Housing Sales Volume and Police Spending: A Regression Discontinuity Investigation

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Abstract

Housing sales volume has been shown to be affected by many factors. These include population, the money supply, the stock market, and house prices. We show that local government spending on police protection also affects trading volume. We use regression discontinuity to find that communities that vote to cut police spending have about a two standard deviation increase in transaction volume the year after the vote. The increased housing turnover persists at lower levels five years after the vote. Increased fear of crime may be especially responsible, perhaps more than a signaling mechanism, and certainly more than increased crime rates.

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1. Introduction

Over five million houses have been sold in the United States every year since 2015. Even during the worst of the housing crisis in 2008, over four million were sold (statista, 2021). Sales volume is an important topic. It helps determine the incomes of over one million realtors and numerous housing contractors and construction workers in the U.S. Housing has a large multiplier effect because when people buy a house they often buy appliances, furniture, and gardening equipment, too--about $6,320 worth in 2021 dollars by one estimate (National Association of Realtors, 2010). Sales volume is a symptom of household formation, which informs both demographic trends as well as the economic health of a region.

The literature has uncovered many factors that affect sales volume, including house prices, tax rates, population, mortgage interest rates, the money supply, the unemployment rate, the stock market, and the loan-to-deposit ratio of banks. The current study uncovers a new factor that drives the number of houses sold: spending on police protection. We hope this discovery will spur more research that investigates links between sales volume and other types of local government spending like schools, transit, open spaces, fire protection, and the provision of roads and other public infrastructure.

We collect the universe of single-family detached housing sales that take place in Ohio from 1995 to 2016. We match this to voting data for hundreds of local governments in Ohio. We use regression discontinuity to examine whether votes to fund police protection are related to the number of houses sold in subsequent years. We find that communities that barely vote to renew police funding have fewer houses sold than otherwise similar communities that barely vote to cut funding. Voting to cut police funding causes about a two standard deviation increase in the number of houses sold the year after the vote. The effect diminishes to one standard deviation but persists for as many periods as we tested--four and five years after the vote. We hypothesize that the increased sales volume stems from some combination of increased fear of crime (Ditton and Farrall, 2017), a community signaling a decreased commitment to police protection (Spence, 1973), and an actual increase in crime brought about by decreased police funding (Boggess, Greenbaum and Tita, 2013). After all, public safety is an important public service that underlies Tiebout (1956) sorting, and crime rates have been linked to house prices (Zabel, 2015). If crime rates are capitalized into house prices, it means police protection is being inefficiently under-provided (Brueckner, 1979). Cutting funding to an already under-provided public service could lead to an important outflow of residents, as evidenced by an increased sales volume, and their replacement by residents with a weaker taste for and/or ability to pay for crime prevention. Testing in Section 7 suggests that fear of crime may be the strongest driver of a change in sales volume.

As far as we can tell, causal inference techniques have only been used to explore taxes as a driver of sales volume, but this represents a minority of the literature. The regression discontinuity design that we use has drawbacks: it is rare to find a situation that allows its use, and results can usually only be generalized to economic agents with characteristics like those near the cutoff. On the other hand, the assumptions that underlie the model are few and testable, and the technique has been shown to not only alleviate but to statistically eliminate omitted variable bias well enough to replicate the results of randomized experiments (Shadish, et al. 2011; Berk, et al. 2010; Buddelmeyer and Skoufias, 2004). These characteristics compare favorably even with most other types of causal inference techniques.

2. Literature Review

There is a vibrant literature on factors related to the volume of housing sales. Wheaton (1990) builds a search model that features three types of households: those matched properly to one house, those mismatched with one house, and those matched but owning two houses, one of which is vacant. The key insight is that owners only put their house on the market after finding a new house that matches their preferences. Any shock to the economy like a change in housing supply or demographics causes a change in the number of vacant houses, and it is through this mechanism that sales volume changes. De Wit, Englund and Francke (2013) contains an excellent review of the literature. It studies about two million houses offered for sale from 1985 and 2007 in the Netherlands. Using a vector error correction model, it finds that exogenous shocks to the market like a change in mortgage interest rates cause an immediate increase in sales volume and a gradual rise in house prices. Hort (2000) uses vector autoregression to find that a rise in the mortgage rate causes an instantaneous drop in sales volume but only a gradual decrease in house prices.

