American Economic Review 2008, 98:3, 1103–1127
http://www.aeaweb.org/articles.php?doi=10.1257/aer.98.3.1103

Estimates of the Impact of Crime Risk on Property Values from Megan's Laws

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Crime is predominantly a local issue. The majority of both violent and nonviolent offenses takes place less than one mile from victims' homes, and most government expenditures on police protection are local (Bureau of Justice Statistics 2004; Census of Governments 2003). In response to crime risk, residents generally have two options: they can vote for anti-crime policies, or they can vote with their feet. When individuals exercise the latter option, local response to crime will be observed in the housing market. This may be particularly salient for crime, since individuals can reduce their exposure without moving great distances, and empirical evidence on urban flight supports this notion (Julie B. Cullen and Steven D. Levitt 1999).

Understanding the relationship between property values and local crime risk is useful for measuring the willingness of individuals to pay to reduce their exposure to crime risk. This, in turn, can help determine the appropriate level of public expenditures that reduce crime, such as police services. A number of papers have documented an inverse relationship between property values and local crime rates. In one of the earliest studies, Richard Thaler (1978) finds a negative relation between property crimes per capita and property values. His estimates imply that a one-standard-deviation increase in the incidence of property crime reduces home values by about 3 percent. A more recent study by Steve Gibbons (2004) finds a decrease in property values of 10 percent for a one-standard-deviation increase in property crime.

These studies, however, face potential omitted variable problems in both the cross section and time series. In the cross section, crime rates are likely to covary with other geographic amenities for which researchers cannot adequately control. Over time, crime rates may change as the composition and characteristics of neighborhoods change. Reductions in crime levels may correspond to other changes that increase the value of property located in a particular neighborhood.

In this paper, we combine data from the housing market with data from sex offender registrations to estimate individuals' valuation of living in close proximity to a convicted criminal. By exploiting both the timing of move-in and the exact locations of sex offenders, we can improve on past estimates of the impact of crime risk on property values. The exact location of these offenders allows us to exploit variation in the threat of crime within small, relatively homogenous groupings of homes. The timing of a sex offender's arrival allows us to confirm the absence of substantive preexisting differences in property values and to control for the remaining minor differences.

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Our study is the first to exploit both intertemporal and cross-sectional variance in the presence of an offender, but not the first to exploit the cross-sectional variance alone. James E. Larsen, Kenneth J. Lowrey, and Joseph W. Coleman (2003) examine the cross-sectional relationship between property values and proximity to sex offenders using a single year of data from Montgomery County, Ohio. They find that houses sell for 17 percent less within a tenth of a mile of an offender's home, and find significant differences in price up to a third of a mile from offenders' locations. Although their study is similar to ours in the empirical question it addresses, their empirical strategy suffers from the same potential biases mentioned above. Indeed, applying their empirical strategy on our data, we estimate that property values are 19 percent lower near sex offenders. However, most of this difference reflects the fact that sex offenders tend to live in areas where house prices are lower, on average.

We find that prices of homes near sex offenders decline considerably following an offender's arrival in the neighborhood. We estimate that the average price of homes sold closest to the offender declines by roughly 4 percent (about $5,500). However, this effect is extremely localized and dissipates quickly with distance. We estimate that homes directly adjacent to an offender decline in value by 12 percent, but we find no evidence of any impact on homes located more than a tenth of a mile away from offenders' locations.

In a paper subsequent to ours, Jaren C. Pope (2007) investigates the same issues using data on sexual offenders in Hillsborough County, Florida, and finds similar results. He estimates that offenders cause a reduction in the sale prices of homes of 2.3 percent (or $3,500, given the average value of homes in the area) within 0.1 miles of an offender's location. Unlike our dataset, his includes residential histories of offenders, allowing him to observe their departures from neighborhoods, as well as their arrivals. The estimated decline in sale prices that occurs with an offender's arrival disappears after the offender's departure, providing further support that the impact of offenders on local property values is causal.

This paper is organized as follows. In the next section, we describe federal and North Carolina sex offender registration laws. In Section II, we describe the data used in our study. In Section III, we describe our empirical methodology, present graphical evidence on the impact of sex offenders' arrivals, and describe the model we use for formal statistical analysis. We present our empirical results in Section IV. We use these results to estimate victimization costs of sexual offenses in Section V, and we conclude in Section VI.

I. Sex Offender Registration Laws and Databases

In 1994, the Jacob Wetterling Crimes Against Children and Sexually Violent Offender Registration Program required all states to maintain a registry of convicted sexual offenders. [1] An amendment to the Wetterling Act in 1996, dubbed "Megan's Law," required public notification of the location and description of convicted sex offenders. This amendment was motivated by the rape and murder of a seven-year-old girl, Megan Kanka, by a neighbor who had been convicted in 1981 for an attack on a five-year-old child and an attempted sexual assault on a seven-year-old. According to the National Center for Missing and Exploited Children, there were over 600,000 registered sex offenders in the United States as of February 2007.

By imposing requirements on a class of individuals previously convicted of a crime after they have completed their sentences, these laws represent a significant change in the legal practice of dealing with convicted criminals after they have been released from prison. Megan's Laws have been extremely controversial and subjected to numerous legal challenges. Two such challenges

[1] 42 U.S.C. § 14071 (2000). Jacob Wetterling was abducted in Minnesota in 1989; neither he nor the perpetrators were ever found.

reached the Supreme Court, but in both cases the court upheld the laws as a legitimate civil regulation in response to the recidivism threat imposed by sex offenders on the communities in which they live (*Connecticut Department of Public Safety et al. v. Doe*, 538 U.S. 1 (2003), *Smith et al. v. Doe,* 538 U.S. 84 (2003)).

While federal law requires registration of offenders and community notification, states are given significant latitude in their implementation of these provisions. The registries must include a range of identifying information, including offenders' names, addresses, and photographs. Registries are not required, however, to contain information on when an offender moved into his/her current address. To the best of our knowledge, North Carolina, Florida, and Montana are the only states that provide information on offenders' move-in dates.

A. *North Carolina Sex Offender Registration*

The North Carolina sex offender registration law was adopted in 1996 and is similar to many of the registration laws that exist in other states. All individuals convicted on or after January 1, 1996, of a sexual offense are required to register, as are sexual offenders released from prison on or after January 1, 1996, even though their convictions took place prior to this date. Individuals are required to register for ten years after being released from prison, and the law applies equally to individuals convicted in other states who move to North Carolina.

Sex offenders must register within ten days of being released from prison and, if they move, they must notify the registry within ten days. Failure to register an address is a felonious offense and cause for revocation of parole. In addition to these reporting requirements, the state is required to verify offenders' addresses. A postcard that cannot be forwarded is periodically mailed to each sex offender and, if this card is not returned, the local sheriff is required to verify whether the individual still resides at the registered address. If the offender is no longer living there, he/she may be subject to criminal penalties.

Information in the sex offender registry is provided to citizens via a Web-based interface that is maintained by the State Bureau of Investigation's Division of Criminal Information. The registry reports each offender's current address, zip codes of past addresses, the offense for which the individual was convicted, a picture of the individual, and identifying information such as height, weight, race, gender, distinguishing characteristics, hair color, and eye color.

Statistics from the North Carolina registry show that compliance with the sex offender registration law is very high.[2] Between January 1, 1996, and March 9, 2003, North Carolina released a total of 8,287 individuals who would be required to register. Of these offenders, 1,007 (12 percent) had reported moving to another state. Of those remaining, 103 (1.4 percent) had failed to register their addresses.[3]

II. Data Sources

Our analysis is based upon three sets of data regarding the locations of sex offenders, the locations and characteristics of properties in Mecklenburg County, and property sales. January 2005 data on registered sex offenders in North Carolina were provided by the North Carolina Department of Justice (NCDOJ). This dataset contains information on offenders' basic demographics, type of offense, date of offense, current address, and date of registration at current

[2] Available at http://ncregistry.ncsbi.gov/(S(ptqa4wifhwwagd55nnxdvc45))/sexoffen/SEXMar12_2003.pdf.

[3] Noncompliance by offenders convicted in other states but residing in North Carolina is impossible to measure. However, the in-flows and out-flows of reporting offenders are roughly equal; as of January 2005, the fraction of registered offenders convicted outside of North Carolina was 10 percent.

address. Because of the strict provisions governing timely registration in North Carolina, the registration date is a close approximation of the date an offender moved to their current location.

In January of 2005, there were approximately 9,200 registered sex offenders in North Carolina. In Mecklenburg County, which contains the city of Charlotte, there were 518 registered offenders, the most of any county in the state. The vast majority of all sexual offenses committed by registered offenders fall into a small number of categories. Sixty-eight percent of crimes by sex offenders in Mecklenburg County are classified as Indecent Liberty with a Minor (typically referred to as "child molestation"), 11 percent are Sexual Offense (sexual acts other than rape, where force or violence is involved), 10 percent are Rape, and 6 percent are Attempted Sex Offense or Attempted Rape. The statewide percentages are very similar.[4]

Our second source of data comes from the Mecklenburg County division of Property Assessment and Land Record Management. These assessment data contain Geographical Information Systems (GIS) information on all real estate parcels in the county as of March 2005. With GIS information, we can measure the distance in feet between the centers of any two parcels. The assessment data also provide comprehensive physical characteristics for each parcel (e.g., number of rooms, square footage, etc.). The Mecklenburg County Tax Assessor's Office also separates parcels into 1,004 different "neighborhoods" that have similarly valued properties. The relative homogeneity of property within neighborhoods allows us to control for unobservable fixed and time-varying characteristics at the neighborhood level. These neighborhoods are much smaller than census tracts (there were 144 tracts in Mecklenburg County in 1990) or even census block groups (there were 373 block groups in Mecklenburg County in 1990) and encompass just 0.47 square miles on average.

