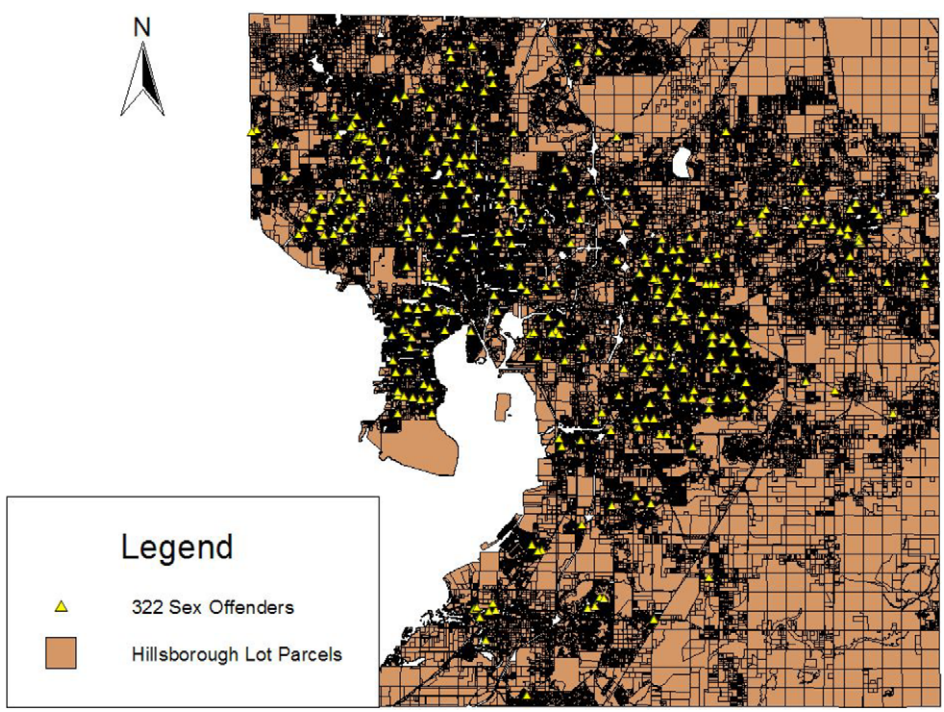

Fig. 2. The 322 sex offender locations used in the analysis.

(iv) *two_tenth_post* is a dummy variable equal to 1 for houses that sold within two tenths of a mile of a sex offender the year *after* the sex offender moved into the neighborhood, and 0 otherwise. Panels A, B, and C in Table 1 also provide summary statistics for these key variables. Fig. 3 shows an example of a sex offender residence in relation to residential parcels within 0.1, 0.2 and 0.3 miles.

4. Identification strategy for the hedonic analysis

The hedonic pricing method is commonly used by economists in conjunction with the housing market to understand households' valuations of local amenities and disamenities. Rosen (1974) is widely credited with developing the theoretical foundation for hedonic models, and early applications of the technique included

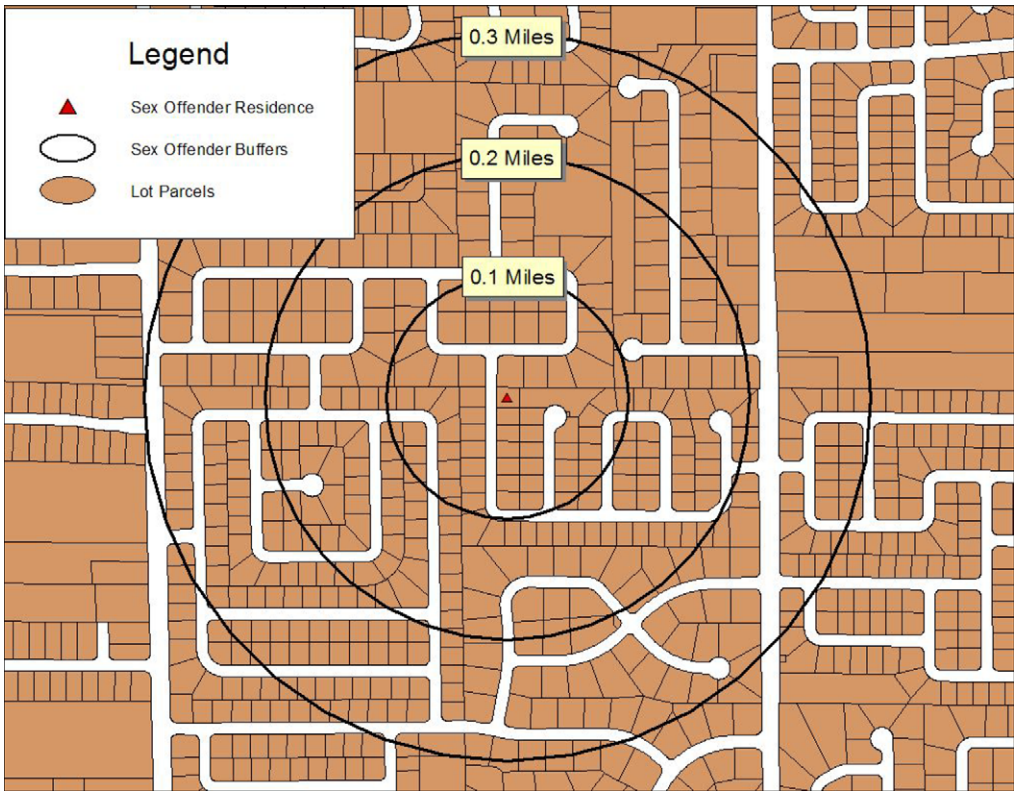

Fig. 3. Example of parcels within 0.1, 0.2, and 0.3 miles.