Tsai (2018) explores changes in the money supply as a cause of changes in sales volume. Excess money supply causes inflation, which increases housing prices, which tricks housing contractors into thinking that demand for houses has increased. In response, more houses are built and sold, resulting in a higher transaction volume. Clayton, Miller and Peng (2010) finds that exogenous shocks like changes in the mortgage market, labor market, and stock market cause changes in both transaction volume and house prices. Hui, Yu and Ng (2014) uses vector autoregression, impulse response analysis, and variance decomposition analysis to find that the volume of housing transactions is affected by the money supply, house prices, interest rates, stock market returns, and banks’ loan to deposit ratio. Boggess, Greenbaum and Tita (2013) finds higher levels of crime are related to a higher volume of housing sales one year later.

Perhaps the most actively studied factor affecting sales volume is housing prices. Miller and Sklarz (1986) studies condominium sales in Hawaii and Salt Lake City, and finds that the number of sales in a prior period affects sale prices in the next quarter. Stein (1995) comes to the opposite conclusion: that house prices cause a change in sales volume. It presents a model of financing constraints, arguing that an increase in house prices causes improved home equity for existing homeowners. These homeowners use the increased equity in their homes to purchase larger houses, which in turn puts upward pressure on house prices. Berkovec and Goodman (1996) regresses median sale price as a function of a change in turnover in the same period, finding a positive link. Using a four-equation structural model, Follain and Velz (1995) finds sales volume negatively related to housing price. The difference in sign seems to be due to the time period studied, as the regressions of Berkovec and Goodman also show a negative correlation using the data of Follain and Velz. Hort (2000) uses a panel of housing sales in Sweden, finding no consistent link between changes in house price and sales volume. Andrew and Meen (2003) constructs a two-equation vector autoregression. One equation measures a change in housing prices, and the other measures the volume of sales as a fraction of the housing stock. It finds that deviations from housing market equilibrium move sales volume and prices in the same direction. Genesove and Mayer (2001) explains the positive correlation between house prices and sales volume as stemming from loss aversion. Using 1990s data from Boston condominium sales, it shows that loss aversion is a more important driver of the correlation than liquidity constraints. Akkoyun, Arslan and Kanik (2013) explores the Granger causality between house prices and transaction volume. It finds a stronger correlation when transaction volume is low, and it shows the direction of causality varies by region of the U.S. In a similar vein Clayton, Miller and Peng (2010) studies 114 cities in the U.S. from 1990 to 2002, finding that a decrease in housing prices Granger causes lower sales volume, but an increase in house prices does not Granger cause a change in sales volume. Charles, Garion and Youngman (2002) studies Hong Kong data from the 1990s, finding sales volume usually leads--and Granger causes--a change in housing prices in the same direction.

There is also a literature about the effect of transaction taxes on sales volume. Microeconomic theory predicts that an increase in taxes should raise house prices and decrease sales volume, and a number of studies quantify the effects seen in the market. And while the studies previously mentioned sometimes use Granger causality and vector autoregression, it is hard to find a study that uses a causal inference technique. They are easier to find in the tax-and-sales-volume literature. For example, Dachis, Duranton and Turner (2008) uses difference in differences estimation at the border of Toronto and surrounding municipalities to find that a 1.1% land tax causes a 15% fall in the number of housing sales. Davidoff and Leigh (2013) uses an instrumental variables model with post code fixed effects. It uses Australian housing data from 1993 to 2005, finding the Australian stamp tax lowers housing turnover, with a -0.3 elasticity. Hilber and Lyytikäinen (2017) notes that there is a discontinuity--a kink--in the transfer tax schedule of the U.K. The tax rate is 1% for houses that sell for less than 250,000 quid and 3% above that selling price. Using regression discontinuity and British housing sales from 1996 to 2007 it finds that raising tax rates by two percent causes about a 30% decline in sales volume. Fritzsche and Vandrei (2019) uses a fixed effects panel model to study the German real estate transfer tax from 2005 to 2014. It finds a one percentage point increase in the tax reduces transactions by seven percent, which equates to a -0.25 sales volume elasticity with respect to tax rates. Petkova, Kunka and Weichenrieder (2017) also studies the German transfer tax. Using a panel of German states from 2005 to 2015 with year, state, and city fixed effects, it finds a -0.23 elasticity of transaction volume with respect to tax rates for single-family houses, but no effect for investor-owned apartments. It finds no link between population and transaction volume, but a negative link between the unemployment rate and transaction volume.

Our study extends the vibrant literature on the factors that drive sales volume. The study most related to ours is Boggess, Greenbaum and Tita (BGT, 2013) from the criminology literature, but there are differences. While BGT studies crime rates, we study spending on police protection. We provide evidence in Section 7 of this paper that the effect of police spending on sales volume is driven by different factors than an increase in the crime rate itself. BGT uses five years of house sales in Los Angeles; we use 22 years of house sales in Ohio. BGT restricts its exploration to the effect of lagged crime rates on current levels of house sales, which effectively looks at a one-year impact. We look for short-term effects of police spending on transaction volume, too, but we also look for longer-term effects up to five years after a change in spending. The methodology is different, as well. While BGT uses ordinary least squares with time fixed effects, we use the causal inference technique regression discontinuity, which we now discuss in greater detail.