In order to measure the proximity of property sales to offender locations, we matched offender addresses from the NCDOJ data to addresses in the assessment data. Of the 518 offenders registered in the county, 66 could not be matched with a parcel in the assessment data.[5] Additionally, 56 offenders were registered as living in a jail or halfway house, and we drop them from our analysis. We then matched single family homes to the first offender to arrive within a three-tenth-mile radius. We chose 0.3 miles based on the Louisiana law requiring sex offenders to inform all neighbors living within this distance from their home of their presence. We have found similar results using a 0.25 mile radius. Homes matched to an offender were given an "arrival date" based on the date the offender registered his/her address and a "distance to offender" based on the distance to the offender's parcel. In this way, each offender creates an "offender area" of about 0.28 square miles—smaller than the average size of the neighborhoods defined in the assessment data.

We merge the matched offender-assessment data with property sales from January 1994 to December 2004, provided by the Mecklenburg County Property Assessment and Land Record Management Office.[6] Prices are normalized to December 2004 dollars using the monthly South

[4] One might suppose that the danger to neighbors, and thus the impact on house prices, might vary significantly across offenders, depending on their criminal histories. Although some crimes committed by registered offenders suggest that they pose less danger to neighbors (e.g., 2 percent of convictions were for incest), these were too rare for us to attempt to estimate heterogeneous impacts of offenders by crime committed.

[5] Thirty-five had an unknown street address, and 31 listed addresses that did not match a parcel in the assessment data. Of the matched offenders, nearly all address matches were exact. The only exceptions were four offenders whose street number could not be matched but whose street name, city, and zip code did match and whose street numbers seemed reasonably close to another parcel. For example, an offender who claimed to live on "838 Everett Place" was matched to "836 Everett Place."

[6] We were able to match 96 percent of sales with an address in the assessment data. Thirty-three offenders with matched addresses were not matched with any single family home sales within 0.3 miles of their location. We drop a small number of irregular sales entries (e.g., sales that took place fewer than three days following another sale of the same parcel). Parcels in which the registered offenders reside have also been dropped from the sample.

Urban CPI, and we drop sales outside the range of $5,000 to $1 million (i.e., the first and ninety-ninth percentile of the price distribution), giving us a total of 169,577 sales.

We limit our analysis to offenders who had lived in their current location for one year or more and we examine sales that occur within a four-year window surrounding offenders' arrivals (i.e., two years prior and two years after).[7] These sample limitations ensure that we observe (roughly) equal prior and post periods for each of the offenders. (We find similar results if we include offenders who had lived in their current location for at least six months.) Ultimately, we examine 9,086 sales that occurred within a 0.3 mile radius of 174 registered offenders and took place within two years of the offenders' arrivals; 1,344 of these 9,086 sales occurred within 0.1 miles of an offender location.

Table 1 provides summary statistics of the various parcels that are sold in Mecklenburg County during the period of interest. The first column provides information on all sales in the county and the second column shows the sales that occur within 0.3 miles of where a sex offender either has located or will eventually locate. This demonstrates the importance of the localized data we use in this analysis, because the areas in which sex offenders locate have smaller houses that sell for less money. In other words, sex offenders, on average, move to the cheaper neighborhoods of Mecklenburg County. Column three provides a hedonic decomposition of the log of the sale price of homes within 0.3 miles of an offender to gauge the importance of the various characteristics. The regression also includes dummy variables for the composition of the house's exterior and offender area by year fixed effects. These control variables are included in our subsequent regression analysis, but, for simplicity, we do not report these coefficients in the tables below.

III. Empirical Methodology

Choice of residence represents choice of labor market, school quality, social group, environment, etc., in addition to choice of house characteristics. The demand for homes in areas with particular characteristics is therefore also a measure of individuals' preferences regarding all of the local factors that affect economic outcomes. A large number of studies have examined the relation between property values and location-specific (dis)amenities, such as school quality, pollution, crime, and property taxes.[8]

The difficulties in identifying the hedonic price function for local (dis)amenities are well known. A major obstacle is that variation in the local amenity may be correlated with unobservable factors (Timothy J. Bartik 1987; Dennis Epple 1987). In addition, if the long-run supply of housing is elastic, then changes in demand for local property will, in equilibrium, show up in quantities, not prices (Matthew Edel and Elliott Sclar 1974). Thus, an effective empirical

[7] Twenty-nine offenders have a missing move-in date, and we exclude them from our analysis. Seven offenders had no sales occur within a 0.3 mile radius during the four-year window around their arrival date. One hundred fifty-three offenders had lived in their current address for less than one year (including 38 offenders who were released from prison less than a year prior to the end of our sales data). This leaves us with 174 offenders. Thus, it is possible that our estimates might not be representative of the effects of the average sex offender moving into a neighborhood if offenders who move frequently would cause different changes in property values than offenders who choose to live in a single place for an extended period of time. Unfortunately, measuring the impact of itinerant offenders is not possible, given their short durations and our reliance on sales data.

[8] Some recent examples are Sandra E. Black (1999), Peter F. Colwell, Carolyn Dehring, and Nicholas Lash (2000), Allen K. Lynch and David W. Rasmussen (2001), Linda T. M. Bui and Christopher J. Mayer (2003), Lucas W. Davis (2004), Steve Gibbons (2004), David N. Figlio and Maurice E. Lucas (2004), and Kenneth Y. Chay and Michael Greenstone (2005).

Table 1—Characteristics of Homes Sold in Mecklenburg County, 1994–2004

	All parcels	Within 1/3 mile of offender	
	Mean (*Standard deviation*)	Mean (*Standard deviation*)	Marginal effect in price regression[1]
Sale price	2.048	1.438	
($100,000)	(1.324)	(0.848)	
Square footage	2.075	1.620	0.266
(1,000 square feet)	(0.880)	(0.595)	(0.012)*
Quality rating (1 to 6)	3.251	3.066	0.047
	(1.208)	(0.979)	(0.006)*
Age (in years)	10.347	16.322	−0.008
	(12.090)	(12.815)	(0.001)*
Bedrooms	3.327	3.061	0.028
	(0.648)	(0.566)	(0.010)*
Bathrooms	2.018	1.737	0.028
	(0.592)	(0.539)	(0.008)*
	Percentage	Percentage	
Air-conditioned	93.3%	84.6%	0.091
			(0.011)*
Sold in year built	34.2%	19.5%	−0.100
			(0.013)*
Story height			
1 story	39.4%	56.5%	
1.5 stories	6.4%	5.4%	0.033
			(0.016)*
2 stories	49.1%	32.5%	0.031
			(0.010)*
3 or more stories	1.6%	0.6%	0.059
			(0.048)
Split level	1.1%	1.4%	0.009
			(0.028)
Other	2.4%	3.5%	0.020
			(0.021)
Quality tier			
Tier 1	0.7%	2.1%	
Tier 2	75.7%	89.7%	0.242
			(0.028)*
Tier 3	17.4%	6.6%	0.508
			(0.036)*
Tier 4	4.8%	1.2%	0.543
			(0.056)*
Tier 5	1.1%	0.3%	0.671
			(0.099)*
Tier 6	0.4%	0.1%	1.058
			(0.166)*
Sample size	170,239	9,092	9,086
R^2			0.75

[1]Estimated for parcels sold in offender areas by regressing log (sale price) on listed variables and offender area by year fixed effects.

strategy for uncovering capitalization might examine short-run changes in property values due to arguably exogenous changes in local (dis)amenities.[9]

[9] Though we focus on changes in property values in the short run, long-run impacts are also important. For example, neighbors may perceive more risk over time if the threat of the offender becomes more well known; or, perhaps, if an offender has lived in a community for a long time without reoffending, the perceived risk might diminish. Nevertheless, long-run price responses are more difficult to identify, since changes in neighborhood characteristics will affect the quality and quantity of housing, not just prices, when supply can adjust (Thomas J. Kane, Douglas O. Staiger, and

Figure 1. An Offender Area and Surrounding Neighborhoods

Note: X marks the center of the offender's exact location. The surrounding circle marks all parcels within one-quarter mile. Neighborhoods are distinguished by shades of gray. Parcels within a neighborhood are usually, but not necessarily, contiguous.

Sex offenders, like all individuals, are likely to choose a neighborhood based on their income and preferences. As illustrated in Table 1, sex offenders do tend to move to areas that, on average, have lower property values. The covariance of sex offender location and both observable and unobservable neighborhood characteristics makes it difficult to identify the effect of sex offenders on property values by comparing areas with sex offenders to areas without them.

