Braakmann N.

**The link between crime risk and property prices in England and Wales:
Evidence from street-level data.**

***Urban Studies* (2016)**

DOI: <http://dx.doi.org/10.1177/0042098016634611>

Copyright:

This is the author's accepted manuscript of an article that has been published in its final definitive form by Sage Publications Ltd, 2016.

DOI link to article:

<http://dx.doi.org/10.1177/0042098016634611>

Date deposited:

01/01/2016

This work is licensed under a [Creative Commons Attribution-NonCommercial 3.0 Unported License](https://creativecommons.org/licenses/by-nc/3.0/)

The link between crime risk and property prices in England and Wales – Evidence from street-level data

*

[This version December 7th, 2015]

Abstract:

This paper uses street-level data on property sales and crime rates for England and Wales to investigate compensating differentials for crime risk. My identification strategy relies on the use of non-parametric regional time trends on various levels of spatial aggregation as well as various fixed effects for streets and wider areas to control for unobserved amenities and regional economic conditions. The data comes from transaction data collected by the Land Registry and recently published crime maps for the whole of the UK. My estimates, which are robust to a range of sensible specification changes, suggest that each case of anti-social behavior per 10 population in the same street leads to an approximately 0.6 to 0.8% drop in property prices, while a corresponding increase in violent crime decreases prices by roughly 0.6 to 1.6% and a corresponding increase in non-violent crime by about 0.2 to 0.4%. The majority of estimates are at the upper end of these intervals. Estimates for robbery, burglary and vehicle crime are either zero or positive, but are possible biased due to reverse causality. Crime outside of the respective street does not appear to matter, which is consistent with earlier findings. Expressed in monetary terms each case of anti-social behavior costs society between £5000 to £6700 and each violent crime between £5000 to £13,300. The results

* All analyses used Stata 13. Do-Files are available from the author on request. All analyses and opinions expressed in this paper as well as any possible errors are under the sole responsibility of the author. The data is released under Open Government Licence for public sector information (<http://www.nationalarchives.gov.uk/doc/open-government-licence/>). The land registry data covers the transactions received at land Registry in the period January 1st, 2011 to December 31st, 2013 and is covered by Crown copyright. If you have found an error with the data, please contact [Her Majesty's Land Registry \(HMLR\)](#).

confirm estimates based on prior willingness-to-pay studies and other studies using smaller areas such as single cities.

Keywords: cost of crime; compensating differential; property prices

1. Introduction

A large literature in economics has been concerned with estimating the social cost of crime. The most common approach in that literature has been the calculation of compensating wage differentials in either wages or property prices as predicted by models by Roback (1982, 1988), who also provides some evidence. An alternative approach are willingness to pay studies for the avoidance of victimization relying on stated preferences (Ludwig and Cook, 2001; Cohen et al., 2004; Atkinson et al., 2005). The logic underlying the former approach is that regional amenities or disamenities influence the utility of individuals living or working in a region and consequently their willingness to buy a property at a certain price or to work for a certain wage. Other papers following this approach include, inter alia, Gerking and Neirick (1983), Blomquist et al. (1988), Smith (2005), Schmidt and Courant (2006) and Braakmann (2009) for wages and Bowes and Ihlanfeldt (2001), Lynch and Rasmussen (2001) and Gibbons (2004) for house prices. A different strand of the literature uses similar methods to look at the non-monetary costs of crime such as forced behavioral changes (e.g., Hamermesh, 1999; Braakmann, 2012a), effects on well-being (Powdthavee, 2005) or effects on mental and physical health (Braakmann, 2012b; Dustmann and Fasani, 2015).

Most of the earlier literature used larger area crime rates, e.g., city or county crime rates. A few notable exceptions are Gibbons (2004), who looks at criminal damage in London and Linden and Rockoff (2008) and Pope (2008) who look at the location of sex offenders using data from one US county. In contrast, this paper considers a variety of criminal offences for the whole of England and Wales using street-level crime data linked with data on all property purchases (a total of close to 2 million transactions) from January 2011 to December 2013. The property data are available from the land registry and contain information on the price paid, the location of the property, specifically the exact address, and

some limited information on the property itself such as whether it is newly constructed or whether it is a flat or a certain type of house. Using the address information, I combine this data with street-level crime data from www.police.uk, a website operated by the British police since December 2010 that contains monthly reports on the location of various types of crime.¹ It is important to be clear that reported crime rates might differ from true crime rates due to over- or underreporting. Consequences of this fact for the estimation of compensating differentials are discussed in section 3.

Empirically identifying compensating differentials for crime rates has been proven to be challenging for at least two reasons. First, crime rates will generally be correlated with other regional factors that are likely to influence crime, some of which might be time-varying such as economic conditions², and some of which might be unobserved, such as changes in other regional (dis-)amenities. Second, there is evidence that regional crime rates have an influence on the location decisions of businesses (e.g., Cullen and Levitt, 1999; Abadie and Dermisi, 2008), which might lead to indirect reverse causality as crime might influence local labor market conditions that in turn might influence crime.

Using street-level data allows me to address the empirical challenges in a relatively simple way. First, I am able to use low-level regional fixed effects, roughly on the level of neighborhoods or alternatively city quarters, that can be expected to capture most of the regional (dis-)amenities that would matter for house prices. These low-level fixed effects are likely to capture more unobserved factors than city or county dummies as used in previous papers (e.g., Braakmann, 2009). I also use low-level socio-demographic classifications on the

¹ Interestingly the daily press suspected early on that the better information about crime that the public receives through the publication of these maps might have an effect on house prices, e.g., Collins (2011).

² For the link between economic conditions and crime see, e.g., Piehl (1998) or Freeman (1999) for surveys and Reilly and Witt (1996), Carmichael and Ward (2001), Raphael and Winter-Ebmer (2001), Gould, Weinberg and Mustard (2002), Edmark (2005) and Braakmann (2012c) for recent evidence.

postcode level as additional controls and provide some evidence using streets with repeated purchases during the observation period, which allows the inclusion of street fixed effects. Second, it is important to note that most of the unobserved time-varying factors that influence crime will vary on a higher level of aggregation than the street. Labor market conditions, for example, will generally vary on the level of the local labor market, which is closer to the city rather than to any particular street. To capture these factors I rely on the inclusion of city/local authority*month dummies or in some specifications city-quarter*month dummies that can be expected to capture most, if not all common time-varying confounders. Third, the fact that I use data for just three years (as opposed to several years as in most of the previous literature) attenuates remaining concerns regarding reverse causality of the type described above, simple because businesses will not have had much time to react to eventual changes in crime rates.