3. Methodology

Regression discontinuity is a powerful tool that essentially converts observed data into a randomized experiment. In order to do so, one must observe a running variable *X*, also called a forcing variable. This running variable must have a fixed, known cutoff *c* that determines whether an observation receives treatment or is part of the control group. In the context of the current study the running variable is vote share: the proportion of votes in year *t* in favor of renewing a police tax levy. In Ohio a simple majority is required to pass, making *c* = 0.50. Let *y* be the outcome, the number of houses sold in a city, village or township, which we index with *i*. With *t* indexing time, the estimating equation takes the form of Equation (1):

*yit*+*τ* = *ϐDit* + *ϴXit* + *ϵit* . (1)

Most votes in Ohio occur in November, so we would not expect a contemporaneous causal effect. Equation (1) therefore contains *τ*, which is a lead or lag year before or after the vote. Negative values of *τ* will be useful for testing whether omitted factors or random jumps in the data are responsible for significant treatment effects, and positive values will test for persistent treatment effects across time. The symbol *D* is a dummy variable which takes the value 1 if the tax levy passes and zero if it fails, making *ϐ* the treatment effect. Because it has no subscripts, the treatment effect is an estimate averaged over all votes across time and across local governments. The running variable *X* also appears. In regression discontinuity theory, the value of the running variable encapsulates information about the agents. For example, cities, villages and townships that vote 0.43 in favor of renewing a police tax levy have similar levels of population, income, racial composition and the like. Local governments that vote 0.65 in favor likewise have similar values of observable and unobservable characteristics. Crucially the same is true for communities that vote within a narrow range of 0.50, so that the only difference between communities narrowly distributed around 0.50 is that some of them renew the tax and others vote to cut funding. If the only difference between treated and control groups near the cutoff is whether funding drops or stays the same, then *ϐ* is perfectly identified with no confounding omitted variable bias. To finish Equation (1), *ϵ* is a Gaussian error term.

Unfortunately, Equation (1) is an incomplete description of the estimation because the treatment effect at the cutoff is a mathematical abstraction. While observations at the 0.50 cutoff may very well have the same levels of observed characteristics, and we can test for this, the exact value 0.50 of the running variable is a failing vote. We require votes both in favor and against the tax. It is tempting to include the entire data set in the regression, called the global sample, but the characteristics of communities that vote 0.43 in favor are probably different from the characteristics of communities that vote 0.65 in favor. We require enough votes near 0.50 to provide statistical power to the estimates, but the farther from 0.50 we go, the more likely communities are to differ significantly in characteristics, which would violate the random assignment of observations around the cutoff condition and bias the treatment effect estimate. The state of the art is to select a bandwidth *h* around the cutoff using the technique of Calonico, Cattaneo, Farrell and Titiunik (2019), as described in Equation (2):

^1/(3+2*p*). (2)

In Equation (2), *V* is the variance of the estimator, *j* is a subscript representing bandwidth option, all five of which five are employed, *n* is the number of observations, and *p* is the polynomial order. The treatment effect is the difference in outcomes between pass and fail groups within this bandwidth, as governed by Equation (3):

*Y*+,*p*(*h*) -  *Y*-,*p*(*h*) - . (3)

In Equation (3) *ωo* is a single-entry vector, and the plus and minus symbols represent outcomes to the right and left of the cutoff. The last term is a bias-correction term. The bias correction term is necessary because traditional estimation that minimizes mean squared error (e.g., Imbens and Kalyanaraman, 2012) is biased in the presence of covariates. The bias correction term requires a bandwidth *b*, which is separate from the bandwith *h* for the treatment effect. Calonico, Cattaneo, Farrell and Titiunik (2017) estimates the *ϐ* terms by minimizing in Equation (4):

(4)

where , , *Z* is a set of covariates included to augment precision of the estimates, given that there are less than 1,000 observations, and *K* is a triangular kernel estimation function with *h* as its bandwidth. The triangular kernel is favored over other options because it minimizes the mean squared error of treatment effect estimation (Cattaneo, Idrobo and Titiunik, 2019).