Rather than compare aggregated areas, however, we know the specific locations in which sex offenders have chosen to live and the dates of their arrivals. The specific location data allow us to compare the value of home sales within very small areas in which the housing stock is more homogenous than in normal aggregate comparisons. This notion is illustrated by Figure 1, which shows the location of one of the sex offenders in our data, the surrounding parcels grouped by neighborhood, and a circle that outlines all parcels located within 0.3 miles of the offender's location. The offender's particular choice of residence is extremely close to some houses in the neighborhood and farther from others. Moreover, houses in adjacent neighborhoods vary in their distance from the offender's location.

Gavin Samms 2003). For example, the continued presence of a sex offender in a neighborhood may reduce investment in existing housing and deter development of new housing. High mobility among sex offenders also limits the extent to which a long-run analysis would be possible—many of the offenders in our data have lived in their current residence for under two years.

Relying on cross-sectional variation alone, however, would be problematic if property characteristics vary within these small areas in ways that are unobservable to the researcher. If, for example, sex offenders move into the cheapest property available in a given area (e.g., next to a local "eyesore" like the home of a resident who has allowed his or her property to deteriorate significantly, the artist who decided to paint his house fluorescent pink, or the local mechanic who has turned his or her front yard into a garage), then variation in the sale value of property around the sex offender's home may reflect distaste for the location to which the offender moved, rather than distaste for living near the offender.

This is a constant concern in the literature that attempts to exploit variation in housing prices along geographic administrative boundaries (see Patrick Bayer, Fernando V. Ferreira, and Robert McMillan 2004). We therefore examine within-neighborhood variation in property values shortly before and after the arrival of a sex offender. This allows us to control for preexisting differences in property values between homes closer to the offender and homes farther from the offender within the same neighborhood. This framework would be compromised only if sex offenders consistently moved into properties near which a localized disamenity was likely to emerge. This possibility seems unlikely when one considers that the nature of the search for housing is also a largely random process at the local level. Individuals may choose neighborhoods with specific characteristics, but, within a fraction of a mile, the exact locations available at the time individuals seek to move into a neighborhood are arguably exogenous (Bayer, Stephen L. Ross, and Giorgio Topa 2004).

One important caveat in our methodology is that, like all such studies, we can observe prices only for houses that sell. If the composition of individuals buying or selling a home changes with an offender's arrival, for example, the prices that we observe may not be indicative of the average willingness to pay not to live near a sexual offender in these neighborhoods. It is possible that households that sell their homes after the arrival of a sex offender are those with a higher than average willingness to pay to avoid the risks posed by the offender. If these families set lower reservation prices, this will tend to lower observed sales prices.[10] By the same intuition, however, households that buy these homes would be more likely to have a lower than average willingness to pay to avoid offenders, and would not demand a large discount. Thus, it is unclear whether selection/composition of buyer or seller characteristics would lead us to overestimate or underestimate the average willingness to pay by other types of households. This issue is present in all empirical work on local amenities and property values, as well as in the literature on compensating differentials in the labor market. Unfortunately, without data on buyer or seller characteristics, we cannot examine this issue in our context.[11]

A. *Graphical Evidence*

If living close to a sex offender has a negative impact on property values, we should see prices of homes near the offender's location fall subsequent to the offender's arrival. Moreover, we should observe a larger impact on homes closest to the offender. Figure 2A shows the price

[10] For empirical evidence on the impact of seller reservation prices and seller characteristics on sales prices, see David Genesove and Mayer (1997, 2001) Michel Glower, Donald R. Haurin, and Patric H. Hendershott (1998), John P. Harding, Stuart S. Rosenthal, and C. F. Sirmans (2003), and William Goetzmann and Liang Peng (2006).

[11] Although data on actual buyer and seller characteristics are unavailable, demographic data from the decennial census could be used to proxy for the demographic characteristics of households affected by the offenders in our sample. We explored this possibility by estimating regressions with interactions between offender proximity and demographic characteristics (i.e., fraction of households with children under 18, median household income, and fraction of households where both parents are employed) at the block-group level. While we did not find statistically significant interaction effects, it is important to recall that block-groups are significantly larger than the neighborhoods in our data, and thus may not accurately measure the characteristics of relevant households.

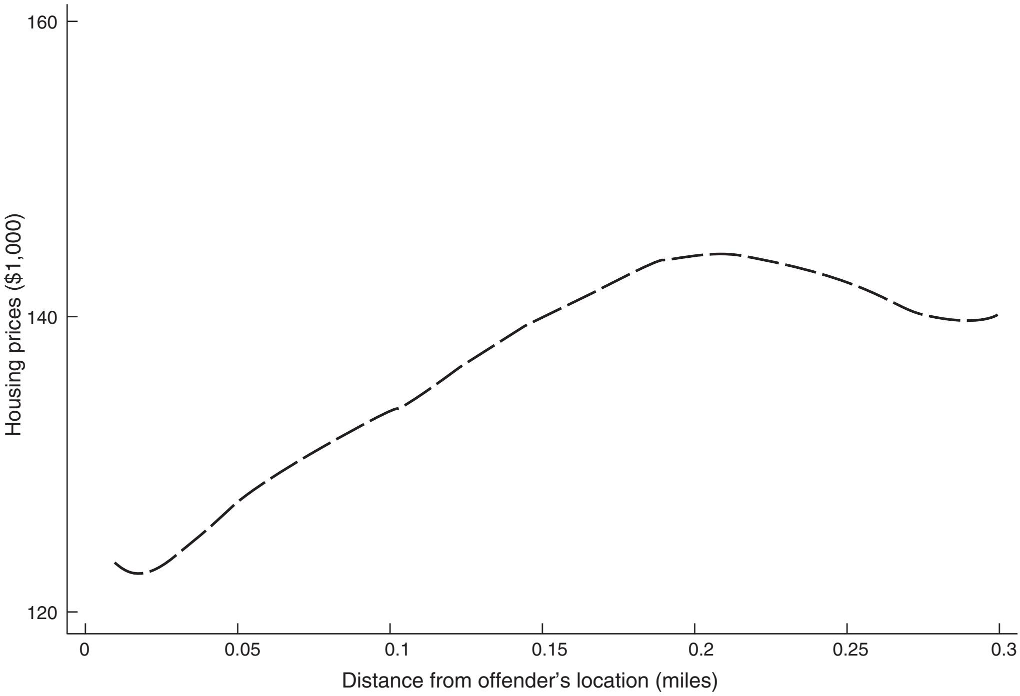

FIGURE 2A. PRICE GRADIENT OF DISTANCE FROM OFFENDER
(Sales during year after arrival)

Note: Results from local polynomial regressions (bandwidth = 0.075 miles) of sale price on distance from offender's future/current location.

gradient of distance to sex offenders' locations during the year after offenders' arrivals.[12] Prices are lowest for homes closest to the offenders, rise with distance until reaching homes about 0.1 miles away, and then flatten out.

Figure 2B adds the price gradient of distance to sex offenders' locations during the year before offenders' arrivals. The price gradients are quite similar between 0.1 and 0.3 miles from the offender before and after arrivals. However, there is a clear decline with proximity to a sex offender for homes within 0.1 miles of the offender. Homes located 0.05 miles from the offender sold for about $145,000 on average before the offenders arrived, but sold for about $125,000 afterward. The decline in sale price was greater for homes even closer to the offender.

The notion that the price decline within 0.1 miles of an offender reflects a causal impact of the offender's arrival would be supported if the decline coincides with the offender's arrival and does not reflect a preexisting downward trend in prices. Figure 3A shows the price gradient of time with respect to sex offenders' arrivals. This gradient is measured separately for the two years before and after offenders' arrivals for homes sold within 0.10 miles of an offender's (future) location. Time is measured in days relative to the date sex offenders arrive. If the price decline showed in Figure 2A reflected a preexisting trend, we would expect to see a gradual

[12] Price gradients are calculated using a locally weighted polynomial estimator, sometimes referred to as a "Fan regression" (Jianqing Fan 1992; Fan and Irene Gijbles 1996).