Relying on property prices instead of wages has some further advantages in this context: First, the theory of compensating differentials is derived using competitive markets, which is generally a debatable assumption for labor markets (see, e.g., Manning 2003, 2011). Property markets are in comparison relatively unregulated, which makes it more likely that differences in (dis-)amenities show up in prices. Second, while looking at wages always leads to concerns regarding selective regional mobility and sorting of workers into regions (see, e.g., Braakmann, 2009, section 4.5), properties are generally immobile.

The previous literature on property prices is relatively sparse and usually based on evidence from a single city such as Atlanta (Bowes and Ihlanfeldt, 2001), Jacksonville (Lynch and Rasmussen, 2001) or London (Gibbons, 2004) or on data from some other small region such as Mecklenburg County in North Carolina (Linden and Rockoff, 2008) or Hillsborough County in Florida (Pope, 2008). In contrast this paper uses data for the whole of England and Wales, including rural and urban areas. The general conclusion that can be

drawn from the literature, regardless of whether the estimates are based on a selection-on-observables assumption as in Bowes and Ihlanfeldt (2001) or Lynch and Rasmussen (2001) or an instrumental variables strategy as in Gibbons (2004), is that crime has a negative effect on property prices or values. These effects are found for various types of crime and are generally economically large: Bowes and Ihlanfeldt (2001) find a 3–5.7% decrease in property prices for one additional crime per acre. Lynch and Rasmussen (2001) find a 4% decrease in property prices for a one-standard-deviation increase in violent crimes, and an insignificant increase in prices for higher levels of property crime. Finally, in the only other study for Europe, Gibbons (2004) results show a 10% decrease in property prices for a one-standard-deviation increase in criminal damage to property and an insignificant relationship with burglary. Finally, two recent studies look at the related but slightly different question whether living close to a convicted sex offender reduces property prices (Linden and Rockoff, 2008; Pope, 2008). Both studies find that having a registered sex offender moving into a house close by reduces property prices by between 2% (Pope, 2008) and 3 to 4% (Linden and Rockoff, 2008). Both studies also find evidence that these effects are very localized and quickly decline with distance to the offender.

These results are broadly confirmed in this paper: Each case of anti-social behavior per 10 population in the same street leads to an approximately 0.6 to 0.8% drop in property prices, while a corresponding increase in violent crime decreases property prices by roughly 0.6 to 1.6% and a corresponding increase in other crime by about 0.2 to 0.4%. Estimates for robbery, burglary and vehicle crime are either zero or positive, but are possible biased due to reverse causality. The majority of estimates using a range of sensible specifications are at the upper end of these intervals. Similarly in spirit to results of Pope (2008) and Linden and Rockoff (2008) crime in the wider area, e.g., the neighborhood or the city-quarter does not seem to have an effect on prices once crimes in the same street are accounted for.

The rest of the paper is organized as follows: Section 2 and 3 describe the data and the estimation approach respectively. Results can be found in section 4. Section 5 concludes.

2. Data

The data used here come from two sources – www.police.uk, a website created by the British police that provides monthly street-level information on recorded crimes and the land registry, which records all property sales in the UK. The first source was set up by the British police in 2010 as part of the open-data initiative of the British government. It provides street-level maps of recorded criminal offences for each month since December 2010 and also allows the download of the underlying data. The prevalence of crime is measured on a monthly basis by the counts of recorded offences in several categories, specifically “anti-social behavior”³, “burglary”, “criminal damage and arson”, “drugs”, “other theft”, “other crime”, “public disorder and weapons”, “robbery”, “shoplifting”, “vehicle crime” and “violent crime”. Not all of the categories are measured consistently over the observation period: “Criminal damage and arson”, “drugs”, “other theft” and “shoplifting” are separate categories only since mid-2011 and were part of “other crime” before. “Public disorder and weapons” undergoes several changes: Until mid 2011 it was part of “other crime” and from mid 2013 it split up into two separate categories, “public disorder” and “possession of weapons”. Theft also undergoes several changes with “bicycle theft” and “theft from the person” being split from “other theft” towards the end of the observation period. Given these restrictions, I focus on the following types of crime: “anti-social behavior”, “violent crime”, “burglary”, “robbery”, “vehicle crime” and “other crime”, where the latter contains all remaining crime categories.

³ The notion of “anti-social behavior” was introduced in the 1998 Crime and Disorder Act with some changes being introduced in the 2003 Anti-social Behaviour Act. It basically describes acting “in a manner that caused or was likely to cause harassment, alarm or distress to one or more persons not of the same household as himself [the perpetrator]” (Part I, Chapter 1, Section 1 of the Crime and Disorder Act 1998).

The property data come from the UK land registry, a government department founded in 1862 that serves as the central registry for all land owners in England and Wales. The data used here is the so-called price paid data⁴ that has been made publicly available for a number of years (currently 1995 onwards). The version used here contains all property sales in England and Wales from January 2011 to December 2013. The data contain information on the full address of each property, the price paid, the date of transaction, the property type (flat, terraced house, semi-detached house or detached house), whether the property is newly built and whether the property is freehold or leasehold.

Both data sets are merged based on a combination of coordinates and UK unit postcodes, which are essentially equivalent to streets or parts of streets. In a first step all crimes in each month are merged to the nearest postcode based on latitude and longitude, where “nearest” means the smallest geodetic distance between the coordinates of the crime and the coordinates of the postcode calculated using formulas derived by Vincenty (1975).⁵ The crimes are then aggregated to monthly counts in each category per postcode. As postcodes differ in population size, I merge this data to postcode population counts from the 2011 census and calculate crime rates as monthly crimes per 10 population. I also calculate two measures of wider-area crime, specifically the crime rates in each category by lower layer super output area (LSOA) and by middle layer super output areas (MSOA). Lower and middle layer super output areas are spatial units used by the UK census to present data in a consistent way over time. Both are relatively small spatial units: According to the Small Area Population Estimates by the Office for National Statistics, LSOAs have on average 1600 inhabitants, while MSOA have an average population of 7700 (as of mid-2010). One can think of both as being close to city quarters or – in the case of LSOAs – even smaller

⁴ See <https://www.gov.uk/government/collections/price-paid-data>.

⁵ This uses the Stata ado-file `geonear` by Robert Picard.

neighborhoods. In a second step this postcode-crime data is merged to the property data based on the postcode and month.