4. Institutional Detail

In 1785 the United States passed the Land Ordinance Act, which tasked government surveyors to divide land in Ohio into townships, the dimensions of which were to be six miles on each side. The township is the original form of local government in Ohio, headed by a board of trustees and able to propose property taxes to fund services. Residents of one or multiple townships may petition to incorporate into a village, which has the ability to levy income taxes and is generally headed by a mayor and city council. When villages exceed 5,000 residents they become cities. There are about 250 cities, 700 villages, and 1,300 townships across the state. Cities, villages, and townships, which we sometimes call ‘cities’ for brevity, generally have their own police departments. Counties are collections of about fifteen townships each, which have their own sheriff’s offices, but there are few county-wide votes on funding for crime prevention, so county-level observations are excluded from the analysis.

A mill is one dollar collected for every $1,000 in assessed property value. Each local government in Ohio levies up to 10 mills of property tax without having to pass a tax levy, to be used however the local government wishes. Ten mills is a small amount. The average effective local property tax rate in Ohio is 148 mills, or 1.48% (smartasset, 2021). If the city wants more tax revenue, it must ask voters. Tax levies in Ohio come in several different categories. Cities may spend money on police departments through current expense tax levies, but oftentimes they propose a tax levy specifically for police department funding. The proposed tax must pass with a simple majority vote. To vote in Ohio one must be a U.S. citizen who has resided in Ohio for at least 30 days before the election, be 18 years of age, and not have violated election laws, been declared incompetent by a court, or have been incarcerated for a felony conviction.

A proposed tax levy must specify not only the amount of the tax but the duration as well. The most common duration of a police tax levy is five years, representing over 80% of our sample. When a tax is set to expire, the police department will ask voters to renew the levy, so the typical city in Ohio that passes a police department tax levy votes periodically on police funding. More than one police tax levy may be in effect at the same time. If voters vote against renewing a police tax levy, the tax levy expires and funding is cut. Although other police tax levies may still be in effect, and funding from current expense tax levies may still help fund the police department, a vote to cut funding represents a shock to the police department. Money could be diverted from current expense revenue, but conversations with city managers suggest that when voters choose not to renew a tax levy for a dedicated purpose like police protection, funding will be cut for that purpose.

5. Data

Sheila Milligan and Serena Henderson from the Ohio Secretary of State’s office provided voting data for older local government tax levies that are not posted electronically. Thanks to them, a team of students, and our own efforts, data from 52,641 tax levies from 1991 through 2018 are entered into Excel spreadsheets. Police tax levies represent 2,380 of these. As tempting as it is to use all 2,380 tax levies, it would be a shame to use regression discontinuity to obtain unbiased treatment effects when a form of selection into treatment might contaminate the results. The problem is that a police district chooses many aspects of a vote on funding, including when to ask for new funding. Police districts might try to time a vote to maximize the chances of passage, like when economic conditions are favorable or when a shock to the local community causes strong pro-police feelings. There is a fairly rare class of taxes in Ohio that last for a “continuing period of time.” Such taxes stay in effect until a new vote repeals or replaces the tax rate with a lower or higher one. These votes are excluded from our sample because their timing is endogenously chosen, too. The most common case is that a tax is passed to last a specific amount of time, usually five years. At the end of five years the police department will ask voters to renew the tax. These are the tax levies included in our sample. When a new tax is passed in 1996, for example, the vote to renew will occur in 2001. The timing of the vote to renew is not chosen in 2001 by the police department: it is exogenous to it, having been set in 1996. It is this set of 738 votes with exogenous timing that forms the focus of our investigation, and it is the same strategy chosen in Brasington (2017).

The role of covariates in regression discontinuity is different than their role in ordinary least squares regression. OLS regressions include covariates to control for omitted variable bias to isolate the independent relationship between a key covariate and the dependent variable. In regression discontinuity, in contrast, covariates are not necessary for estimation: the running variable *Xit*­ in Equation (1) encapsulates the information needed to fully identify the treatment effect estimate (Lee and Lemieux, 2010, p. 297); however, the current sample is only 738 observations. Including covariates explicitly in the regression increases the precision of the estimate of the treatment effect. Unlike OLS regression, covariates in regression discontinuity need not be related to the outcome variable but must be related to the running variable, the proportion of votes in favor of renewing the tax levy. To this end, % Minority is one covariate included, representing the proportion of residents in each community that self-describes as racially non-white. Next, % Under 5 and % 5 to 17 capture information about the age distribution of residents. Population is included, as is Income, measured in constant 2010 U.S. dollars. % Owner is the proportion of dwellings in a community occupied by owners rather than renters. % Single Parent is the proportion of households headed by a single parent, and % Divorced and % Separated help capture marital status. The Labor Force Participation rate is the proportion of residents aged 16 and over who are part of the labor force; that is, either employed or unemployed but actively looking for a job.