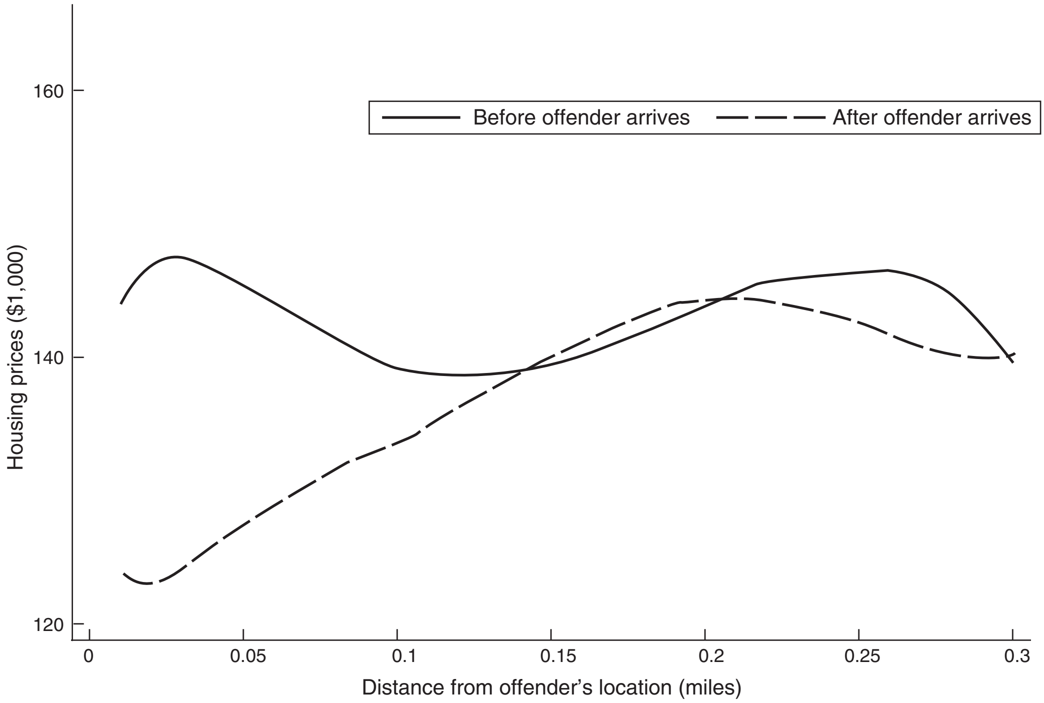

Figure 2B. Price Gradient of Distance from Offender
(Sales during year before and after arrival)

Note: Results from local polynomial regressions (bandwidth = 0.075 miles) of sale price on distance from offender's future/current location.

downward price movement over this time period. Instead, we find a fairly sharp decrease in prices coincident with offenders' arrivals.

Figure 3B shows the price gradient with respect to offenders' arrivals for home prices within 0.1 miles and between 0.1 miles and 0.3 miles of the offender's locations. These latter homes are still quite close to the offenders' locations and (as we saw in Figures 2A and 2B) were selling at similar prices to the affected homes prior to the offenders' arrivals. In contrast to the homes closest to the offenders, prices in these proximate areas did not decline after the offenders' arrivals. It is plausible that the two groups of homes would have had the same trend in prices over time in absence of the offenders. This notion is supported by the fact that, prior to arrivals, the price of homes between 0.1 and 0.3 miles was similar to that of homes within 0.1 miles of the offenders' locations. If so, then homes slightly farther away from offenders can be used as a control group for measuring the impact of offenders on property values.

B. *Statistical Estimation Framework*

We proceed by estimating empirical models inspired by the graphical evidence: a cross-sectional difference estimator and a difference-in-differences estimator. First, we use the cross-sectional difference estimator to check for preexisting differences in the characteristics of parcels located within 0.1 miles of an offender and those located between 0.1 and 0.3 miles of an offender. Given the similarity of these geographic areas, we then use a difference-in-differences

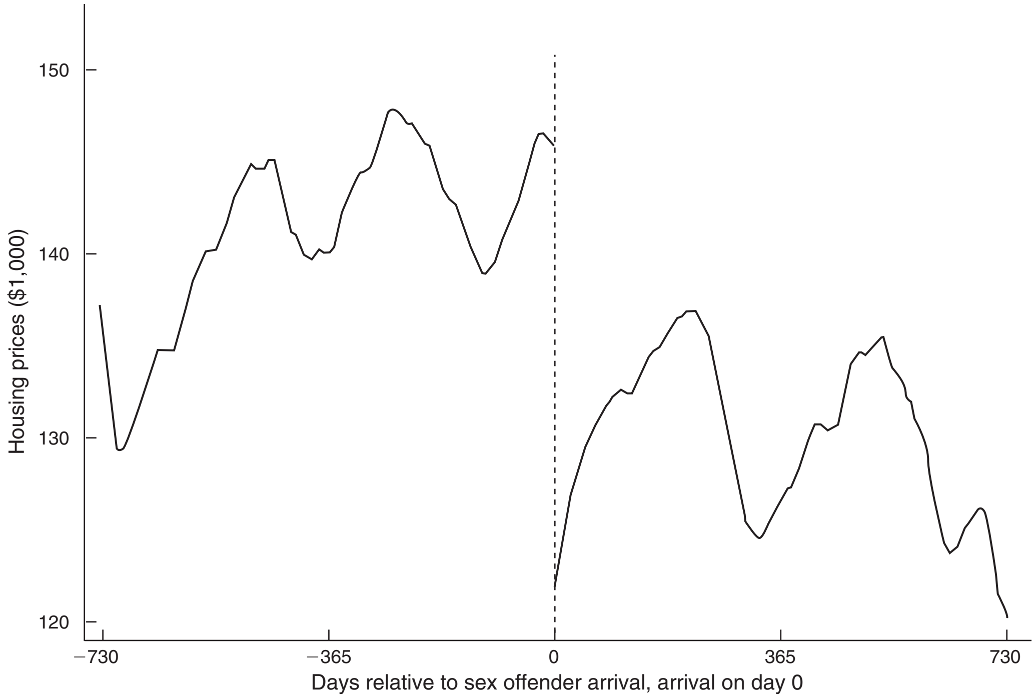

FIGURE 3A. PRICE TRENDS BEFORE AND AFTER OFFENDERS' ARRIVALS
(Parcels within a tenth of a mile of offender location)

Note: Results from local polynomial regressions (bandwidth = 90 days) of sale price on days before/after offender arrival.

model—in which homes that are sold within 0.1 and 0.3 miles of an offender are used as controls for homes that are sold within 0.1 miles of an offender—to estimate the impact of sex offenders on local property values.

The cross-sectional difference specification takes the following form:

$$\log(P_{ijt}) = \alpha_t + \pi_1 D_{ijt}^{1/10} + \varepsilon_{ijt}. \tag{1}$$

The log of the deflated sale price of the house is a function of a measure of distance from the offender, a random error term (allowing for year specific correlation in prices by offender area), and α_t, a year specific effect. The term $D_{ijt}^{1/10}$ is an indicator variable set to one if a parcel sale occurs within 0.1 miles of an offender's address. To examine variation in other parcel characteristics, we substitute those characteristics for log sale price as the dependent variable.

The difference-in-differences specification adds an indicator variable for homes within 0.3 miles of offenders' locations ($D_{ijt}^{3/10}$) and an interaction of this indicator with an indicator for whether the sale took place after the offender's arrival ($Post_{it}$). Thus, the counterfactual change in price for homes sold close to an offender is estimated using homes just slightly farther away. It also includes a neighborhood-year fixed effect (α_{jt}) and observable property characteristics ($\mathbf{X}_i$):

$$\log(P_{ijt}) = \alpha_{jt} + \boldsymbol{\beta}\mathbf{X}_i + (\omega_0 D_{ijt}^{3/10} + \pi_0 D_{ijt}^{1/10}) + (\omega_1 D_{ijt}^{3/10} + \pi_1 D_{ijt}^{1/10})^* \, Post_{it} + \varepsilon_{ijt}. \tag{2}$$

The estimated impact of a sex offender on home values is given by the term π_1.

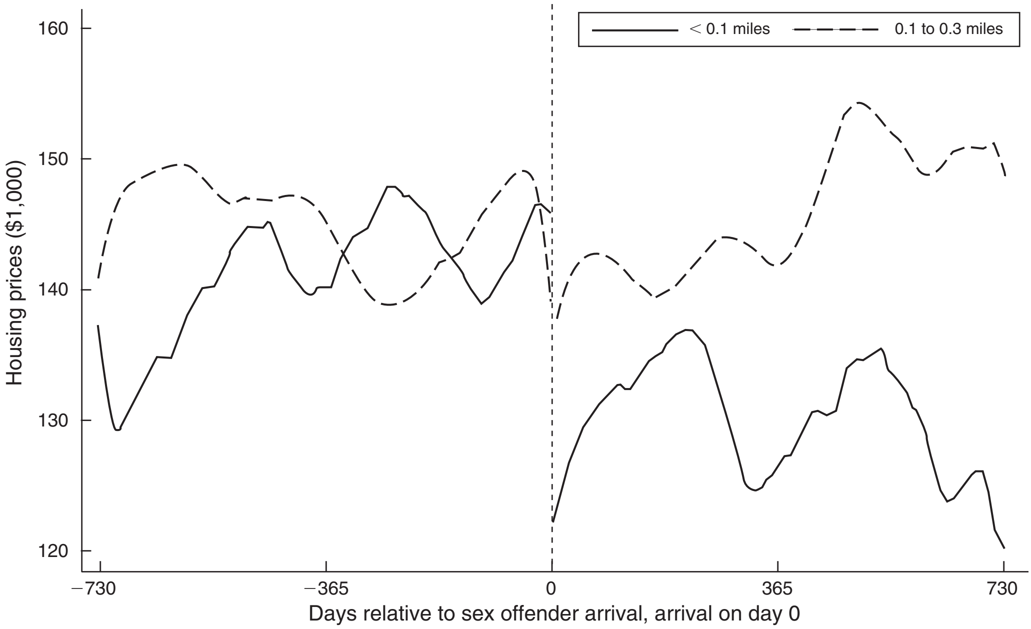

FIGURE 3B. PRICE TRENDS BEFORE AND AFTER OFFENDERS' ARRIVALS
(Parcels within three-tenths of a mile of offender location)

Note: Results from local polynomial regressions (bandwidth = 90 days) of sale price on days before/after offender arrival.