I also merge this data with information on the socio-demographic structure of streets, specifically the 2011 area classification by the Office for National Statistics (2015). This data is based on a variety of variables from the 2011 census and aggregates these to 8 supergroups, 26 groups and 76 subgroups describing the socio-demographic structure of an area.

In the resulting data set each property sale is one observation with measures of crime recorded for the postcode/street where the property is situated and two measures of wider area crime, specifically LSOAs and MSOAs. The final sample consists of 1,892,958 observations.

(TABLE 1 AROUND HERE.)

Table 1 contains descriptive statistics for the variables of interest in the the estimation sample. Note that both the minimum and maximum price for a property seem rather extreme. While both are also plausible – a price of £6950 can easily be the result of an auction sale for a (potentially derelict) property starting at £1 and there are houses in the UK, in particular in London, that sell for £55m – the robustness of the estimates will be checked on a subsample where the top and bottom 1% of all prices have been dropped. Descriptive statistics for this subsample can be found in the lower panel of table 1.

3. Estimation strategy

I estimate regressions of the form

$$\ln(p_{iprlt}) = X_i' \beta + \lambda' SD_p + \tau^* c_{prlt} + \gamma^* \dot{c}_{rlt} + \eta_{lt} + \alpha_r + \varepsilon_{irlt}, \quad (1)$$

where $\ln(p_{iprct})$ is the natural logarithm of the price for property i in postcode p in area r in local authority l at time t . X_i contains a set of property characteristics. SD_p are dummy variables for the 76 area classification subgroups for the respective postcode. c_{prlt} is the crime rate for the respective street/postcode in which the property is situated and $\dot{c}_{rlt}$ is the wider area crime rate, i.e., the crime rate in the same LSOA or MSOA outside of the respective street. α_r are a set of area fixed effects, depending on the specification either for LSOAs, MSOAs or streets/postcodes, and η_{lt} contains local authority-month fixed effects or in some specifications MSOA-month fixed effects. The various fixed effects, essentially dummy variables for each area or each area-month combination, control for all observed and unobserved factors that are constant within the respective area or area-month. The following pages discuss in greater detail how these address various possible biases in the estimation. Finally, ε_{irlt} is a standard error term. Standard errors are generally clustered on the highest area level at which one of the regressors varies, i.e., LSOA for estimations involving street and LSOA-level regressors, MSOA if MSOA-level variables are included and so on. In practice, the choice of clustering makes very little difference for the statistical significance of any of the estimates.

When trying to estimate compensating property price differentials for crime risk, there are three econometric issues that one needs to be worried about. First, crime risk is likely to be correlated with a range of other local amenities such as housing quality, the extent and quality of public services such as schools, libraries or public transport or the type of persons one is likely to get as a neighbor. To the extent that these are time-constant over the period studied in this paper, they will be captured by the area fixed effects α_r as well as the postcode area classification SD_p . Note that MSOAs and in particular LSOAs are already fairly small spatial units, i.e., it seems likely that most regional (dis-)amenities that matter for individuals' buying decisions will be captured by these fixed effects. Remaining socio-economic

differences will, at least partially, be covered by the postcode area classifications that aggregate a large number of socio-demographic and economic variables from the 2011 census. In some robustness checks I also use the fact that 432,603 postcodes have multiple purchases either in the same month or at different points in time to include street/postcode fixed effects. Note that it is not entirely clear whether these estimates are necessarily better on theoretical grounds than the estimates using LSOA or MSOA fixed effects. On the one hand, street fixed effects will capture unobserved differences in housing quality or amenities between streets in the same wider area. On the other hand, the presence of postcode fixed effects means that the estimates use only within-postcode variation in crime over time. As houses are durable consumption goods it is not entirely clear whether prospective buyers are really influenced by these, somewhat transitory, fluctuations. Estimates using LSOA or MSOA fixed effects also include the effects of a property being situated in a “bad” street with higher crime rates than the wider area. In this sense, it seems likely that the estimates including postcodes are absolute lower bounds for the effects of interest.

Second, one might be concerned about the influence of the region’s economic situation and local labor market conditions. The original Becker (1968) model of crime emphasizes the role of legal work opportunities and empirical evidence has been found for a link between economic conditions and crime.⁶ Furthermore, it seems likely that a region’s economic conditions will have some impact on property prices, even in the relatively short period considered in this paper. When talking about economic conditions, it is important to be clear that we would not generally expect economic conditions in the respective street to matter. Instead legal work opportunities for someone living in a certain street will be more likely determined by the overall economic conditions in the local labor market. These in turn

⁶ See Piehl (1998) and Freeman (1999) for surveys and Reilly and Witt (1996), Carmichael and Ward (2001), Raphael and Winter-Ebmer (2001), Gould, Weinberg and Mustard (2002), Edmark (2005) and Braakmann (2012c) for recent evidence.

can easily be captured by the region-month effects η_{lt} . The region-month fixed effects either refer to local authorities, which are roughly equal to cities or to larger rural areas or even smaller spatial units such as MSOAs (city quarters). Note that estimates based on these two specifications are usually very similar indicating that not much of importance is missed by the local authority-month effects. These estimates also capture any relevant general trends in property prices and/or crime rates in the respective region that might otherwise lead to spurious correlation between crime rates and prices, for example if these two variables were simply trending similarly.

Finally, a remaining concern could be direct reverse causality running from the price of property i to the risk of property i being affected by crime. This reverse causality could potentially be much stronger compared with papers using city-wide crime rates simply because a single criminal offence would constitute a much larger relative increase in crime on the street than on the city level. The resulting bias will depend on whether certain types of crime are more or less likely to occur in streets with more expensive properties. If for example, a certain type of crime were more likely to occur in more expensive streets, the resulting estimates would be biased upward. As the true causal effect of crime is likely negative, i.e., crime risk is a disamenity that lowers property prices, we might find a positive, negative or zero effect depending on which effect is stronger. It is important to be clear, however, that the area fixed effects and the postcode-level controls will capture many factors that could introduce such reverse causality, such as some areas having nice properties, others having a flourishing nightlife or yet others being generally deprived.

The variation used to identify the effects in this paper come from within-area within-region-month variation in crime rates and property prices. In other words, I exploit the fact that in some months some properties that are on the market will experience higher street-level crime rates than other properties that go on the market in the same area in either the same or a

different month, while taking into account trends in the wider region.