The number of observations in Tables 1 and 2 deviates from 738 for many reasons. First, the outcome variable is not observed after 2016, so tax levies from 2017 and 2018 are excluded. Second, the means in Table 1 come from the lead value of *τ* = 3. Because the sample ends in 2016, any tax levy in 2016, 2015, or 2014 has no data for outcomes three years after the tax levy. Third, although tax data is available from 1991, housing transaction data is only available starting in 1995. Fourth, the number of police departments changes slightly from year to year as cities and villages form and dissolve and townships are annexed. Fifth, there is some randomness in the number of police tax levies voted upon in each year.

[insert Table 1 about here]

The outcome is the number of housing sales per capita in each community in each year. This information comes from a CoreLogic housing data set spanning the years 1995 through 2016. It is useful to detail the type of structures included in the calculation. Starting with the universe of buildings sold, only residential structures are kept. Next, multifamily dwellings are dropped, as well as all single-family dwellings that are not arms-length transactions.

The first columns of Table 1 show the variable means for the full sample, also called the global sample. It breaks the means into levy passage status because one of the assumptions of the regression discontinuity model is that the only difference in characteristics between the samples is the outcome variable. Covariate means in global samples are usually very different from each other, but in this case they are highly similar to each other. Income shows the biggest difference, but even income is only about $6,000 different on a basis of $60,000. The danger is that differences in income between pass and fail groups could be causing a difference in outcomes, but the correlation between Income and Transaction Volume is only 0.06. Moreover, the effective sample—the sample within the bandwidth estimated to be optimal—shows an even more modest difference in Income of $3,000 between groups, and differences between other covariates remain small.

Speaking of small, the difference in Transaction Volume between the group of cities that passes and fails to renew police funding is small. On the surface one would expect no significant treatment effect, and the raw data in regression discontinuity studies often suggests one. But it is important to bear in mind that the raw outcome data is unadjusted data. Significant treatment effects can come from regression discontinuity estimates once a regression controls for the running variable. There are even cases where raw data suggests a negative treatment effect when in fact the estimated treatment effect is positive (e.g., Meyersson, 2014). A graph of the unadjusted outcomes is found in the Appendix.

In addition to covariate balance between treatment and control groups, another challenge to the validity of regression discontinuity results is there may be some agent that can precisely assign cities to specific values of the running variable. In the current context it may be a group that has access to the ballots cast like the League of Women Voters, or the county board of elections, or a foreign government that wishes to manipulate the level of funding for local police. In this case we would expect very distinct differences in the number of tax levies on either side of the cutoff. We use the density test of Cattaneo, Jansson and Ma (2020) to assess this possibility. The calculated *p*-value of the test is 0.55, failing to reject the null hypothesis of no discontinuity in vote share density around the cutoff. A histogram of vote share is presented in the Appendix.

Another test of the data in regression discontinuity studies is to see whether covariate values exhibit a discontinuity at the cutoff. The danger is that the value of a covariate jumps discontinuously at the cutoff, and the treatment effect is due to this jump in covariate values rather than a jump in outcomes. One way to assess this is to perform a seemingly unrelated regression (Lee and Lemieux, 2010). A system of equations is run with each covariate as dependent variable, with the running variable and a treatment dummy as explanatory variables. A test for whether the ‘treatment effect’ is jointly zero in the system has an estimated chi-squared test statistic of 6.5 with a *p*-value of 0.78, suggesting no covariate discontinuity. Graphs of covariate smoothness are found in the Appendix.

6. Results

Before jumping to the results, it is important to see whether unobserved variables or random jumps in the data may be responsible for statistically significant treatment effects. To this end, the first two columns of results in Table 2 show treatment effect estimates of a vote in period *t* on outcomes in prior periods. It would cast into question any subsequent results if a vote in 2001 caused a change in the number of house sales in 2000 and 1999 and, reassuringly, the results show that no such direction of causality is found. In a similar way, in unreported regressions available from the authors, we replace the 0.50 cutoff with a false cutoff equal to the mean vote share of 0.63, as well as false cutoffs of 0.45, 0.55, and 0.60 and fail to find a statistically significant treatment effect with any year after the vote.

[insert Table 2 about here]

The next column of results in Table 2, *t*+1, shows treatment effect estimates of renewing police department funding on the number of sales per capita one year after the vote. The results are statistically significant using all five different bandwidth selection options. The mean treatment effect is – 0.024, which means that cities, villages and townships that vote to renew their police taxes have 2.4 fewer house sales per 100 residents the next year. Equivalently, it means that voting to cut police funding causes 2.4 more house sales per 100 residents one year after the vote. Table 1 shows that – 0.024 is more than one standard deviation in magnitude, and about double the standard deviation of the full sample. We also tried clustering standard errors at the city level, because one quarter of the cities have more than one renewal levy in the sample. The results are qualitatively identical and basically quantitatively identical as well.