IV. Estimation Results

A. *Differences in Characteristics of Homes Located Close to an Offender*

Our estimation strategy hinges on the relative similarity of homes sold within 0.1 miles of an offender to homes sold between 0.1 and 0.3 miles of an offender. While this is supported by the graphical evidence in Figures 2B and 3B, we formally estimate these differences. We proceed by estimating equation (1) using homes within 0.3 miles of an offender.

First, we limit the sample to sales that took place before the offender's arrival (Table 2, panel A). We find little evidence of any preexisting differences in either sale price or house characteristics. The only difference that is marginally statistically significant is the fraction of homes built in the same year in which they are sold.

Sales need not be representative of parcels in general. To allow for this possibility, panel B of Table 2 compares the characteristics of all existing parcels, rather than just those that sell. These differences are similarly small, but with a much larger sample. As a result, the power of the hypothesis tests is sufficiently increased that these small differences are now distinguishable from zero.

Overall, the results in panels A and B of Table 2 demonstrate the relative homogeneity of the areas compared in our study. The differences, for example, are smaller than the differences one would observe walking down a typical street in these areas. Consider the largest difference that we observe—the 60- 80-square-foot difference in the size of homes. The average standard deviation in the size of homes by street name is 244 square feet (the size of a bedroom), or about 15 percent of the mean. The difference of 60 to 80 square feet in average size between the areas

Table 2—Pre- and Post-Arrival Differences in Average Characteristics of Homes Sold Close to Offenders' Locations

Panel A: Pre-arrival differences in sales	Log price	Built in year sold	Age in years	Square feet (1,000s)	Number of bedrooms	Number of bathrooms
Within 0.1 miles of offender	0.007	0.062	−1.081	0.059	0.022	< 0.001
	(0.034)	(0.035)+	(1.117)	(0.047)	(0.034)	(0.036)
Constant	11.745	0.186	16.616	1.589	3.050	1.717
	(0.036)*	(0.030)*	(1.153)*	(0.039)*	(0.028)*	(0.034)*
Sample size	4,497	4,497	4,497	4,497	4,497	4,497
R^2	0.05	0.03	0.04	0.03	0.03	0.03
Panel B: Differences in all existing parcels			Age in years	Square feet (1,000s)	Number of bedrooms	Number of bathrooms
Within 0.1 miles of offender			0.619	−0.077	−0.049	−0.068
			(0.877)	(0.029)*	(0.027)+	(0.024)*
Constant			35.691	1.655	3.059	1.669
			(1.218)*	(0.033)*	(0.025)*	(0.031)*
Sample size			31,531	31,531	31,531	31,531
R^2			< 0.001	< 0.001	< 0.001	< 0.001

Notes: Pre-arrival (post-arrival) refers to the two-year period before (after) the date upon which the offender registered a current address. Standard errors (in parentheses) are clustered at the offender area level.

* Significant at 5 percent level.

+ Significant at 10 percent level.

is about the size of a walk-in closet. Given the price elasticity with respect to size (column 3 of Table 1), this increase in home size is worth about 2 percent of the average house price. The average standard deviation of sale price on the same street within offender areas is 16 percent of the street's average price.

B. *The Impact of the Arrival of a Sex Offender*

For illustrative purposes, we first present estimates of equation (1), including sales of all homes in Mecklenburg County, sale-year fixed effects, but no other control variables. The estimate of π_1 from this specification is simply a measure of the average price difference between houses within 0.1 miles of an offender's future location and other houses sold within the same year. This difference is approximately 34 percent (column 1 of Table 3) and confirms that homes close to offenders' locations are cheaper than in other parts of the county. However, when we include neighborhood-year fixed effects and house characteristics in the regression (column 2 of Table 3), we estimate that homes within 0.1 miles of an offender sell for only 0.7 percent less on average.[13] This difference is not statistically different from zero at any reasonable confidence level. These results demonstrate that the control variables in the regression capture almost all of the differences between areas in which offenders move and the rest of the county. Controlling for these variables, sex offenders' locations were not significantly less expensive than other parts of their neighborhoods prior to arrival.

[13] Our controls include building quality, the square footage of the property, fireplaces, number of bedrooms, number of bathrooms, a dummy variable for properties built in the same year they are sold, the age of the house in years, and dummy variables for the number of stories, the external wall type, and air-conditioning. Building quality is measured on a 36-point scale. There are six tiers of quality (e.g., "Below Average"), and within each tier there are six quality ranks (e.g., Below Average 1, Below Average 2 … Below Average 6). We include dummy variables for the six quality tiers and a linear term for the 1–6 quality rating. The coefficients from our regressions confirm the nature of the quality rating system.

TABLE 3—IMPACT OF SEX OFFENDERS' LOCATIONS ON PROPERTY VALUE AND SALE PROBABILITY

	Log (sale price) pre-arrival		Log (sale price), pre- and post-arrival				Probability of sale†
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Within 0.1 miles of offender	−0.340 (0.052)*	−0.007 (0.013)	−0.007 (0.012)	$<$0.001 (0.013)	−0.006 (0.012)	−0.006 (0.012)	−0.029 (0.035)
Within 0.1 miles × post-arrival			−0.033 (0.019)+	−0.041 (0.020)*	−0.036 (0.021)+	−0.116 (0.059)+	0.126 (0.059)*
Dist*≤ 0.1 miles × post-arrival (0.1 Miles = 1)						0.107 (0.064)+	
Within 1/3 miles of offender				−0.010 (0.007)			
Within 1/3 miles × post-arrival				0.010 (0.010)	0.003 (0.016)	0.004 (0.016)	−0.055 (0.040)
H_0: within 0.1 miles × post-arrival = 0			*p-value* = 0.079	*p-value* = 0.0443	*p-value* = 0.0828	*p-value* = 0.0502	*p-value* = 0.0361
Housing characteristics		✓	✓	✓	✓	✓	✓
Year fixed effects	✓						
Neighborhood-year fixed effects		✓	✓	✓			
Offender area-year fixed effects					✓	✓	✓
Restricted to offender areas					✓	✓	✓
2 years pre- and post-arrival							
Standard errors clustered by…	*Neighbor-hood*	*Neighbor-hood*	*Neighbor-hood*	*Neighbor-hood*	*Offender area*	*Offender area*	*Offender area*
Sample size	164,993	164,968	169,557	169,557	9,086	9,086	1,519,364
R^2	0.01	0.84	0.83	0.83	0.75	0.75	0.01

Note: Pre-arrival (post-arrival) refers to the two-year period before (after) the date upon which offenders registered their current address. Standard errors in parentheses.

* Significant at 5 percent level.

\+ Significant at 10 percent level.

† Probability sale is measured as percentage points, e.g., probability of sale + 1 would be 100 percentage points.

Estimating a simple pre-post comparison (equation (2) without the indicator variables for parcels selling between 0.1 and 0.3 miles from the offenders), we find that homes located within 0.1 miles of an offender's location sold for 4.0 percent less, on average, than surrounding homes after the offender's arrival (column 3 of Table 3), but just 0.7 percent less on average prior to the offender's arrival. This 3.3 percent decline in price is statistically significant at the 8 percent level. Estimating equation (2)—our difference-in-differences specification—we find a slightly higher estimate of the impact of a sex offender's arrival. This estimate is −4.1 percent, and is statistically significant at the 4 percent level (column 4). The estimated change in value for homes located between 0.1 and 0.3 miles of an offender's location when the offender arrives is positive (1 percent) but statistically insignificant. Thus, homeowners living just slightly farther away from the offender (between 0.1 and 0.3 miles) experienced no decrease in property values on average.[14]

This is a sizable loss. Single family homes within 0.1 miles of offenders' locations sold for about $135,000 in the two years prior to the offenders' arrivals. Thus, our estimates suggest that homeowners who live extremely close to a sex offender and sell their homes lose about $5,500 relative to the amount they would have received if the offender did not move in. If these effects are representative of the impacts of the 373 offenders known to live in private residences in Mecklenburg County, then they collectively depress property values by an estimated $60 million.

[14] In this specification, we assume that the effect is constant over the two-year period after the offender arrives. Figures 3A and 3B support this assumption. We have, however, tested this by running regressions that add interactions between the arrival of an offender and a sale taking place at least one year after the offender's arrival. The interactions were not statistically significant and the point estimates were extremely small (results available upon request), confirming the patterns seen in the raw data.