A common concern with reported crime data is the possibility of measurement error due to underreporting of crimes. Additionally, measurement error could also be introduced through errors in the matching of crimes to postcodes and properties. Specifically, there are the following sources of potential measurement error:

a) Under- or over-reporting of crime: Essentially, this would mean that reported and real crime rates differ. This type of measurement error could cause bias if buyers and sellers of properties could observe and act on the true crime rates, while this paper has to rely on reported crime. In practice, this is an unlikely scenario as at least buyers are unlikely to observe true crime rates and would have to rely on reported crime when deciding about their willingness to pay for a certain property. Even if this type of measurement error mattered, it is not obvious that it would be systemically related to property prices, in which case the resulting measurement error would lead to attenuation bias towards zero. If this was the case, estimates would represent lower bounds for the true effects.

b) Mismatch of crimes to postcodes: In principle, there are two potential sources of measurement error when matching crimes to postcodes. Firstly, the police might record crime at the wrong place, for example, by mistyping an address. Secondly, the spatial matching procedure matches crimes to the (geographically) nearest postcode, which might not be the postcode where the crime occurred. These errors are unlikely to be systematically related to house prices and would consequently lead to attenuation bias. They are also less likely to affect the estimates for LSOA and MSOA crime rates as these would only be influenced by mismatches that place a crime in a different LSOA or MSOA.

c) Mismatch of house prices to postcodes: 1,892,958 property transactions could be successfully matched to postcodes, while matching failed for 205,860 transactions due to

missing or non-existent (and likely mistyped) postcodes. Given the existence of mistyped postcodes in the land registry data, it is also possible that some properties are matched to the wrong postcodes and consequently crime rates. The former type of error would only matter if the missing data process was related to property prices, while the latter will essentially weaken the relationship between crime rates and property prices and would lead to attenuation bias towards zero.

In sum, in all likelihood measurement error in this case will lead to attenuation bias towards zero, essentially allowing for an interpretation of the results as lower bounds for the true effects. This measurement error is also more likely to be relevant for the postcode crime estimates than for the estimates looking at LSOA or MSOA crime.

A final question concerns the representativeness of the estimation results. Estimates based on property transactions will inevitably have more observations from more active property markets. This fact might matter for the estimated overall effect if the crime-property price relationship varies systematically across market types, in which case the estimates will reflect the situation in higher-volume markets to a greater extent than that in lower-volume market. It is important to be clear that while this does not introduce bias in the conventional sense of the word, it essentially affects the aggregation of heterogeneous effects to an overall effect. Table 2 compares the socio-economic classification of all postcodes, postcodes with property purchases and postcodes with multiple transactions. It is clear from the table that both transaction samples contain fewer rural postcodes and a higher share of urban and suburban postcodes. However, there is a fair share of all postcode types in all samples, suggesting that the estimates do not completely miss parts of the picture.

(TABLE 2 AROUND HERE.)

4. Results

4.1 The impact of crime on house prices

Consider first the base results displayed in table 3. Column (1) contains the most basic estimates excluding property characteristics, which are added in column (2). Column (3) add the postcode 2011 area classifications. Columns (4) to (6) then add either LSOA or MSOA crime rates or both as additional regressors. The first thing to note is that columns (2) to (6) generally show very similar results for the street level crime rates, which suggests that postcode-level confounders as well as wider-area crime do not play a large role. For anti-social behavior, violent crime and other crime, point estimates also have the expected (negative) sign and are economically large. For anti-social behavior each additional crime per 10 population leads to a drop in property prices by approximately 0.8%, while each additional violent crime leads to a drop by 1.6%. Effects are smallest for other crime – where reverse causality issues are potentially largest – and suggest a 0.4% decrease in property prices per additional crime. Expressed in terms of standard deviations the results suggest a roughly 0.2% decrease in property price for a one standard deviation increase in anti-social behavior, a 0.16% decrease for an equivalent increase in violent crime and a 0.1% decrease for non-violent crime. Effects for burglary and robbery are essentially insignificant and economically small, while vehicle crime seems to be positively related to property prices. These estimates should be treated somewhat cautiously as reverse causality is likely to be a larger problem with these types of property crimes. The second thing to note is that the impact of LSOA or MSOA level crime is essentially zero after crimes in the respective street are accounted for. All estimates are statistically insignificant with small point estimates and equally small standard errors. This finding is very similar to results obtained by Linden and Rockoff (2008) and Pope (2008) who find strong drops in prices for properties close to the place of living of convicted sex offenders, but find no impact on the prices of properties slightly further away.

(TABLE 3 AROUND HERE.)

Table 4 considers the impact of adding lagged crime rates. We can expect these to matter if, for example, prospective property owners look at several months of crime data before making buying decisions and if there is sufficient variation over time. Column (1) replicates column (3) from table 3 for comparisons, column (2) adds the respective street-level crime counts lagged by one, two and three months. Other specifications also included LSOA and MSOA-level crimes. These were again generally zero and statistically insignificant (as in table 3) and are not shown to save space. The evidence suggests that past as well as current crimes have a negative impact on property prices, but that this impact varies with time. At the same time, the sum of the coefficients on current and lagged crime counts is usually similar to or larger in absolute value than the estimates in table 3 and again suggests that the latter are probably lower bounds for the total effect of crime on property prices.

(TABLE 4 ABOUT HERE.)

Table 5 explores the robustness of the findings to various changes in the sample and the specification. Note that the specifications in each column are identical to the ones in table 2 except for the respective variation stated. Coefficients for LSOA and MSOA-level crime are still zero and are omitted to save space. The first thing one might be concerned about is the presence of a few outliers in the price data as mentioned in section 2. Panel (a) of table 5 re-estimates equation (1) on a sample where observations in the top and bottom 1% of property prices have been dropped. Dropping these barely changes the estimates, suggesting that outliers do not drive the results.

(TABLE 5 ABOUT HERE.)

A second question one might ask is to what extent the city quarter fixed effects matter. Panel (b) of table 5 explores this issue by replacing the LSOA fixed effects from table

2 with MSOA fixed effects. As we can see the effects are generally very similar to those from from table 5, suggesting that confounding effects in the local area do not matter much for the results.

Thirdly, one might be concerned that the local authority-month effects do not capture all confounding effects that vary over time, for example gentrification processes that operate on the city-quarter level. Panel (c) presents estimates where the local authority-month effects have been replaced by MSOA-month effects (in addition to LSOA fixed effects). In these specification the effects of MSOA-level crime is no longer identified as it is absorbed by the MSOA-month effects, which makes specification (3) and (5) and (4) and (6) identical. More importantly, the effects of street-level crime remain again very similar to the estimates in table 3 or panel (a).