The remaining columns of Table 2 also show statistically significant effects. The mean magnitude of the results one through five years after the vote is -0.024, -0.023, -0.017, -0.010, and -0.010, suggesting that the effects persist over time, but at less than half the original magnitude in later periods. Even so, the -0.10 magnitude achieved four and five years after the vote is still almost one standard deviation of the mean number of sales for the full sample. Figure 1 graphs the treatment effect by year after the vote. It reverses the sign to show the treatment effect of failing to renew the tax levies.

[insert Figure 1 about here]

Results from regression discontinuity typically only apply to observations with characteristics like those near the cutoff, which can differ markedly from the global sample. But in the current case the means are similar between the local and global samples. The local average treatment effect may therefore represent a treatment effect applicable to the average local government in the entire state of Ohio.

Readers might question the validity of the results, wondering if confounding factors are responsible for the treatment effects rather than simply votes to cut police spending. For example, perhaps other types of tax levies that are voted upon in the same year are the true drivers. We have a number of responses to this potential criticism. The first is, if other tax levies are driving significant treatment effect estimates in periods *t*+1 through *t*+5, why aren’t they doing the same in periods *t­*-1 and *t*-2? Surely such an imbalance would also be evident in periods *t­*-1 and *t*-2, and yet there is no significant treatment effect for these periods. Second, why would the treatment effect diminish with time? This is a reasonable pattern of results for a regression discontinuity study, but it would be unlikely if it were a result of confounding factors that should stay roughly constant over time. Third, confounding votes may be viewed as an unobserved characteristic. The theory of regression discontinuity says that if observed covariates are balanced as if from randomization around the cutoff, unobserved covariates should be, too (Dunning, 2012; Murnane and Willett, 2010). Despite these arguments, we test this possible flaw empirically. We collect data on parks and recreation tax levies that occur in the same years as police protection tax levies within the effective bandwidth to see if there a balanced number of park tax votes across the pass and fail police levy samples, but in addition to balance we found that parks and recreation tax levies rarely even occur at the same time as police tax levies.

7. Conclusion

We collect voting data on cities, villages and townships in Ohio that vote on whether to renew police funding. Using regression discontinuity, we find 2.4 more houses sold per 100 residents one year after communities vote to cut police funding. This is a sizeable effect, about two standard deviations in magnitude for the initial periods. The effect persists through all years that we test, but dropping in magnitude to 1 house per 100 residents four and five years after the vote.

To what can we attribute this treatment effect? We see three inter-related possibilities. One is through the fear of crime. The second is through a signaling mechanism. The third is through an actual increase in the crime rate.

The literature on the fear of crime surprised us with its breadth; a google scholar search of the term revealed 2.4 million hits. Useful reviews of the literature include Garofolo (1981), Hale (1996), and Ditton and Farrall (2017). Pope (2008) finds house prices drop 2.3% when a registry reports that a sex offender moves into a neighborhood. Importantly, it is able to assess a return to normal house prices when a sex offender leaves, suggesting that the original 2.3% drop was due to fear of increased sex crimes and not confounding factors. It could be that some residents in communities that vote to cut police funding fear an increase in crime, whether or not crime levels actually rise. The most fearful residents may sell their houses and move to another community.

It could also be that communities that renew police funding signal that they place a high value on public safety. Residents may continue to reside in their current houses secure in this knowledge. Communities that vote to cut police funding signal a lower commitment to public safety, which may induce some residents to leave, as public safety is a powerful determinant of why individuals choose their houses (Tiebout, 1956).

The final explanation we consider is that voting to cut police funding may lead to a drop in the effectiveness of the police force, causing increased crime rates the year after funding is cut, leading to an exodus of a subset of the population and an increase in the number of houses sold. This would be the mechanism of Boggess, Greenbaum and Tita (2013), which finds that increased levels of crime in one year are related to higher rates of housing transactions in the next year. Studies on the link between police spending and crime rates are plagued by measurement error and endogeneity. Boggess, Greenbaum and Tita (2013) itself uses ordinary least squares, making it subject to omitted variable bias. Some studies find no link, a tenuous link, or even a positive link (e.g., Zhao, Scheider and Thurman, 2002; Pogue, 1975; Worrall and Kovandzic, 2007). On the other hand, there are studies that find police spending and increased numbers of police cause less crime (McPheters and Stronge, 1974; Vollaard and Hamed, 2012; Levitt, 2002).