attempts to understand the value of air quality (e.g. Ridker and Henning, 1967), the value of schools (e.g. Kain and Quigley, 1970), and more closely related to this study, the value of reducing crime (e.g. Thaler, 1978). The typical goal of hedonic analysis in housing markets has been to isolate the "implicit price" of a particular housing attribute that is the focus of a study.[17]

A major concern with the implementation of the hedonic pricing method in the housing market using cross-sectional data has been the issue of omitted variable bias. The fear is that unobserved factors that are spatially correlated with the amenity of focus will lead to inconsistent estimates of implicit prices. For example, in the case of sex offenders, the concern is that sex offenders' residential locations may be correlated with other unobserved forms of crime, housing attributes or negative neighborhood features which would lead to estimates that overstate sex offenders' impact on housing prices.

The potential empirical importance of omitted variable bias has recently been highlighted by Black (1999) and Chay and Greenstone (2005), both of whom developed quasi-random experimental procedures that allow the econometrician to better purge omitted variables from the model. The work presented in this study builds off of this literature in the construction of an identification strategy to understand the impact of sex offender locations on housing prices. Furthermore the identification strategy used in this study exploits a "reversal treatment" that is rarely available in quasi-experimental work, to test the robustness of the results. This is important because quasi-random experiments can still suffer from omitted variables that can bias estimates obtained from the hedonic pricing method. This section begins by showing how a cross-sectional approach to the problem would proceed, and then a quasi-experimental identification strategy is developed.

4.1. Cross-sectional strategy

A simple cross-sectional approach that uses the distance of houses to the nearest sex offender to estimate the impact of sex offenders on housing prices would take the following form,

$$\text{lprice} = \beta + \alpha\text{Structural} + \phi\text{Spatial\_dums} + \theta\text{Proximity\_dums} + \varepsilon \quad (1)$$

where "lprice" is the log of housing transaction prices over one time period (a year for example), "Structural" is a set of structural characteristics of houses in the sample, "Spatial_dums" are dummy variables for spatial subsets of the study area (such as census tracts or block groups), and "Proximity_dums" are dummy variables for houses within specified distances of the nearest sex offender location listed on the registry at the end of the time period.[18] Also, ε is an error term and β is a parameter to be estimated and α, ϕ, and θ represent vectors of parameters to be estimated. If omitted variable bias is present, estimates of θ will likely overstate the true impact of sex offender locations since the "Proximity_dums" variables likely reflect other negative locational attributes that are correlated with sex offender locations and are not being adequately controlled for by the "Structural" and "Spatial_dums" variables.[19] Determining the appropriate spatial ag-

[17] For more detail on the hedonic pricing method see Palmquist (2005) who provides an excellent review of the hedonic pricing method in the context of environmental valuation.

[18] As discussed in Section 2, informed buyers' responses to the information that an offender lives in close proximity to a house are likely to be a mixture of subjective risk assessments of a sexual offense against the household, changes in perceptions of the neighborhood and other emotional responses. In the identification strategies described in this section the distance to a sex offender's residence is used as a proxy for the emotional and risk responses made by households.

[19] See Angrist and Krueger (1999) for a more thorough discussion of the difficulties inherent in using cross-sectional analysis to determine causal impacts.

gregation for the "Spatial_dums" variables is therefore especially important in a cross-sectional identification strategy.

Another difficulty with specification (1) is that since the "Proximity_dums" are distance dummies to the *nearest* sex offender, if sex offender locations are correlated with each other in space then the estimates may reflect the impact of multiple offenders (for example, ten offenders living in the same half-way house) rather than the estimate of a single sex offender on housing prices. An added complication arises if some sex offenders moved during the period of the analysis, then some housing transactions may be inappropriately indicated by the "Proximity_dums" since the transactions may have actually occurred before the sex offender moved in or after the sex offender moved out.

4.2. Defining the sex offender treatment

The spatial–temporal nature of the collected housing and sex offender data allows for an identification strategy that exploits both space and time to isolate the impact of the publicly available sex offender information on housing prices. The public availability of the information in the registry is relatively constant over the study period's time frame, while its spatial relevance for any given household changes with sex offender movements. This allows for a test of the differences in housing prices before and after a sex offender moves into a neighborhood. A sex offender moving into a neighborhood can be thought of as the experimental "treatment." Ideally this treatment on transaction prices in the neighborhood can be compared to similar houses transacted in neighborhoods that did not receive an offender.

Sex offenders as a group are highly transient. The "stay_length" variable in panel A Table 2 shows that the average length of stay for all locations where sex offenders lived between 1997 and 2006 in Hillsborough County was approximately 297 days. However the distribution is skewed. The median number of days is only 100. In areas that have sex offenders moving in and out frequently, it would be difficult to estimate the "sex offender treatment effect" when multiple treatments are occurring over a short time horizon.[20] Housing subsets #1 and #2 were created to address this issue. The average stay length for the set of sex offenders used to create these datasets is approximately 3 years (reported in panel C of Table 2).