Finally, panel (d) provides the strongest test of the link between crime and property prices. Firstly, postcode/street-fixed effects replace LSOA-fixed effects and postcode-level controls. These can be expected to capture practically all local amenities and other street-level factors that might matter for both crime and property prices. Secondly, the estimates also include MSOA-month effects to allow for small-scale regional changes in unobservables. It is important to note again that these estimates are identified using month-to-month changes in postcode-level crime relative to small-scale regional trends. These estimates are also likely to exclude a lot of the variation that prospective property owners might be interested in: As properties are durable consumption goods, it is actually less likely that prospective owners care much for transitory fluctuations in local crime rates and more about whether the street where a property is situated suffers from a permanently higher crime count than neighboring streets. As such, it seems likely that these estimates represent absolute lower bounds for the effects of interest. However, even these estimates still show the expected negative relationship between property prices and most types of crime crime, even

though the effect sizes are only about 1/3 of the original estimates and estimates are not always statistically significant. These absolute-lower bound effects suggest that each additional case of anti-social behavior per 10 population lowers prices by 0.6%, each additional violent crime by 0.6% and each additional non-violent crime by 0.2%.

In total, it appears as if the results are fairly robust to a range of sensible changes in the specification: Each case of anti-social behavior per 10 population in a street leads to an approximately 0.6 to 0.8% drop in property prices, while a corresponding increase in violent crime decreases house prices by roughly 0.6 to 1.6% and a corresponding increase in non-violent crime by about 0.2 to 0.4%. The majority of estimates are at the upper end of these intervals. Compared with the earlier literature these estimates appear to be very similar, but at the lower end of the previous findings. Results for burglary, robbery and vehicle crime are somewhat counterintuitive, but might potentially be explained by stronger reverse causality for these crime.

4.2 Implied costs of crime

Percentage changes in property prices through changes in crime rates as those presented in the previous section are useful to test the theory of compensating differentials. However, cost-benefit-analyses, such as whether it pays to hire an additional policeman who prevents X future crimes of a certain type but costs a certain amount of money per annum, require a monetary value to be placed on each crime. This section calculates these costs under several assumptions. These calculations also enable comparisons with past papers such as Gibbons (2004), who uses a similar calculation for property crime, and willingness-to-pay studies such as Cohen et al. (2004) for the US or Atkinson et al. (2005) for the UK. Finally, these estimated costs provide a sensible plausibility check in terms of the magnitude of the effects. Note first that crime affects the value of properties that were sold as well as that of

properties that were not sold. At the time of the 2001 census there were 1.75 million unit postcodes in England and Wales that cover 27 million delivery points (Office for National Statistics, 2004, p. 1). Assuming that each delivery point corresponds to one property, this means that there are on average 15 properties per street/postcode. The effects in the previous section were calculated based on rates per 10 population. To arrive at an effect per crime, these need to be converted to reflect an additional crime in an average street, which had 43 inhabitants according to the 2011 census. If we are also willing to assume that the average price of properties that were sold is equal to that of unsold properties we can get a monetary estimate of the cost of each crime as

% drop in price per crime per 10 population / (average population per street (=43) / 10)

** avg. property value (=£238,916) * avg. number of properties per street (=15). (2)*

Note that we do not need to consider properties outside of the respective street as the estimates suggest that wider area crime plays no role for property prices. Admittedly, these calculations are very approximate as (a) delivery points and properties will not be exactly equal and (b) the assumption that the value of sold properties and properties not on the market is debatable as the value of sold properties will generally be above the previous owner's reservation price while this may not be true for properties not on the market. However, while admittedly being a back-of-the-envelope calculation, it is comparable to the calculations made by Gibbons (2004).

Carrying out these calculations, leads us to implied costs (rounded to the nearest £100) of £5000 to £6700 for each case of anti-social behavior, of £5000 to £13,300 for each case of violent crime and to £1700 to £3300 for each "other" crime. The respective lower values are based on the estimates with street fixed effects and MSOA-month-effects from column (6) in panel (d) in table 5 and the respective upper bounds on the base estimates from column (6) in

table 3. The estimate for anti-social behavior looks very high relative to violent crime at a first glance, but one should keep in mind that the former includes various types of harassment, while the latter also includes relatively minor types of violent crimes.

How do these results compare to the findings from the literature? Cohen et al. (2004) report several estimates for people's willingness-to-pay (WTP) for the avoidance of some types of violent crime for the US, specifically a of \$70,000 for the avoidance of one serious assault, \$237,000 for rape and sexual assault and \$9.7m for murder. Similarly, Ludwig and Cook (2001) end up with an estimate of around £1m per gun-related wounding. Converted to Dollar values my estimates for violent crime are substantially lower than the Cohen et al. (2004) estimates for serious assault, which seems plausible given that my measures will include plenty of less serious cases of violent crime. Atkinson et al. (2005) report WTP estimates for common assault, serious wounding and other wounding. Their estimates range from a mean WTP of £5,282 for common assault to one of £35,844 for serious wounding. My estimates appear to be remarkably similar with the remaining differences potentially due to differences in methodology (stated vs. revealed preference) or due to differences in what is captured as violent crime. It is plausible, however, that my estimates are dominated by the much more frequent less serious cases of violent crime, such as common assault, and less by more serious, but less frequent cases such as homicides.

The best comparison for anti-social behavior is probably Gibbon's (2004) estimate for criminal damage, which is £104,000 per case. Compared with these results the estimates for anti-social behavior (and for other types of non-violent crime) seem comparatively low. However, Gibbon's (2004) results are fairly large in comparison with the estimates by Atkinson et al. (2005) for much more serious crimes. Furthermore, anti-social behavior, while including fairly serious incidents such as harassment, also includes a wide range of "crimes" that can be considered to be less severe than criminal damage, such as playing loud music at

night or the consumption of alcohol in certain places.