This suggests that households would be willing to pay a high cost for policies that remove sexual offenders from their neighborhoods.[15]

Implicit in our estimation strategy is the assumption that the relationship between housing characteristics and prices outside of the offender areas are valuable in estimating the relationship between characteristics and prices within the offender areas. If the relationship between house characteristics and prices were systematically different in offender areas and nonoffender areas, this could affect our results. We therefore reestimate equation (2) using only sales from offender areas (column 5 of Table 3). Rather than controlling for neighborhood by year fixed effects, we instead control for offender area by year fixed effects and estimate standard errors clustering at the offender area level. These results are consistent with those in columns 3 and 4, suggesting that using additional data from sales outside of offender areas did not bias our estimates.

While these differences document the average change in prices resulting from the arrival of a sex offender, Figures 2A and 2B suggest that property closest to the offenders' location declines more steeply in value after the arrival of the offender. To check for this heterogeneity in the treatment effect, we add an interaction of the dummy variable indicating a sale within 0.1 miles of an offender after the offender has moved in, with distance from the offender. (Note that, for ease of interpretation, we rescale distance so that 1 equals 0.1 miles.) The results are consistent with the figures. Parcels directly adjacent to the offenders' location are estimated to decline by 11.6 percent, and those parcels a tenth of a mile away experience virtually no change in value (Table 3, column 6).

These effects on property values are similar in magnitude to the effects of other changes in local amenities and disamenities found in other recent studies. The average effect (4 percent) is slightly larger than the effect of attendance rights at the higher scoring of schools serving adjacent neighborhoods (2.5 percent) found by Black (1999) and reductions in air pollution caused by EPA regulation (2 percent) found by Chay and Greenstone (2005). The effect of having a sex offender move into the adjacent property (11.6 percent) is closer to the impact of a cancer cluster on child leukemia risk (14 percent) found by Davis (2004) and the temporary one-year impact of getting an "A" (instead of a "B") school quality rating (20 percent) found by Figlio and Lucas (2004).

In addition to the impact on sales prices, we find some evidence that sex offenders' arrivals cause some households to decide to move. We estimate the impact of sex offenders on the probability that a home sells, using a monthly panel of all parcels in the offender areas for two years before and after the offender's arrival date. Column 7 of Table 3 presents the estimate of a linear regression of the probability that a parcel sells (measured in percentage points) within the difference-in-differences framework. The results suggest that the arrival of an offender does increase the probability that nearby parcels sell by 0.13 percentage points. This is a 20 percent increase from the baseline probability of sale of 0.57 percentage points.

C. *Falsification Tests*

Our analysis shows little evidence of any preexisting differences in homes located close to an offender relative to other homes in their neighborhoods. It is theoretically possible, however, that

[15] The recent emergence of laws regulating where sex offenders can live supports this finding. For example, a Georgia law (currently under a restraining order from a federal judge) prohibits registered sex offenders from living within 1,000 feet of schools, school bus stops, childcare facilities, public and private parks, recreation facilities, or churches—effectively barring sex offenders from any densely populated area in the state. The law's sponsor, Georgia House Majority Leader Jerry Keen, has said publicly that this is intentional, and that he hopes the law will "make it so onerous on those that are convicted of these offenses […] they will want to move to another state" (*Washington Post*, November 22, 2006).

Table 4—Falsification Tests on Impact of Sex Offender Location

	Baseline estimates	Two-year prior arrival dates	Three-year prior arrival dates
		(1)	(2)
Within 0.1 miles of offender	<.001	−0.018	−0.013
	(0.013)	(0.016)	(0.016)
Within 0.1 miles × post-arrival	−0.041	0.019	−0.003
	(0.020)*	(0.020)	(0.020)
Within 0.25 miles of offender	−0.010	−0.010	−0.011
	(0.007)	(0.007)	(0.008)
Within 0.25 miles × post-arrival	0.010	−0.000	0.000
	(0.010)	(0.007)	(0.008)
H_0: within 0.1 miles × post-arrival = 0	*p-value* = 0.0443	*p-value* = 0.3474	*p-value* = 0.8628
Sample size	169,557	169,557	169,557
R^2	0.83	0.83	0.83

Notes: The dependent variable is the log of house price. All regressions contain neighborhood-year fixed effects and housing characteristics (see text for list of characteristics included). Baseline results are taken from column 4 of Table 3. Standard errors (in parentheses) are clustered by neighborhood.

the decrease in values after offenders' arrival is driven by differential trends in values for homes closest to an offender. In other words, the prices of houses in offender areas may be trending over time in a different way than other houses in their neighborhoods. For example, if houses located near the parcel where an offender moves were experiencing slower growth in prices, this could lead to a spurious negative "impact" of the offender's arrival. We investigate this possibility by estimating equation (2) using false arrival dates equal to two years and three years prior to offenders' actual arrival dates. In both of these specifications, we find no evidence of a spurious effect (Table 4).

V. Estimates of the Cost to Victims of Sexual Offenses

The welfare cost to victims of crimes can be used to make optimal policy decisions, such as how much to spend on programs that reduce crime. The results above present evidence that the arrival of a sex offender has a statistically and economically significant impact on the value of homes in the immediate vicinity. Households' willingness to trade off lower house prices against increased victimization risk can be used to estimate the welfare cost of crimes committed by sexual offenders. If the decline in property value close to offenders is indeed driven by increased risk of victimization, then we can make this calculation.

The Department of Justice (DOJ) currently estimates victimization costs using other methods. In a widely cited DOJ study, Ted R. Miller, Mark A. Cohen, and Brian Wiersema (1996) estimate victimization costs for various crimes and include measures of tangible costs (e.g., medical expenses, lost work time, property loss, etc.) and intangible costs (e.g., pain, suffering, fear, lower "quality of life"). Estimates of tangible costs use a number of sources, but rely heavily on losses and injuries reported in the National Crime Victimization Survey (NCVS). Intangible cost estimates rely on data from jury awards to compensate victims (i.e., not punitive damages) and, for fatal crimes, the average value of life estimate across studies reviewed by W. Kip Viscusi (1993). For comparison, victimization cost estimates from this study are shown in Table 5. Adjusting the DOJ estimates to 2004 dollars, estimated average victimization costs of rape and sexual assault are roughly $114,000, 95 percent of which represents intangible costs. In contrast, the estimated average victimization cost of burglary is $2,000, almost all of which is due to direct costs such as property loss.

Table 5—Previously Estimated Victimization Costs from Department of Justice Study

Type of crime	Cost ($2004)
Sexual offenses	
Rape and sexual assault	113,732
Violent crimes	
Murder/manslaughter	3,843,363
Assault	31,374
Robbery	10,458
Kidnapping	43,140
Nonviolent crimes	
Burglary	2,092
Larceny	523
Motor vehicle theft	5,229

Notes: These cost estimates are taken from Tables 2 and 4 in Miller et al. (1996). Their cost estimates are given in 1993 dollars. We adjust these for inflation using the 1993 and 2004 annual CPI for all urban consumers. Victimization costs for kidnapping are not listed in their study, and we therefore set them equal to the cost of assault with injury against a child under the age of 11.

Relying on survey responses and jury awards to estimate victimization costs is problematic to the extent that this information does not accurately reflect individuals' willingness to pay to reduce crime risk. For example, jury awards are often based upon testimony of experts who estimate intangible victimization costs from contingent valuation surveys. Since these surveys require people hypothetically to make a trade-off between suffering from a crime and paying varying amounts of money, one might think that these surveys are likely to misstate the true amount an individual would be willing to pay to avoid being the victim of a crime.

Our empirical strategy is to assume individuals accurately perceive the risks associated with the arrival of an offender and then to infer individuals' willingness to pay to reduce crime risk using the actual distribution of crimes committed against neighbors by sex offenders. Our calculation is based on a simplistic model of the choice faced by the marginal home buyer, whose preferences determine the price discount for living close to a convicted sex offender. This household can choose to live far from a sex offender or to live close to an offender, get a price discount, and expose itself to higher crime risk. The marginal home buyer will have equal utility under either choice, i.e., the discount for living near an offender will compensate them for the increased crime risk. This notion is expressed by equation (3), where utility is a function of lifetime wealth (w), the individual knows the discount (d) and the increased probability of crime ($f(c)$) for living near an offender, and v_c is a scalar that maps crime victimization into an equivalent wealth loss:

$$U(w) = \int U(w + d - v_c c) f(c) dc. \tag{3}$$

Our estimates suggest that property value declined by about 4 percent in areas within 0.1 miles of an offender. At the median price of homes sold in these areas prior to the offenders' arrivals ($135,000), a 4 percent impact implies a decline in value of $5,500. We specify the utility function to have constant relative risk aversion equal to two. This is generally considered a relatively high level of risk aversion, and perhaps even an upper bound given empirical evidence on labor supply decisions (Raj Chetty 2006). We set lifetime wealth at $1.575 million. This is the amount of annual income needed to obtain a mortgage equal to the value of the median home in our sample (about $35,000), multiplied over 45 years.[16]

The amount of additional crime risk faced by neighbors of sex offenders requires a more complex calculation. We estimate the probability distribution with which offenders commit crimes against neighbors using data from the DOJ, the Federal Bureau of Investigation (FBI), and the NCVS. This calculation requires a number of steps, and details are given in the Appendix. We make several assumptions in this calculation, and we examine the sensitivity of our results to alternate assumptions.[17] The assumptions we make generally should lead us toward low estimates of victimization costs. Despite these choices, however, our estimates remain high relative to the lifetime income of our representative household.