By excluding the most transient offenders and limiting the analysis to residential areas that were only "treated" by a sex offender one time (or never), this subset of sex offenders and housing allows for a clear definition of the sex offender treatment. This definition being: *the introduction of one sex offender into a residential neighborhood where sex offenders had never previously resided*. Restricting the data in this way is not costless. Estimates of the average treatment effect for this selected portion of the data may not provide an accurate measure of the housing price effects of areas with multiple sex offenders or areas with a high percentage of apartments and trailer courts. Estimating the differential impact of sex offenders that dwell in other types of housing or in areas with a highly transient population of sex offenders on housing prices is left for future research.

[20] Residential areas that have had more than 1 sex offender during the period of analysis appears to be substantial. Using the sex offender residential locations where the sex offender lived in the residence for at least 6 months, Appendix Table A.1 was derived. It provides summary statistics for some of the housing variables of houses transacted with different numbers of sex offenders having lived within 0.3 miles of the house at some point over the time period of this analysis. As can be seen, approximately 32% of the transactions in the database were in areas where more than 2 sex offenders had lived. 12% were in areas where 5 or more sex offenders had lived and 3.5% were in areas where 10 or more sex offenders had lived. Houses in areas with more sex offenders on average are smaller, older, located on smaller lots and have lower transaction prices.

4.3. Spatial–temporal identification strategy

An initial specification that exploits both the cross-sectional and temporal variation could take the following form,

$$\begin{aligned}\text{lprice} = \beta + \alpha\text{Structural} + \phi\text{Spatial\_dums} \\ + \delta\text{Year\_dums} + \theta\text{Proximity\_dums} \\ + \omega\text{Proximity\_dums\_post} + \varepsilon. \end{aligned} \tag{2}$$

The difference between the cross-sectional specification shown in Eq. (1) and the specification developed in Eq. (2) is the inclusion of the "Year_dums" to control for year specific housing market features in the data and the "Proximity_dums_post" variables. These latter dummy variables indicate houses that sold within a certain distance of a sex offender *after* the sex offender moved into the residence. The coefficients on these dummy variables provide estimates of the impacts of a registered sex offender on housing prices relative to some omitted proximity dummy variable.[21] The task of developing an adequate "control" for this "treatment" is discussed in the remainder of this section.

4.3.1. Controlling for potential temporal confounders

While the identification strategy described by Eq. (2) extends a simple cross-sectional analysis, it may nonetheless yield biased estimates. If there is one or more omitted temporal or spatial variables that invalidate what is being used as the control group, then these factors can lead to biased estimates and confound the ability of the model to detect these types of subtle influences. For example, Hillsborough County and many other counties in Florida have seen rapid price appreciation relative to many other parts of the country over the past few years. If the dummy variables for annual effects do not adequately control for differential trends in price appreciation over the spatial extent of the county during the 1996–2006 time period of the housing dataset, this limitation could bias estimates. To mitigate this possibility a set of linear time trends that correspond with the spatial indicators were created.

Another potential temporal confounder was that Florida had two child abductions and murders committed by sex offenders in the spring of 2005 that were widely covered by the media. On February 23, 2005 nine-year-old Jessica Lunsford was reported missing. A search for the girl which included extensive media coverage was unsuccessful until on March 19, 2005 it was discovered that Jessica Lunsford was killed by a registered sex offender who had been living near her residence at the time. Households in Florida and throughout the nation were outraged. It was reported that the number of hits on the FDLE website increased from 19,000 the week before to 209,000 the week after the event. Three weeks later on April 10, 2005 thirteen-year-old Michelle Lunde was also reported missing. On April 17, 2005 it was announced that Michelle Lunde had also been abducted and killed by a registered sex offender. This second abduction occurred in Hillsborough County. A concern is that the estimates may be driven by housing transactions occurring after these widely covered abductions. To check for a differential impact of this sort, a dummy variable for housing transactions that occurred after March 19, 2005 was created and interacted with the "Proximity_dums_post" variables.

There may be other temporal confounders not controlled for by these linear time trends over such a long time horizon. To help mitigate other potential temporal confounders, only housing transactions near a sex offender that took place the year before or the year after the sex offender moved into the neighborhood were retained for the final analysis. Eq. (2) can be updated to include the

[21] The primary regressions presented later on in Section 6 use proximity dummies of 0.1, 0.2, 0.3 miles.

measures used to mitigate potential temporal confounding influences in the following way,

$$\begin{aligned}\text{lprice} = \beta &+ \alpha\text{Structural} + \phi\text{Spatial\_dums} \\ &+ \delta\text{Year\_dums} + \theta\text{Proximity\_dums}^R \\ &+ \omega\text{Proximity\_dums\_post}^R \\ &+ \psi\text{Spatial\_linear\_trends} \\ &+ \xi\text{Proximity\_dums\_post\_interact} + \varepsilon \end{aligned} \tag{3}$$

where "Proximity_dums" and "Proximity_dums_post" have been superscripted with an "*R*" to denote that housing transactions that are proximate to sex offenders are "restricted" to the year before or the year after the sex offender moved into the residential location, "Spatial_linear_trends" are the set of linear time trends for each of the "Spatial_dums," and the "Proximity_dums_post_interact" variable denotes houses proximate to sex offenders that transacted after the Jessica Lunsford abduction.