5. Conclusion

Based on street level data for property sales and criminal offences, I investigated the relationship between property prices and three types of non-property crime, specifically anti-social behavior, violent crime and drug crime while controlling for unobserved neighborhood characteristics and non-parametric regional trends. My estimates, which are robust to a range of sensible specification changes, suggest that each case of anti-social behavior per 10 population in the same street leads to an approximately 0.6 to 0.8% drop in property prices, while a corresponding increase in violent crime decreases prices by roughly 0.6 to 1.6% and a corresponding increase in non-violent crime by about 0.2 to 0.4%. The majority of estimates are at the upper end of these intervals. Estimates for robbery, burglary and vehicle crime are either zero or positive, but are possible biased due to reverse causality. Compared with the earlier literature these estimates appear to be very similar. Crime outside of the respective street does not appear to matter, which is consistent with earlier findings by Linden and Rockoff (2008) and Pope (2008). It is also consistent with people looking up the postcode of a property on the crime maps at www.police.uk. Expressed in monetary terms each case of anti-social behavior costs society between £5000 to £6700 and each violent crime between £5000 to £13,300. These estimates are roughly in line with previous evidence from both stated and revealed preference studies.

REFERENCES

Abadie, Alberto and Dermisi, Sofia. 2008. "Is terrorism eroding agglomeration economies in central business districts? Lessons from the office real estate market in downtown

- Chicago.” *Journal of Urban Economics* 64(2), 451-463.
- Atkinson, Giles, Andrew Healey, and Susana Mourato. 2005. “Valuing the costs of violent crime: A stated preference approach.” *Oxford Economics Papers* 57 (4), pp. 559-585.
- Becker, Gary S. 1968. “Crime and Punishment: An Economic Approach.” *Journal of Political Economy* 76(2): 169-217.
- Blomquist, Glenn C., Mark C. Berger, and John P. Hoehn. 1988. “New Estimates of Quality of Life in Urban Areas.” *American Economic Review* 78(1): 89-107.
- Bowes, David R., and Keith R. Ihlanfeldt. 2001. “Identifying the Impacts of Rail Transit Stations on Residential Property Values.” *Journal of Urban Economics* 50(1): 1-25.
- Braakmann, Nils. 2009. “Is there a compensating wage differential for high crime levels? First evidence from Europe.” *Journal of Urban Economics* 66(3): 218–231.
- Braakmann, Nils. 2012a. “How individuals deal with victimization and victimization risk? Longitudinal evidence from Mexico.” *Journal of Economic Behavior and Organization* 84(1), pp. 335-344.
- Braakmann, Nils. 2012b. “The health cost of crime - Longitudinal evidence from Mexico.” *Mimeo*, Newcastle upon Tyne.
- Braakmann, Nils. 2012c. “Spatial aggregation and the unemployment-crime relationship.” *Mimeo*, Newcastle upon Tyne.
- Carmichael, Fiona and Robert Ward, 2001. Male unemployment and crime in England and Wales. *Economics Letters* 73(1), pp. 111-115.
- Cohen, Mark A., Roland T. Rust, Sara Steen, and Simon T. Tidd. 2004. “Willingness-to-pay for crime control programs.” *Criminology* 42(1): 89–110.

- Collins, Nick, 2011. "House prices could be hit by crime maps." *The Telegraph*, February 1, 2011. <http://www.telegraph.co.uk/news/uknews/crime/8294104/House-prices-could-be-hit-by-crime-maps.html>.
- Cullen, Julie B. and Steven D. Levitt, 1999. "Crime, urban flight, and the consequences for cities." *Review of Economics and Statistics* 81(2): 159-169.
- Dustmann, Christian and Francesco Fasani, 2015. The effect of local area crime on mental health. Forthcoming: *Economic Journal*.
- Edmark, Karen, 2005. Unemployment and crime: Is there a connection? *Scandinavian Journal of Economics* 107(2), pp. 353-373.
- Freeman, Richard B. 1999. "The Economics of Crime." In: Ashenfelter, Orley, and David Card (eds.). *Handbook of Labor Economics, Vol. 3C*. North-Holland: 3529-3571.
- Gerking, Shelby D., and William N. Neirick. 1983. "Compensating Differences and Interregional Wage Differentials." *Review of Economics and Statistics* 65(3): 483-487.
- Gibbons, Steve. 2004. "The Cost of Urban Property Crime." *The Economic Journal* 114(499): F441-F463.
- Gould, Eric D., Bruce A. Weinberg, and David B. Mustard. 2002. "Crime rates and local labor market opportunities in the United States: 1979–1997." *Review of Economics and Statistics* 84(1): 45–61.
- Hamermesh, Daniel S. 1999. "Crime and the Timing of Work." *Journal of Urban Economics* 45(2): 311-330.
- Linden, Leigh and Jonah E. Rockoff. 2008. "Estimates of the Impact of Crime Risk on

- Property Values from Megan's Laws.” *American Economic Review* 98(3), pp. 1103-1127.
- Ludwig, Jens and Philip J. Cook. 2001. “The Benefits of Reducing Gun Violence: Evidence from Contingent-Valuation Survey Data.” *Journal of Risk and Uncertainty* 22(3): 207-226.
- Lynch, Allen K., and Rasmussen, David W. 2001. “Measuring the impact of crime on house prices.” *Applied Economics* 33(15): 1981-1989.
- Manning, Alan, 2003. “*Monopsony in motion: imperfect competition in labor markets*”, Princeton University Press.
- Manning, Alan, 2011. “Imperfect competition in the labor market”, in: O. Ashenfelter & D. Card (eds.), *Handbook of Labor Economics*, Volume 4B, Elsevier, pp. 973–1041.
- Raphael, Steven and Rudolf Winter-Ebmer, 2001. Identifying the Effect of Unemployment on Crime. *Journal of Law and Economics* 44(1), pp. 259-283
- Reilly, Barry and Robert Witt, 1996. Crime, deterrence and unemployment in England and Wales: An empirical analysis. *Bulleting of Economic Research* 48(2), pp. 137-159.
- Roback, Jennifer. 1982. “Wages, Rents, and the Quality of Life.” *Journal of Political Economy* 90(6): 1257-1278.
- Roback, Jennifer. 1988. “Rents, and Amenities: Differences Among Workers and Regions.” *Economic Inquiry* 26(1): 23-41.
- Office for National Statistics, 2004. “*Census 2001. Key statistics for postcode sectors in England and Wales.*” ONS, London.
- Office for National Statistics, 2015. “*Pen portraits for the 2011 area classification of output*

areas.” ONS, London.

Piehl, Anne M. 1998. “Economic conditions, work and crime.” In: Tonry, Michael (Ed.). *The Handbook of Crime and Punishment*. Oxford University Press, Oxford” 302–319.

Pope, Jaren C. 2008. “Fear of crime and housing prices: Household reactions to sex offender registries.” *Journal of Urban Economics* 64(3), pp. 601-614.