We estimate that each sexual offense has a wealth-equivalent welfare cost of almost $1.2 million. Thus, the housing market impacts we identify above imply very large costs to victims of sexual offenses—an order of magnitude larger than the DOJ estimates.[18] We examine the sensitivity of our results to the assumptions embedded in our estimates by estimating victimization costs under wide-ranging alternate assumptions. These alternative estimates, shown in Table 6, vary from about $0.6 million to $2.5 million. We therefore feel confident that the large implied welfare losses are not an artifact of the assumptions built into our calculation.

There are, however, other potential explanations for the large implied costs we find. First, it may be that individuals overestimate the amount of crime risk associated with living in close proximity to a sex offender. There is a long-standing literature that shows individuals tend to overweight rare events in making decisions under risk, and tend to overestimate the actual probability of rare events (Daniel Kahneman and Amos Tversky 1979; Sarah Lichtenstein et al. 1978; Viscusi 1990, 1999). If individuals overestimate the risks posed by sex offenders, then cost estimates based on objective probabilities will be too high. To illustrate the power of overestimation of risk, we recalculate our victimization cost estimates assuming that individuals believe that any crime sex offenders commit against a neighbor will happen to them. Under this (albeit extreme) assumption, we estimate that sexual offenses have a victimization cost of $90,000 (bottom of Table 6).

Another explanation for our results is that there is an additional cost—exclusive of crime risk—to living in close proximity to a released sex offender. This additional cost could come from several sources. First, it is reasonable to believe that individuals derive utility from interaction with their neighbors, and that this utility may vary with their neighbor's characteristics (e.g., shared interests). If individuals derive low utility from interactions with neighbors who are sex offenders, this could lead to a larger impact on house prices. Second, there may be consumption losses that stem from the sex offender's presence (e.g., your friends refuse to visit you). Third, there may be a psychic cost to living near a sex offender, i.e., a cost to living with increased *fear* of crime. The cost of living in close proximity to an offender may include a constant reminder of the possibility of rare but tragic outcomes, such as those faced by the families of Megan Kanka and Jacob Wetterling.

[16] Lenders often follow the 28 percent rule: a family can pay up to 28 percent of gross monthly income (before other debt payments) as mortgage payments. A 30-year fixed rate mortgage of $135,000 (the median home price) at 6 percent interest would give rise to payments of $810 per month. Family income must therefore be about $2,890 per month or about $35,000 per year.

[17] For example, the relationship of offender to victim is reported in the NCVS, and "neighbor" and "stranger" are both potential responses. Recognizing that some "strangers" may actually be "neighbors," we assume that the true fraction of crimes committed by neighbors is 200 percent of the fraction of victims who claim the offender was a neighbor.

[18] This is a much more significant difference than the usual difference between hedonic and contingent valuation studies. Richard T. Carson et al. (1996) find that, on average, the estimates of contingent valuation studies of the value of public goods are about 20 percent lower than those of revealed preference studies (which include hedonic estimates).

TABLE 6—SENSITIVITY OF SEX OFFENSE VICTIMIZATION COST ESTIMATES

Assumptions in calculation	*Estimated victimization cost*
Baseline assumptions	$1,242,000
Lower risk aversion (λ=1)	$2,186,100
Higher risk aversion (λ=3)	$890,000
Fewer neighbors (60)	$1,093,000
More neighbors (180)	$1,320,000
Fewer offenses by neighbors (100% of NCVS)	$2,485,000
More offenses by neighbors (300% of NCVS)	$621,200
Systematic overestimation of risk: households neglect to realize that risk is spread among neighbors	$90,300

Note: Baseline assumptions are as follows: (1) utility function with constant absolute risk aversion equal to 2, (2) lifetime wealth equals $1.575 million, (3) housing market discount equals $5,500, (4) neighborhood risk is spread among 120 neighbors, (5) the fraction of crimes committed against neighbors is 200 percent of the reported rates in the NCVS.

This last explanation is supported somewhat by the distance gradient of the impact of a sex offender's arrival. Recall that the impact of a sex offender's arrival on housing prices is extremely localized. We find no impact more than 0.1 miles (about two city blocks) from the offender's location and the largest impacts are on the homes virtually next door to the offender. We do not know of any evidence of whether the expected change in crime risk should have a similar gradient, but it seems unlikely that the risk posed by the sex offender should decline so quickly in distance and be confined to such a small area. It may be, however, that those neighbors living closest to the offender are far more likely to be aware of his/her presence by passing by the offender's home or coming into contact with the offender on the street.

VI. Conclusion

We use the hedonic estimation methodology to measure the impact of crime risk on property values. Using very detailed data on the locations of convicted sex offenders (whose identities and residential locations are made public on the North Carolina Sex Offender Registry) and the dates on which they move into a neighborhood, we estimate that, on average, the values of homes within 0.1 miles of an offender fall by roughly 4 percent. This effect dissipates quickly with distance of homes from the offender; homes between 0.1 and 0.3 miles away show no effect. Because we exploit the quasi-random process that introduces a convicted criminal into a very specific geographic area at a very specific time, we believe these results are a significant improvement upon the existing literature that attempts to identify the causal relationship between the risk of crime and changes in property values.

Our estimates suggest that individuals have a strong distaste for living in close proximity to a sex offender. We estimate that a single offender depresses property values in the immediate vicinity by about $5,500 per home. If we aggregate these effects across all homes affected and all offenders, we find that the presence of sex offenders depresses property values in Mecklenburg County by about $60 million. This suggests that households would be willing to pay a high cost for policies that remove sexual offenders from their neighborhoods.

We also combine the estimated decline in property values with data on crimes committed by sexual offenders against neighbors to estimate costs to victims of sexual offenses. To do so, we make two key assumptions: the entire decline in property value is due to increased crime risk; and neighbors' perceptions of risk match the objective data. We estimate victimization costs of over $1 million—far in excess of estimates taken from the criminal justice literature—implying a high willingness to pay for policies that reduce the incidence of sexual offenses.

Unfortunately, we cannot test the two assumptions underlying the victimization cost estimate. It is possible that individuals substantially overestimate the risks posed by neighboring sex offenders or experience a cost of living in close proximity to an offender that is unrelated to crime risk. If so, the willingness to pay for policies that only decrease crime risk would be lower. Under these alternative hypotheses, however, households would be willing to support policies that provided accurate information regarding the risks posed by sex offenders or isolate sex offenders without decreasing crime risk.

Appendix: Calculation of Crimes Committed against Neighbors by Sex Offenders

For illustrative purposes, suppose that there is only one kind of crime and that $g(c)$ is the probability distribution of crimes committed by sex offenders. Further, let us suppose that there is a constant probability that, conditional on crimes being committed, they are committed against neighbors (P_N). Finally, let us suppose that there is a constant number of neighbors (N) who are potential victims, that all neighbors are equally likely to be victims, and that crime, conditional on being committed against neighbors, is committed against a single neighbor. The probability distribution of crimes committed against neighbors, $f(c)$, will then be:

$$f(c) = g(c)\,\frac{P_N}{N}.$$

Under these assumptions, we can use data on $g(c)$, P_N, and N, to estimate $f(c)$.

If neighbors are concerned only with the increased risk of sexual offenses (rape and sexual assault) associated with living near a sex offender, then the assumption that c is scalar is fairly trivial and c would represent the number of sex offenses committed by the sex offender. However, sex offenders commit many types of crime, ranging from murder to motor vehicle theft, and it seems reasonable that neighbors would be concerned with these crimes as well as sexual offenses. Unfortunately, we cannot separately calculate victimization costs for various crimes because we do not have variation in the willingness to pay to reduce the risk of various types of crime. To simplify the problem, we assume that all crimes can be specified as a fraction or multiple of a sex offense. For example, victims of a presumably less severe crime, such as burglary, can be seen as suffering costs that are equivalent to a fraction of a sex offense.

If we knew the relative severity of various types of crime, specifying all crimes in terms of sex offenses would be a straightforward exercise. Because we do not know these relative severities ex ante, we must use estimates of relative victimization costs from some other source. We choose to use the estimates listed in Table 5 of Miller et al. (1996) as a rough approximation of the relative costs of victimization among different types of crime; e.g., the relative cost of assault is about 30 percent the cost of rape. It is important to note that, for our calculation, we require only that the *relative* costs of various crimes be estimated correctly in the Miller et al. (1996) study. We find very similar results using relative victimization cost estimates from Mark A. Cohen et al. (2004). While the absolute measures of victimization costs are somewhat larger than those from Miller et al. (1996), they find very similar relative costs of crime.