4.3.2. Controlling for potential spatial confounders

Unobserved spatial heterogeneity is always a concern in hedonic analyses of the housing market. It is not clear what would be the appropriate spatial controls to include in the specification. A valid control group from the perspective of identifying the impact of a registered sex offender on housing prices is one where the spatial variables statistically render the treated and untreated groups comparable. Several different sets of "Spatial_dums" variables were developed including tax districts, census tracts and census block groups in Hillsborough County.[22] An attractive alternative to these political boundary dummy variables would be sex offender specific spatial dummies. To this end, spatial dummy variables for all houses within each 0.3 mile area of each sex offender were created. These are the "Spatial_dums" used in conjunction with housing subset #2 in the most tightly bounded (in time and space) regressions reported in the next section. However, this reduces the housing sample substantially so specifications using housing subset #1 and block group spatial dummies (the smallest available political units) are also considered.[23]

A related spatial concern is that the locations in which sex offenders choose to live, even within the 0.3 mile spatial controls or block group controls, is not random. The summary statistics for the housing data in Table 1 suggest that the sex offenders in single-family residential houses tend to be in areas with lower housing prices. If sex offenders live in the housing that is of lower quality in terms of unobserved attributes relative to other housing in the block-group level or 0.3 mile spatial control areas, this would bias a cross-sectional analysis. It may also bias an analysis that exploits the timing of sex offender movements if the introduction of an unobserved low quality attribute is correlated with the sex offenders' arrival in the neighborhood. For example if sex offenders move into newly built low income housing, estimates of the impact of sex offenders on housing prices may be influenced by the perceptions about the low income housing as well as the sex offenders.

In an effort to determine if the specifications used in the primary regressions are being confounded by unobserved temporal or spatial variables, a specification check and a robustness check are considered. The specification check involves estimating models using housing subset #1 with the block group controls and housing subset #2 with the 0.3 mile controls excluding observations that are "near" a sex offender and that occurred after the sex offender moved into the neighborhood. Eq. (4) describes this strategy,

$$\begin{aligned}\text{lprice} = \beta &+ \alpha\text{Structural} + \phi\text{Spatial\_dums} \\ &+ \delta\text{Year\_dums} + \theta\text{Proximity\_dums}^{RN} \\ &+ \psi\text{Spatial\_linear\_trends} + \varepsilon \end{aligned} \tag{4}$$

where the "*R*" in the "*RN*" superscript on "Proximity_dums" is a reminder that observations are again "Restricted" to the year before and after a sex offender's arrival. The "*N*" is a reminder that those observations that are "Near" a sex offender and that occurred the year after the sex offender moved into the neighborhood have been removed.[24] If the results from such a regression suggest no difference between the control group and observations in the same area as the "treated observations" that were excluded, then the specification would appear to be providing adequate control for any unobserved temporal and spatial elements of the housing environment.

The robustness check exploits another treatment that the temporal nature of the archived sex offender information in Florida allows. Not only do these data provide an approximate date for when sex offenders moves into a neighborhood, they also provide an approximate move out date. If sex offender locations do in fact have a causal impact on housing prices, then it might be expected that once sex offenders leave a neighborhood, housing prices would rebound.[25] Such a result would lend credibility to the causal interpretation of the sex offender "move in treatment" effect. The specification in Eq. (3) can be redefined to analyze the sex offender "move out treatment." The housing observations in this specification would be restricted to those that occurred the year before or the year after the sex offender *moved out* of a neighborhood. Thus the estimate on the coefficient for "Proximity_dums" would be negative if sex offenders lower housing prices in these distances and the estimate on the coefficient for "Proximity_dums_post" would be approximately zero if housing prices immediately rebound once the sex offender moves out of the neighborhood.

4.4. Heterogeneity of the sex offender treatment for "predators"

The impact of the "type" of sex offender on housing prices may provide clues as to how households use the publicly available sex offender information and what they consider to be the sex offender housing attribute. If buyers are truly concerned about sexual offense risk to their household and they search out all publicly available information on the sex offender registry, then one would expect to see differential housing price impacts near offenders labeled as "predators." However, if buyers find it too costly to understand the different risks imposed by those offenders labeled as "predators," or if the choice to live near an offender is governed by a simple emotional yes/no heuristic based solely on the person being listed on the registry irregardless of the offender type, then there may not be a differential impact.

To test for heterogeneity in the treatment effect based on sex offender "type," a dummy variable for houses near sex offenders labeled as "predators" was created. It was interacted with the

[22] These various sets of spatial dummies will be compared in a cross-sectional analysis to illustrate the importance of adequate spatial control.

[23] If block groups provide adequate spatial control then the additional observations available in the housing subset #1 will help to identify the other relevant parameters in the model.

[24] The regression results reported in Section 6 suggest that it is the observations within 0.1 miles after the sex offender moved into the neighborhood that should be excluded from this specification check.

[25] This assumes there are no path dependencies such as a stigma effect on a previous sex offender residence. However, since the information made available on the FDLE website does not provide the previous addresses of offenders it is difficult to imagine that a stigma effect would exist.