Powdthavee, Nattavudh. 2007. “Unhappiness and Crime: Evidence from South Africa.” *Economica* 72(287): 531-547.

Schmidt, Lucie, and Paul N. Courant. 2006. “Sometimes Close is Good Enough: The Value of Nearby Environmental Amenities.” *Journal of Regional Science* 46(5): 931-951.

Smith, Claudia. 2005. “Immigration, Crime and Compensating Wage Differential: Evidence from the Mariel Boatlift.” *Mimeo*, Syracuse.

Vincenty, Thaddeus (1975): “Direct and inverse solutions of geodesics on the ellipsoid with application of nested equations”, *Survey Review* 22(176), pp. 88-93.

Table 1: Descriptive statistics

Variable	Mean	Std. Dev.	Min	Max
Price (£)	238,916	283,340	7000	54,959,000
Ln(price)	12.13	0.65	8.85	17.82
Crimes per 10 population, street level				
Anti-social behavior	0.03	0.27	0	120
Burglary	0.01	0.08	0	20
Robbery	0.00	0.03	0	15
Vehicle crime	0.01	0.11	0	110
Violent crime	0.01	0.10	0	40
Other crime	0.03	0.30	0	140
Leasehold	0.22	0.41	0	1
Semi-detached house	0.28	0.45	0	1
Flat	0.18	0.38	0	1
Detached house	0.24	0.43	0	1
Newly built house	0.06	0.23	0	1
Observations	1,892,958			
<i>Excluding top/bottom 1% of house prices</i>				
Price (£)	222,420	153,818	44,975	1,164,999
Ln(price)	12.13	0.59	10.71	13.97
Crimes per 10 population, street level				
Anti-social behavior	0.03	0.27	0	120
Burglary	0.01	0.08	0	20
Robbery	0.00	0.03	0	15
Vehicle crime	0.00	0.07	0	40
Violent crime	0.01	0.10	0	40
Other crime	0.03	0.29	0	140
Leasehold	0.22	0.41	0	1
Semi-detached house	0.29	0.45	0	1
Flat	0.17	0.38	0	1
Detached house	0.24	0.43	0	1
Newly built house	0.06	0.23	0	1
Observations	1,855,023			

Table 2: Socio-demographic characterization of postcodes with (multiple) purchases

ONS classification on 2011 census	Supergroup based	Description	All postcodes	Postcodes with property transactions	Postcodes with multiple property transactions
1		Rural residents	16.19	14.33	11.43
2		Cosmopolitans	9.40	4.37	5.21
3		Ethnicity central	6.50	4.05	4.20
4		Multicultural metropolitans	10.78	11.42	11.59
5		Urbanites	18.72	21.95	24.15
6		Suburbanites	15.78	23.30	24.39
7		Constrained city dwellers	9.12	4.90	4.30
8		Hard-pressed living	13.51	15.67	14.73

Note: The ONS area classification is based on the analysis of a range of variables from the 2011 census and gives a characterization of the overall population structure of an area. Full descriptions of the characteristics of each supergroup can be found in Office for National Statistics (2015).

Table 3: Crime and house prices, dependent Variable: ln(house price in £)

	(1)	(2)	(3)	(4)	(5)	(6)
			Street-level			
Anti-social behavior	-0.011*** (0.003)	-0.009*** (0.002)	-0.008*** (0.002)	-0.008*** (0.002)	-0.008*** (0.002)	-0.008*** (0.002)
Burglary	0.027*** (0.006)	0.002 (0.005)	-0.001 (0.005)	-0.000 (0.005)	-0.001 (0.005)	-0.000 (0.005)
Robbery	0.039* (0.021)	0.014 (0.015)	0.017 (0.014)	0.017 (0.014)	0.017 (0.014)	0.017 (0.014)
Vehicle crime	0.021*** (0.004)	0.012** (0.005)	0.012** (0.006)	0.012** (0.006)	0.012** (0.006)	0.012** (0.006)
Violent crime	-0.030*** (0.008)	-0.020*** (0.006)	-0.016*** (0.005)	-0.016*** (0.005)	-0.016*** (0.005)	-0.016*** (0.005)
Other crime	-0.007** (0.003)	-0.004** (0.002)	-0.004** (0.002)	-0.004** (0.002)	-0.004** (0.002)	-0.004** (0.002)
			LSOA-level			
Anti-social behavior				0.000 (0.000)		0.000 (0.000)
Burglary				-0.001** (0.000)		-0.001** (0.000)
Robbery				-0.000 (0.001)		-0.001 (0.001)
Vehicle crime				-0.000 (0.000)		0.000 (0.000)
Violent crime				0.000 (0.000)		0.000 (0.000)
Other crime				0.000 (0.000)		-0.000 (0.000)
			MSOA-level			
Anti-social behavior					-0.000 (0.000)	-0.000* (0.000)
Burglary					-0.000 (0.000)	0.000 (0.000)
Robbery					0.000 (0.000)	0.000 (0.000)
Vehicle crime					-0.000 (0.000)	-0.000 (0.000)
Violent crime					0.000 (0.000)	0.000 (0.000)
Other crime					0.000* (0.000)	0.000* (0.000)
Property characteristics	No	Yes	Yes	Yes	Yes	Yes
Postcode socio-demographic classification	No	No	Yes	Yes	Yes	Yes
LSOA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Local authority * month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,892,958					

Coefficients, standard errors adjusted for clustering on the respective highest regional

aggregation of regressors in parentheses. */**/** denote statistical significance on the 10%, 5% and 1% level respectively. Property characteristics are dummies for the property being a flat, a semi-detached house or a detached house (with terraced house as the base alternative), for the property being new and for the property being a leasehold.