We perform an exploratory investigation to start to quantify the relative importance of these three possible effects. First we test the hypothesis that cutting police spending causes increased crime, so that sales volume rises through this channel. The existing literature shows a tenuous link between police spending and crime rates. We merge our voting data with the Uniform Crime Reports data to see if our regression discontinuity design can detect an increase in crime when police funding is cut, but it does not. It would be tempting to conclude there is no link between crime rates and sales volume in our data, but the crime data may simply lack power to say anything conclusive. The Uniform Crime Reports only tracks crime for cities with more than 15,000 people, and it has missing values for a lot of years even for the larger cities. The next test will shed additional light on the increased crime mechanism.

We next note that the average police tax levy is 2.2 mills. Failing a large tax levy might affect crime rates, but this is less likely for small tax levies. If failing even the smaller police tax levies causes a change in sales volume, this suggests that the effect is probably due to the fear of crime and/or signaling channels. When we drop tax levies greater than or equal to 2.2 mills, the remaining sample of small tax levies still shows a statistically significant treatment effect most of the time, and in magnitudes similar to those found for the full sample. The results suggest that something more is at work than just the increase in crime mechanism studied by Boggess, Greenbaum and Tita (2013).

Further support for the fear of crime mechanism comes from changing the type of tax levies studied from renewal to new. Passing a new police tax levy for additional funding could help reduce actual crime rates, and it could help signal that a community is serious about public safety. We see no reason why the effect of cutting spending and increasing spending would be asymmetric when it comes to crime rates or signaling. But we see the possibility of asymmetric effects on the fear of crime. Increasing police spending might very well decrease fear of crime, but the theory of loss aversion (Tversky and Kahneman, 1991) suggests that the loss of existing police spending might increase fear of crime more than increasing police spending might decrease it.

We match police tax levies for additional funding to covariate and sales volume data and replicate the regressions that produced Table 2. The results are found in Table 3.

[insert Table 3 about here]

No statistical significance is found for any lead or lag. A lack of power is not responsible, as there are somewhat more new tax levies than renewal tax levies. We argue in Section 5 that new tax levies may produce biased estimates. It is possible that the endogeneity of the timing of new tax levies biases treatment effects toward zero, but the evidence could suggest that fear of crime is the driving force behind a change in sales volume.

What we can say with confidence is that we believe we find a well-identified causal link between changes in police funding and sales volume. It is a strong initial effect that persists at lower levels in even years later. We find the first evidence that public service spending affects sales volume, hopefully opening a new line of investigation of the effects of different types of public service spending on sales volume, like fire protection, roads, schools, and parks and recreation.

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Figure 1

Increase in Number of Houses Sold per Capita by Year after Voting to Cut Police Funding

Table 1

Means by Tax Levy Passage

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | Global Sample | | Local Sample | |
|  | Fail Levy | Pass Levy | Fail Levy | Pass Levy |
| Outcome Variable |  |  |  |  |
| Transaction Volume (house sales per person) | 0.016  (0.013) | 0.014  (0.012) | 0.015  (0.010) | 0.015  (0.018) |
|  |  |  |  |  |
| Covariates |  |  |  |  |
| % Minority | 0.05  (0.07) | 0.06  (0.13) | 0.04  (0.07) | 0.05  (0.08) |
| % Under 5 | 0.07  (0.02) | 0.06  (0.02) | 0.06  (0.02) | 0.06  (0.02) |
| % 5 to 17 | 0.19  (0.03) | 0.20  (0.03) | 0.19  (0.03) | 0.20  (0.03) |
| Population | 10,809  (19,506) | 10,065  (12,967) | 9,817  (17,113) | 9,841  (14,067) |
| Income | 56,882  (12,990) | 62,719  (19,895) | 55,840  (13,036) | 59,179  (16,344) |
| % Owner | 0.74  (0.10) | 0.75  (0.13) | 0.74  (0.10) | 0.74  (0.10) |
| % Single Parent | 0.11  (0.07) | 0.11  (0.06) | 0.12  (0.07) | 0.11  (0.06) |
| % Divorced | 0.12  (0.04) | 0.11  (0.04) | 0.12  (0.04) | 0.11  (0.03) |
| % Separated | 0.02  (0.01) | 0.01  (0.01) | 0.02  (0.01) | 0.02  (0.01) |
| Labor Force Participation | 0.63  (0.09) | 0.63  (0.07) | 0.63  (0.08) | 0.62  (0.08) |
| Number of Observations | 57 | 507 | 52 | 104 |
| Notes: Global sample refers to all police renewal tax levies from 1991 to 2018, local sample refers to those with vote share between 0.566 and 0.434, the effective sample with bandwidth *h* = 0.066 for regression of outcome variable three years after the vote. All nominal values deflated to constant 2010 U.S. dollars. | | | | |