Table 3
Cross-sectional regression results using full sample of housing data and sex offenders

Dep. Var. = lprice Variable	Full sample [1]	Full sample [2]	Full sample [3]	Full sample [4]	Full sample [5]
Within 0.1 miles	−0.411	−0.119	−0.120	−0.044	−0.029
	[0.003]**	[0.002]**	[0.002]**	[0.002]**	[0.002]**
Between 0.1 & 0.2 miles	−0.302	−0.069	−0.071	−0.028	−0.017
	[0.003]**	[0.002]**	[0.002]**	[0.001]**	[0.001]**
Constant	11.629	9.955	9.967	10.236	10.303
	[0.008]**	[0.010]**	[0.010]**	[0.011]**	[0.015]**
Year Dummies	X	X	X	X	X
House Controls		X	X	X	X
Tax District Dummies			X		
Census Tract Dummies				X	
Block Group Dummies					X
# of Spatial Dummies			81	247	759
Observations	189,491	189,488	189,485	189,488	189,488
R-squared	0.36	0.81	0.82	0.88	0.89

Notes: The house controls as described in paragraph 2 of Section 3.2 and summarized in Table 1 include: age, acreage, no. of bedrooms, no. of fixtures, source for heating and cooling, architectural type, and the "effective" area of the house. Standard errors are presented in brackets.
** Significant at 5%.

"Proximity_dums_post" variables in Eq. (3) to create the following specification,

$$\begin{aligned}\text{lprice} = {} & \beta + \alpha\text{Structural} + \phi\text{Spatial\_dums} \\ & + \delta\text{Year\_dums} + \theta\text{Proximity\_dums}^R \\ & + \omega\text{Proximity\_dums\_post}^R \\ & + \psi\text{Spatial\_linear\_trends} \\ & + \xi\text{Proximity\_dums\_post\_interact} + \varepsilon \qquad (5)\end{aligned}$$

where "Proximity_dums_post_interact" denotes both the appropriate interactions of the proximity dummies with the predator dummy and also the abduction interaction variables described earlier in this section.

5. Results

5.1. Cross-sectional results

To illustrate the problem of unobserved spatial heterogeneity in an application of this type, Table 3 presents estimates of a set of cross-sectional regressions. These regressions use the full sample of housing data (1996–2006) and the most proximate location of any sex offender that lived in a single family residence for at least six months. The results in column [1] are generated from a regression of the log of housing price on yearly dummy variables and variables that indicate if a house transaction occurred within 0.1 or between 0.1 and 0.2 miles of the nearest sex offender residential location. The estimates for the parameters on these variables suggest that sex offenders are clustered in areas with significantly lower housing prices. Column [2] reports the results for the same regression as in column [1] with housing characteristics available from the appraiser's database that were described in Section 3 included. Controlling for observed differences in housing characteristics reduces the size of the parameter estimates of the two proximity dummies significantly—from −0.41 to −0.12 for within 0.1 miles and from −0.30 to −0.07 for between 0.1 and 0.2 miles.[26]

Of course there are likely other spatially unobserved characteristics of housing that may be correlated with sex offender locations that should be included. Columns [3]–[5] provide the results of three regressions analogous to Eq. (1), each including a different set of "Spatial_dums" to control for unobserved spatial heterogeneity. Interestingly the estimates of the proximity to sex offender dummies shown in column [3] where 81 tax district dummies were included in the specification are approximately equal to those in column [2] and similar in magnitude to the estimates provided by Larsen et al. (2003). It appears that the sex offender clustering is occurring at a much lower spatial resolution than that provided by the 81 tax districts. The results in columns [4] and [5] indicate that the magnitude of these same coefficients are reduced substantially when the 247 census tract dummies or the 759 census block group dummies are included to control for unobserved spatial heterogeneity.

5.2. Specification check

To determine a specification that exploits both the temporal and spatial movements of sex offenders and provides adequate spatial control, a specification check described by Eq. (4) is reported in columns [1] and [2] of Table 4. Column [1] provides the results from this specification using housing subset #1 and block groups as the "Spatial_dums" (notice the house controls, census block-group dummies, and linear time trends for each block-group have been added). The housing transactions within 0.1 miles that occurred after the sex offender moved into the neighborhood were temporarily dropped. If this specification provides adequate spatial control then the coefficients on the within 0.1 miles and between 0.1 and 0.2 miles variables should be close to zero.[27] However, the coefficients on both variables suggest that there remains unobserved heterogeneity that reduces housing prices by approximately 2.2% for areas within 0.1 miles of where sex offenders will move to *in the future*.

In an effort to better control for unobserved heterogeneity, column [2] also presents regression results following Eq. (4). Now the model is based on the sample labeled housing subset #2 (only housing transactions within 0.3 miles of a sex offender) and the spatial dummies for housing observations within 0.3 miles of each individual sex offender location. The results from this regression (after again removing housing transactions within 0.1 miles of an offender, and adding the housing, 0.3 mile dummies and time trends) suggests that prices of houses between 0 and 0.1 miles

[26] These parameter estimates and all other parameter estimates on dummy variables reported in this section have been corrected using the Halvorsen and Palmquist (1980) correction for interpreting dummy variables in semilogarithmic equations.

[27] As will be seen later on, it appears that sex offender impacts are restricted to this localized area.