In order to estimate $g(c)$, we first calculate the number and type of crimes for which sex offenders are arrested in the three years subsequent to their release from prison. This information comes from "Recidivism of Prisoners Released in 1994," a dataset collected in 1998 by the Bureau of Justice Statistics on prisoners released by 15 states. This dataset includes all 10,337 sex offenders who were released from these states in 1994, and gives a complete inventory of all arrests and adjudications of these offenders through 1998. These states are: Arizona, California, Delaware, Florida, Illinois, Maryland, Michigan, Minnesota, New Jersey, New York, North Carolina, Ohio, Oregon, Texas, and Virginia. The dataset also includes a stratified sample

Table A1—Fraction of Criminals Arrested after Release from Prison, by Type of Crime and Type of Criminal

Type of Crime	Sexual offenders		Other criminals	
	3 Years	1 Year	3 Years	1 Year
Sexual offenses				
Rape	2.1%	0.8%	0.5%	0.2%
Sexual assault	4.0%	1.9%	0.7%	0.3%
Violent crimes				
Murder	0.6%	0.3%	0.7%	0.3%
Manslaughter	0.1%	0.0%	0.2%	0.1%
Kidnapping	1.9%	1.1%	0.7%	0.3%
Robbery	4.3%	2.3%	6.2%	2.8%
Assault	14.1%	5.1%	13.5%	5.8%
Nonviolent crimes				
Burglary	7.1%	2.8%	9.9%	5.4%
Larceny	11.0%	4.3%	16.5%	9.0%
Motor vehicle theft	3.0%	1.1%	4.6%	2.3%

Note: Shown are the fraction of all prisoners released in 1994 who were re-arrested at least once within one and three years of their release.

of all other prisoners released in these states in 1994. Because the data contain offenders' entire criminal histories, we treat as sex offenders all released prisoners who had previously been convicted of a sexual offense, not just those whose most current prison sentence was due to a sexual offense conviction. We use sampling probability weights to construct population averages. We drop offenders for whom a record of arrests and prosecutions (a "RAP sheet") was not successfully located and offenders who died during the three years following their release. We also drop a small number of offenders who had unknown arrest and adjudication dates (making it impossible to distinguish recidivism from prior criminal history) or had adjudication dates that preceded the arrest date for any given offense. Importantly, we assume that these offenders' subsequent behavior are representative of the expected behavior of an offender in North Carolina. If Megan's Laws reduce crime, this may overstate risk.

Table A1 shows the fraction of sexual offenders and other released criminals who are arrested for various crimes during the first three years after their release from prison. Sex offenders are much more likely to be arrested for a sexual offense than other released criminals. The fraction of released sex offenders who are later arrested for rape and sexual assault is 2.1 percent and 4.0 percent, respectively. Moreover, the ratio of arrests for sex offenders versus other criminals is over 4:1 for rape and over 5:1 for sexual assault. Arrests of sex offenders are similar to other released convicts for violent crime, though somewhat more likely for kidnapping and assault, and less likely for murder, manslaughter, and robbery. Arrests of sex offenders are significantly less likely for nonviolent crimes such as burglary, larceny, and motor vehicle theft.

It is important to note that sample selection into this dataset may overstate the frequency of arrests for all criminals at all times. Almost all of the released criminals spent a year in prison for their crimes, whereas 30 percent of sex offenders registered in North Carolina spent less than one year in prison. Also, we examine offenders just after their release from prison, when they are most likely to recidivate. Indeed, of sexual offenders' arrests for rape and sexual assault, 37 percent and 49 percent (respectively) come in the first year after their release. These one-year statistics are also reported in Table A1.

Not all crimes lead to arrests. In order to estimate the crimes actually committed by offenders, we use statistics from Lee and McCrary (2005) on the fraction of crimes that are reported to the police and fraction of reported crimes that lead to an arrest (Table A2). Their calculations are based on comparisons of victimization reports from the NCVS and crimes reported to the police,

Table A2—Estimated Ratios of Crimes to Arrests

Type of crime	Percent of crimes reported	Percent reported crimes that lead to arrest	Ratio of crimes to arrests
Sexual offenses			
Rape	45.0%	54.0%	4.12
Sexual assault*	"	"	4.12
Violent crimes			
Murder	64.0%	77.0%	2.03
Manslaughter*	"	"	2.03
Robbery	26.0%	71.0%	5.42
Assault	57.0%	46.0%	3.81
Kidnapping*	"	"	3.81
Nonviolent crimes			
Burglary	13.0%	58.0%	13.26
Larceny	18.0%	33.0%	16.84
Motor vehicle theft	14.0%	86.0%	8.31

Notes: These figures are taken from Appendix Table II of Lee and McCrary (2005) and are the results of their calculations using data from the NCVS and Uniform Crime Reports for 2002. "*" denotes that no information on reporting and arrests was available for this crime and that it is assumed that reporting and arrests follow the same pattern as the preceding (similar) crime.

and reported crimes that lead to arrests from FBI Uniform Crime Reports (UCR). (See Appendix Table 2 of their study for further explanation.) Because the NCVS and UCR data do not break out crimes into great detail, we assume that similar crimes have similar crime/arrest ratios. For example, we assume that the ratios are the same for rape and sexual assault.

According to their estimates, for every individual arrested for a sexual offense, roughly four offenses had actually been committed (i.e., there is a crime/arrest ratio of 4:1). Although we can use the estimates in Table A2 to gauge crime/arrest ratios, we do not have estimates of the extensive and intensive margins of criminal activity. In other words, even if the crime/arrest ratio is 4:1, it may be that all four crimes were committed by the same offender who was arrested (intensive), or it may be that four different offenders committed one crime each, but only one offender was arrested (extensive).

We assume that the crime/arrest ratio is due entirely to the intensive margin, i.e., each arrest is indicative of multiple crimes, but nonarrested offenders do not commit crimes. Given this assumption, the empirical distribution of arrests and the estimated crime/arrest ratios are sufficient to estimate the empirical distribution of crimes committed. It is important to note that the intensive assumption—placing a larger number of crimes on a small number of offenders—will lead us toward estimates of welfare costs that are lower, given risk aversion, than assuming that some of the crime-arrest ratio is due to offenders who commit crimes but are not arrested.[19]

We estimate the fraction of crimes committed against neighbors using the fraction of victims claiming that the offender was a neighbor in the concatenated NCVS files from 1993 to 2004. Because the NCVS cannot ask murder or manslaughter victims about their offenders, we use the 2003 Supplemental Homicide Reports (a subset of the UCR data) to estimate offenses by neighbors for these crimes. This is, of course, possible only for crimes where the offender is known.

[19] This can be shown in the following manner. Suppose there is a $1/N$ chance of being a victim of N crimes. Indifference to this risk implies $U(w) = (1/N)U(w+d-nv) + (N-1)/N\,U(w+d)$, where notation follows equation (3). As N increases, the probability of being a victim falls, but the number of crimes committed per victimization rises. This is essentially the intensive margin assumption. dv/dN is the change in the wealth equivalent value of a *single crime* that sustains the equation when N rises. Solving for dv/dN yields an expression proportional to $[U(w+d)-U(w+d-nv)]-nv \times U'(w+d-nv)$. The term in brackets equals the loss in utility from victimization, which must be smaller than the second term if the agent is risk averse, i.e., if $U'' < 0$. For a risk-neutral agent, dv/dN would be zero.

Table A3—Offenses Committed by Neighbors

Type of crime	Percent of crimes by neighbors	Percent of crimes by neighbors or strangers
Sexual offenses		
Rape	3.7%	18.7%
Sexual assault	6.9%	24.8%
Violent crimes		
Murder/manslaughter	0.7%	15.5%
Robbery	3.2%	53.4%
Assault	5.5%	31.7%
Kidnapping*	"	"
Nonviolent crimes		
Burglary	11.1%	46.1%
Larceny	5.1%	35.6%
Motor vehicle theft	3.0%	50.7%

Notes: With the exception of murder and manslaughter, these figures are calculated using victims' reports of offenders' identities in the 1993–2004 concatenated NCVS. Figures for murder and manslaughter are calculated using data from the 2003 Supplemental Homicide Reports. "*" denotes that information was not available for this crime and it is assumed that offenses by neighbors follow the same pattern as the preceding (similar) crime.

Murder and manslaughter are not separately identified in this data, so we combine them. For murder/manslaughter, rape, and sexual assault, the fractions of offenses committed by neighbors are 0.7 percent, 3.7 percent, and 6.9 percent, respectively (Table A3, column 1). These figures suggest that the crime risk from neighbors may be quite small. One potential problem with these measures is that victims may not know their neighbors. The fraction of crimes committed by *both* neighbors and strangers is a possible alternate measure, but it is often an order of magnitude greater than the fraction committed by neighbors alone, and is likely to considerably overestimate crime risk from neighbors (Table A3, column 2). Recognizing the problems inherent in both measures, we assume that the true fraction of crimes committed by neighbors is 200 percent of the fraction of victims who claim the offender was a neighbor. In other words, for every crime victim claiming the offender was a neighbor, another victim claimed the offender was a stranger when, in fact, the true offender was a neighbor.

We estimate the number of households in the neighborhood among which crime risk from the sex offender is spread by measuring the number of single-family homes located within one-tenth of a mile of offenders in Mecklenburg County. The median number of single-family homes within one-tenth of a mile of offenders' parcels—at the time they moved in—is 120. This is probably an underestimate of the number of relevant households facing the increased risk of crime, since it does not include other residential structures such as condominiums, multifamily homes, and apartment buildings.

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