Table 2

Effect of Renewing Police Tax Levy on Number of Houses Sold per Capita

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
| Bandwidth | Time Period Relative to Year of Vote | | | | | | |
| Selection | *t*-1 | *t*-2 | *t*+1 | *t*+2 | *t*+3 | *t*+4 | *t*+5 |
| RD | -0.006  (0.21) | -0.11  (0.14) | -0.027  (0.01) | -0.019  (0.02) | -0.016  (0.01) | -0.011  (0.01) | -0.010  (0.01) |
| TWO | -0.0008  (0.83) | -0.005  (0.48) | -0.024  (0.01) | -0.017  (0.03) | -0.015  (0.01) | -0.008  (0.03) | -0.009  (0.01) |
| SUM | -0.006  (0.20) | -0.009  (0.17) | -0.020  (0.02) | -0.020  (0.02) | -0.019  (0.01) | -0.011  (0.01) | -0.010  (0.01) |
| COMB1 | -0.006  (0.20) | -0.011  (0.14) | -0.027  (0.01) | -0.020  (0.02) | -0.019  (0.01) | -0.011  (0.01) | -0.010  (0.01) |
| COMB2 | -0.005  (0.26) | -0.009  (0.19) | -0.024  (0.01) | -0.018  (0.03) | -0.016  (0.01) | -0.011  (0.01) | -0.009  (0.01) |
| # obs | 610 | 620 | 564 | 526 | 505 | 489 | 452 |
| Notes: Treatment effect estimate shown with *p*-value in parentheses. Units are number of house sales in a community divided by population, so -0.027 is 2.7 fewer sales per 100 persons, for example. Bandwidth selection options from Calonico, Cattaneo, Farrell and Titiunik (2017) rdrobust function in Stata: RD imposes a common bandwidth *h* on either side of the cutoff; TWO allows different bandwidths on either side of the cutoff; SUM selects the bandwidth for the sum the of RD and TWO estimates; COMB1 selects the minimum bandwidth of RD and SUM; and COMB2 selects the median bandwidth estimate of RD, TWO, and SUM for each side of the cutoff separately. All covariates from Table 1 are included in the regressions. Estimates are mean squared error-optimal, local linear, with default minimum of three nearest neighbors used to construct the variance-covariance matrix. | | | | | | | |

Table 3

Effect of Passing New Police Tax Levies on Number of Houses Sold per Capita

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
| Bandwidth | Time Period Relative to Year of Vote | | | | | | |
| Selection | *t*-1 | *t*-2 | *t*+1 | *t*+2 | *t*+3 | *t*+4 | *t*+5 |
| RD | 0.002  (0.27) | 0.002  (0.36) | 0.002  (0.38) | 0.001  (0.58) | 0.001  (0.52) | 0.003  (0.18) | 0.002  (0.42) |
| TWO | 0.002  (0.24) | 0.002  (0.36) | 0.002  (0.38) | 0.001  (0.63) | 0.001  (0.50) | 0.001  (0.47) | -0.00  (0.80) |
| SUM | 0.002  (0.26) | 0.002  (0.37) | 0.002  (0.41) | 0.001  (0.57) | 0.001  (0.50) | 0.003  (0.22) | 0.001  (0.57) |
| COMB1 | 0.002  (0.26) | 0.002  (0.36) | 0.002  (0.38) | 0.001  (0.58) | 0.001  (0.50) | 0.003  (0.22) | 0.001  (0.57) |
| COMB2 | 0.002  (0.26) | 0.002  0.37) | 0.002  (0.38) | 0.001  (0.60) | 0.001  (0.49) | 0.002  (0.25) | 0.001  (0.77) |
| # obs | 754 | 770 | 772 | 776 | 797 | 833 | 805 |
| Notes: Treatment effect estimate shown with *p*-value in parentheses. Units are number of house sales in a community divided by population, so 0.002 is 0.2 more sales per 100 persons, for example. Bandwidth selection options from Calonico, Cattaneo, Farrell and Titiunik (2017) rdrobust function in Stata: RD imposes a common bandwidth *h* on either side of the cutoff; TWO allows different bandwidths on either side of the cutoff; SUM selects the bandwidth for the sum the of RD and TWO estimates; COMB1 selects the minimum bandwidth of RD and SUM; and COMB2 selects the median bandwidth estimate of RD, TWO, and SUM for each side of the cutoff separately. All covariates from Table 1 are included in the regressions. Estimates are mean squared error-optimal, local linear, with default minimum of three nearest neighbors used to construct the variance-covariance matrix. | | | | | | | |