Table 4
Primary regression results

Dep. Var. = lprice	"Near" sex off. obs. removed	"Near" sex off. obs. removed	Move in treatment	Move in treatment & Predator interaction	Move in treatment & Abduct. interaction	Move out treatment
Housing subset:	Housing subset #1	Housing subset #2	Housing subset #2	Housing subset #2	Housing subset #2	Housing subset #2
Variable	[1]	[2]	[3]	[4]	[5]	[6]
Within 0.1 miles	−0.022	0.001	0.000	0.001	0.000	**−0.018**
	[0.007]**	[0.008]	[0.009]	[0.009]	[0.009]	[0.010]*
Between 0.1 & 0.2 miles	−0.017	−0.007	−0.008	−0.010	−0.009	−0.011
	[0.006]**	[0.006]	[0.008]	[0.007]	[0.007]	[0.008]
Within 0.1 & sold after sex off. moved in			**−0.023**	**−0.026**	**−0.025**	0.009
			[0.012]*	[0.013]**	[0.013]*	[0.014]
Between 0.1 & 0.2 & sold after sex off. moved in			0.001	0.003	0.000	−0.006
			[0.006]	[0.007]	[0.007]	[0.011]
Within 0.1 miles of predator				−0.011		
				[0.031]		
Between 0.1 & 0.2 miles of predator				0.023		
				[0.049]		
Within 0.1 & sold after predator moved in				0.036		
				[0.026]		
Between 0.1 & 0.2 & sold after predator moved in				−0.033		
				[0.040]		
Within 0.1 & sold after abductions occurred					0.008	
					[0.021]	
Between 0.1 & 0.2 & sold after abductions occurred					0.011	
					[0.015]	
Constant	6.299	13.578	12.935	12.935	12.923	11.695
	[0.140]**	[1.246]**	[1.149]**	[1.149]**	[1.149]**	[0.027]**
Year Dummies	X	X	X	X	X	X
House Controls	X	X	X	X	X	X
Block Group Dummies	X					
Block Group Linear Trends	X					
Clustering at Block Group	X					
0.3 Mile Dummies		X	X	X	X	X
0.3 Mile Linear Trends		X	X	X	X	X
Clustering at 0.3 Mile		X	X	X	X	X
# of Spatial Dummies	619	275	275	275	275	268
Observations	115,532	5518	5923	5923	5923	4278
R-squared	0.90	0.93	0.93	0.93	0.93	0.92

Notes: The house controls as described in paragraph 2 of Section 3.2 and summarized in Table 1 include: age, acreage, no. of bedrooms, no. of fixtures, source for heating and cooling, architectural type, and the "effective" area of the house. Robust, clustered standard errors are presented in brackets.

* Significant at 10%.

** Significant at 5%.

distance from the future sex offender residence are no different from the houses that are between 0.1 and 0.2 or 0.2 and 0.3 distance from the future sex offender location. This appears to adequately remove unobserved spatial heterogeneity and provide a better specification to explain the causal impact of sex offender relocations on housing prices.

5.3. Primary results

Table 4, columns [3]–[5], presents regression results (using the preferred 0.3 mile control specification) that exploit both the temporal and spatial movements of sex offenders. Column [3] corresponds with the specification developed in Eq. (3). The data used in this specification includes housing transactions within 0.1 miles of the sex offender location. The results suggest that after including the various controls and prior to the sex offender moving into the neighborhood, there is no difference between housing prices in the 0–0.1, 0.1–0.2, and 0.2–0.3 bands surrounding the sex offender location. However after the sex offender moves into the neighborhood, houses within 0.1 miles of the sex offender location appear to decline in price by approximately 2.3%. This is significant at the 6% level even after clustering the standard errors for the houses within 0.3 miles of each individual sex offender.[28]

Column [4] evaluates the potential for heterogeneity of the sex offender impact for houses near sex offenders labeled as "predators" using the specification in Eq. (5). The coefficient on the variable indicating houses within 0.1 mile that sold after a predator moved in is positive but insignificant. The coefficient on the corresponding variable for houses between 0.1 and 0.2 miles that sold after a predator moved in is negative and also insignificant. Therefore it does not appear that there is a differential impact for those houses near sex offenders with the "predator" label. Nonetheless some caution must be exercised in interpreting this result given the noise in the estimates.

Column [5] provides the results from the specification in Eq. (5) that checks for a differential impact of sex offenders on housing prices after the abductions of the Florida children. Both interaction variables are insignificant. This suggests that the abductions and the ensuing media coverage did not have a significant influence on home purchase decisions and are not driving the results in the analysis.

5.4. Robustness check: The "move out" treatment

As discussed earlier, the interpretation of the "move in treatment" estimate as the causal impact of sex offender locations on housing prices would be enhanced if housing prices rebounded after the sex offender *moved out* of the neighborhood. Column [6] presents the estimates of a regression that again uses the preferred specification and housing sample #2. The sample was restricted to

[28] Clustering allows the regression errors for observations within 0.3 miles of each sex offender location to be correlated.

those housing transactions that occurred the year before or the year after the offender moved *out* of the house. As a result, in this specification the within 0.1 miles variable indicates those houses within a tenth of a mile of a sex offender location the year after the sex offender *moved out* of the neighborhood. The estimated coefficient for this variable is statistically significant at the 10% level and suggests that the year before the sex offender moved out of the neighborhood houses within a tenth of a mile were 1.8% lower than houses further away. The magnitude of this estimate is similar to the estimate of 2.3% reduction for the "move in treatment." The estimate on the between 0.1 and 0.2 miles variable is also negative but is not significant by conventional standards. The estimates on the within 0.1 miles and sold after the sex offender moved in variable suggests that after the sex offender moved out of the neighborhood, housing prices rebounded to where they appear to be no different than the prices of houses in the 0.1 to 0.2 mile range or the 0.2 to 0.3 mile range which is the omitted distance category.