Table 4: Crime and house prices, lags, dependent Variable: ln(house price in £)

	(1)	(2)
Not lagged		
Anti-social behavior	-0.008***	-0.001
	(0.002)	(0.002)
Burglary	-0.000	0.003
	(0.005)	(0.004)
Robbery	0.017	0.021
	(0.014)	(0.014)
Vehicle crime	0.012**	0.005
	(0.006)	(0.005)
Violent crime	-0.016***	-0.009**
	(0.005)	(0.004)
Other crime	-0.004**	0.000
	(0.002)	(0.002)
Lagged one month		
Anti-social behavior		-0.000
		(0.002)
Burglary		-0.004
		(0.005)
Robbery		-0.006
		(0.010)
Vehicle crime		0.013**
		(0.006)
Violent crime		0.001
		(0.005)
Other crime		-0.000
		(0.003)
Lagged two months		
Anti-social behavior		-0.006**
		(0.003)
Burglary		0.006
		(0.006)
Robbery		-0.002
		(0.010)
Vehicle crime		0.008
		(0.006)
Violent crime		-0.009**
		(0.004)
Other crime		-0.004
		(0.002)
Lagged three months		
Anti-social behavior		-0.001
		(0.002)
Burglary		0.004
		(0.005)
Robbery		-0.013
		(0.009)
Vehicle crime		0.006
		(0.006)
Violent crime		-0.010**
		(0.004)
Other crime		0.002
		(0.002)
N	1,892,958	1,892,958

Coefficients, standard errors adjusted for clustering on the respective highest regional

aggregation of regressors in parentheses. */**/** denote statistical significance on the 10%, 5% and 1% level respectively. Property characteristics are dummies for the property being a flat, a semi-detached house or a detached house (with terraced house as the base alternative), for the property being new and for the property being a leasehold.

Table 5: Robustness checks: Crime and house price, dependent Variable: ln(house price in £)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel (a): Excluding top/bottom 1% of house prices						
Anti-social behavior	-0.009*** (0.003)	-0.007*** (0.002)	-0.006*** (0.002)	-0.006*** (0.002)	-0.006*** (0.002)	-0.006*** (0.002)
Burglary	0.027*** (0.006)	0.003 (0.004)	0.001 (0.004)	0.001 (0.004)	0.001 (0.004)	0.001 (0.004)
Robbery	0.020 (0.017)	0.002 (0.014)	0.006 (0.013)	0.006 (0.013)	0.006 (0.013)	0.006 (0.013)
Vehicle crime	0.013** (0.005)	-0.004 (0.005)	-0.006 (0.005)	-0.006 (0.005)	-0.006 (0.005)	-0.006 (0.005)
Violent crime	-0.024*** (0.007)	-0.015*** (0.005)	-0.011** (0.005)	-0.011** (0.005)	-0.011** (0.005)	-0.011** (0.005)
Other crime	-0.006** (0.003)	-0.003 (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.002 (0.002)
N	1,855,022					
Panel (b): MSAO fixed effects instead of LSOA fixed effects						
Anti-social behavior	-0.011*** (0.003)	-0.009*** (0.002)	-0.008*** (0.002)	-0.008*** (0.002)	-0.008*** (0.002)	-0.008*** (0.002)
Burglary	0.027*** (0.006)	0.002 (0.005)	-0.001 (0.005)	-0.000 (0.005)	-0.001 (0.005)	-0.000 (0.005)
Robbery	0.039* (0.021)	0.014 (0.015)	0.017 (0.014)	0.017 (0.014)	0.017 (0.014)	0.017 (0.014)
Vehicle crime	0.021*** (0.004)	0.012** (0.005)	0.012** (0.006)	0.012** (0.006)	0.012** (0.006)	0.012** (0.006)
Violent crime	-0.030*** (0.008)	-0.020*** (0.006)	-0.016*** (0.005)	-0.016*** (0.005)	-0.016*** (0.005)	-0.016*** (0.005)
Other crime	-0.007** (0.003)	-0.004** (0.002)	-0.004** (0.002)	-0.004** (0.002)	-0.004** (0.002)	-0.004** (0.002)
N	1,892,958					
Panel (c): MSAO*month fixed effects instead of LA*month fixed effects						
Anti-social behavior	-0.012*** (0.003)	-0.010*** (0.002)	-0.009*** (0.002)	-0.009*** (0.002)	-0.009*** (0.002)	-0.009*** (0.002)
Burglary	0.033*** (0.006)	0.003 (0.004)	0.001 (0.004)	0.001 (0.004)	0.001 (0.004)	0.001 (0.004)
Robbery	0.037** (0.018)	0.010 (0.013)	0.012 (0.012)	0.012 (0.012)	0.012 (0.012)	0.012 (0.012)
Vehicle crime	0.023*** (0.004)	0.013*** (0.005)	0.012** (0.005)	0.012** (0.005)	0.012** (0.005)	0.012** (0.005)
Violent crime	-0.031*** (0.007)	-0.020*** (0.005)	-0.016*** (0.005)	-0.017*** (0.005)	-0.016*** (0.005)	-0.017*** (0.005)
Other crime	-0.007** (0.003)	-0.004** (0.002)	-0.004** (0.002)	-0.004** (0.002)	-0.004** (0.002)	-0.004** (0.002)
N	1,892,958					
Panel (d): Postcode fixed effects instead of LSOA-fixed effects, MSAO*month fixed effects instead of LA*month fixed effects						
Anti-social behavior	-0.007*** (0.002)	-0.006*** (0.002)	-0.006*** (0.002)	-0.006*** (0.002)	-0.006*** (0.002)	-0.006*** (0.002)
Burglary	0.011** (0.005)	0.002 (0.004)	0.002 (0.004)	0.002 (0.004)	0.002 (0.004)	0.002 (0.004)
Robbery	0.005 (0.011)	0.001 (0.009)	0.001 (0.009)	0.002 (0.009)	0.001 (0.009)	0.002 (0.009)
Vehicle crime	0.013** (0.006)	0.009* (0.005)	0.009* (0.005)	0.008* (0.005)	0.009* (0.005)	0.008* (0.005)
Violent crime	-0.013*** (0.005)	-0.006 (0.004)	-0.006 (0.004)	-0.006 (0.004)	-0.006 (0.004)	-0.006 (0.004)
Other crime	-0.004** (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.002 (0.002)
N	1,185,903					
Property characteristics	No	Yes	Yes	Yes	Yes	Yes
Postcode socio-demographic classification	No	No	Yes	Yes	Yes	Yes
LSOA fixed effects	Panels (a) and (c)	Panels (a) and (c)	Panels (a) and (c)	Panels (a) and (c)	Panels (a) and (c)	Panels (a) and (c)

Local authority * month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
---------------------------------------	-----	-----	-----	-----	-----	-----

Coefficients, standard errors adjusted for clustering on the respective highest regional aggregation of regressors in parentheses. */**/** denote statistical significance on the 10%, 5% and 1% level respectively. Specifications are identical to table 2 except for variation stated. LSOA and MSOA-crime levels are generally zero (as in table 2) and are omitted from the table. Property characteristics are dummies for the property being a flat, a semi-detached house or a detached house (with terraced house as the base alternative), for the property being new and for the property being a leasehold.