5.5. Discussion of results

A pervasive concern in cross-sectional studies employing the hedonic method in the housing market is omitted variable bias. The cross-sectional results provided here illustrate the nature of the problem. When the spatial fixed effect areas are large (e.g. tax district dummies), the impact of sex offender locations on housing prices appears to be grossly overstated. However, as the size of the fixed effects decreases (e.g. census tract and block group dummies), the estimate begins to converge towards the quasi-random experiment estimates. These results suggest the need for a cleaner identification strategy to determine the causal impact of sex offender locations on housing prices.

Sex offender movements provided variation that could be exploited as sets of quasi-random experiments. An identification strategy that used the sex offender movements was developed to estimate the impact of the residential locations of sex offenders on housing prices. It was shown that using sex offender specific spatial dummies appears to adequately reduce the unobserved spatial heterogeneity. By exploiting this spatial specification together with a housing sample that is bounded to the year before and the year after the first (and only) sex offender ever to have lived in a neighborhood moves in, it is estimated that housing prices within a tenth of a mile are reduced by approximately 2.3%. This percentage change translates into a $3500 reduction for the average priced house in the sample.

The information on sex offender movements also provides the opportunity to analyze the reversal of a neighborhood's sex offender "move in treatment"—the "move out treatment." Rarely outside of a controlled experiment is there the opportunity to witness such a clean reversal of treatments as occurs with the sex offender movements in this analysis. Verifying that housing prices rebound after a sex offender moves out of a neighborhood greatly strengthens the evidence that the "move in treatment" estimates can be interpreted as causal and therefore provides a robustness check. Using the same spatial specification as in the move in treatment analysis, it was found that the year prior to the offender moving out of the neighborhood, housing prices within a tenth of a mile of an offender are still depressed by 1.8% but *immediately rebound* after the sex offender moves out of the neighborhood.

The estimated impact of sex offender locations on housing prices does not appear to be heterogeneous to whether or not the nearest offender is labeled a "predator." This is a surprising result. One would expect if households were fully informed about the sex offender risk information provided by the registry that a higher premium would be placed on living far from predators than from sex offenders. It appears that households are treating sex offender risk discretely in their subjective risk calculations. Whether a person is listed on the registry or not appears to be weighted by households more heavily than the future risk that the person might impose on a neighborhood. This may signal that households are misinterpreting the information on the registries, but some caution must be exercised with this conclusion given that this estimate is somewhat noisy. Furthermore, the results are not sensitive to the widespread media coverage of the child abduction that occurred in Florida.

6. Conclusion

Megan's Law, which made information on sex offender registries publicly available, has been controversial. The law effectively places a scarlet letter on registered sex offenders. Part of the controversy has surrounded two important questions that relate to both the benefits and costs of this legislation: (i) "Do households access and use information on the sex offender registries?," and (ii) "Do households misinterpret the information available on the registries?" The analysis in this paper suggests that households do use the information because housing prices within a localized area decrease when a sex offender moves in and then rebound when a sex offender moves out. However, given that there is no heterogeneity in the estimated impact for houses near offenders that have been labeled as "predators" suggests that households may be misinterpreting the information made available to them.

A complete benefit-cost analysis of the impact of Megan's Law is beyond the scope of this study. However, the results do suggest that providing the information on sex offender registries to the public has value. With information on the locations of sex offenders, informed households are better able to sort themselves. Those households that place a premium on avoiding contact with offenders (e.g. households with young children) can choose not to purchase a house near a child molester. This type of crime risk reduction value to the marginal buyer is certainly what is partially being reflected by the reduction in housing prices after a sex offender moves into a neighborhood. However, these benefits are partially offset by the losses to the owners of homes near sex offenders. Furthermore, part of the estimated impact may not reflect objective crime risk changes to households, but simply their "fear of crime" that is exacerbated by the information. By releasing the sex offender information, the government has drawn the public's attention to thousands of localized disamenities.

Besides furthering the understanding about the role of Megan's law on household decision making, this study also adds to several broader literatures. First, the analysis adds to the literature on the use of quasi-random experiments in the housing market such as those done by Black (1999) and Chay and Greenstone (2005). A key feature of this paper was the development of an identification strategy around a unique quasi-random experiment that would help to eliminate elements of omitted variable bias that have plagued cross-sectional hedonic analyses. A message suggested by the previous literature and reinforced by this study, is that quasi-random experiments combined with high resolution housing data provide many opportunities to learn more about how households respond to a wide range of spatial amenities and disamenities.

This work also relates to at least two other important literatures in economics. For example, it relates to efforts to further understand households valuation for crime reduction (e.g. Thaler, 1978, Cullen and Levitt, 1999, Katz et al., 2001, Kuziemko and Levitt, 2004, and Gibbons, 2004). One of the major difficulties in this literature has been to overcome the endogenous relationship between household actions and crime. The results provided here add to this literature by providing estimates on how households and housing prices react to information on one specific crime—

sexual offenses—using a plausibly quasi-random source of crime variation. The results from this study also relate to recent efforts to understand whether or not public information disclosure programs can influence household decision-making. For example, Jin and Leslie (2003) found that government mandated hygiene grade cards posted in restaurant windows influenced both consumer and firm behavior. The evidence presented here suggests that households react to a type of *neighborhood* grade card; sex offender registries.

There are several avenues for future research. First, although housing prices were found to react to publicly available information about sex offenders, this does not necessarily mean that households are "fully informed" as traditionally assumed by the hedonic model. Further research on the mechanisms by which households become informed and whether this information clarifies the objective sex offense risks to the household, or causes a psychic "fear of crime" would be useful. Second, the analysis in this study focused on single-family residential areas where only one sex offender had ever resided. Analyzing the impact of areas that contain multiple sex offenders would also be an important contribution by providing estimates applicable to the types of neighborhoods that contain multiple offenders. Nevertheless, looking at areas with multiple sex offenders makes identification of the house price effect more challenging due to the potential for strategic interactions and agglomeration effects, which is the reason for leaving this exercise to future research. Finally, work on how *sex offenders* react to being registered would be an important parallel to the work presented here.

Appendix Table A.1
Housing summary statistics for areas with different #'s of sex offenders

Variable	Description	Mean	Median	Standard deviation	Minimum	Maximum	Observations
Panel A: All single family transactions from 1996–2006							
price	sale price of property	172,123.50	143,900.00	114,601.60	5700.00	2,863,299.00	189,285
age	age of property in years	19.36	13.00	20.71	0.00	106.00	189,285
acreage	acreage of lot	0.30	0.19	0.82	0.00	123.97	189,285
eff_ar	the "effective" square footage	2065.16	1953.00	779.88	105.00	10,749.00	189,285
onetenth_mile	within 0.1 miles of S.O.	0.16	0.00	0.37	0.00	1.00	189,285
twotenth_mile	between 0.1 and 0.2 miles of S.O.	0.20	0.00	0.40	0.00	1.00	189,285
Panel B: Transactions in areas with 0 S.O.'s within 0.3 miles							
price	sale price of property	209,584.00	175,300.00	129,145.30	5700.00	2,863,299.00	94,450
age	age of property in years	12.08	5.00	15.99	0.00	105.00	94,450
acreage	acreage of lot	0.37	0.21	1.09	0.00	123.97	94,450
eff_ar	the "effective" square footage	2386.00	2264.00	775.43	191.00	8687.00	94,450
onetenth_mile	within 0.1 miles of S.O.	0.00	0.00	0.00	0.00	0.00	94,450
twotenth_mile	between 0.1 and 0.2 miles of S.O.	0.00	0.00	0.00	0.00	0.00	94,450
Panel C: Transactions in areas with only 1 S.O. within 0.3 miles							
price	sale price of property	161,061.90	138,500.00	96,467.63	6000.00	2,200,000.00	33,947
age	age of property in years	18.21	14.00	18.28	0.00	106.00	33,947
acreage	acreage of lot	0.26	0.19	0.46	0.00	25.07	33,947
eff_ar	the "effective" square footage	2001.31	1891.00	664.20	317.00	6818.00	33,947
onetenth_mile	within 0.1 miles of S.O.	0.15	0.00	0.36	0.00	1.00	33,947
twotenth_mile	between 0.1 and 0.2 miles of S.O.	0.36	0.00	0.48	0.00	1.00	33,947
Panel D: Transactions in areas with 2–5 S.O.'s within 0.3 miles							
price	sale price of property	134,018.40	120,000.00	75,487.02	7000.00	1,600,000.00	37,711
age	age of property in years	25.41	22.00	20.33	0.00	106.00	37,711
acreage	acreage of lot	0.24	0.18	0.41	0.01	50.00	37,711
eff_ar	the "effective" square footage	1769.37	1672.00	612.33	105.00	10,749.00	37,711
onetenth_mile	within 0.1 miles of S.O.	0.31	0.00	0.46	0.00	1.00	37,711
twotenth_mile	between 0.1 and 0.2 miles of S.O.	0.44	0.00	0.50	0.00	1.00	37,711
Panel E: Transactions in areas with 5–10 S.O.'s within 0.3 miles							
price	sale price of property	102,350.40	90,000.02	52,941.40	6500.00	924,999.80	16,769
age	age of property in years	38.16	40.00	22.40	0.00	105.00	16,769
acreage	acreage of lot	0.18	0.16	0.12	0.02	4.20	16,769
eff_ar	the "effective" square footage	1374.62	1310.00	406.66	263.00	8386.00	16,769
onetenth_mile	within 0.1 miles of S.O.	0.53	1.00	0.50	0.00	1.00	16,769
twotenth_mile	between 0.1 and 0.2 miles of S.O.	0.42	0.00	0.49	0.00	1.00	16,769
Panel F: Transactions in areas more than 10 S.O.'s within 0.3 miles							
price	sale price of property	85,414.37	78,000.02	42,643.82	6000.00	425,000.10	6408
age	age of property in years	48.05	48.00	25.14	0.00	106.00	6408
acreage	acreage of lot	0.15	0.14	0.07	0.01	1.86	6408
eff_ar	the "effective" square footage	1222.07	1202.00	338.85	256.00	4235.00	6408
onetenth_mile	within 0.1 miles of S.O.	0.75	1.00	0.43	0.00	1.00	6408
twotenth_mile	between 0.1 and 0.2 miles of S.O.	0.24	0.00	0.43	0.00	1.00	6408

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