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## Do Foreclosures Increase Crime?

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# Do Foreclosures Increase Crime?

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**Abstract**

Among the policy concerns associated with increased foreclosures is an increase in neighborhood crime. We propose that foreclosures increase crime by decreasing informal policing by residents, an aspect of crime deterrence little explored in the empirical economics literature. We investigate the effect of foreclosures on crime using a national county-level panel dataset covering the period 2002 to 2007. Employing an instrumental variables strategy to correct for measurement error in foreclosure rates, we find robust evidence that foreclosures increase burglary. A one percentage point increase in foreclosure rates is estimated to increase burglary rates by 10.1 percent. Sensitive to sample period, we also find positive effects on larceny and on aggravated assault. Our estimates indicate that the recent spike in foreclosure activity will result in associated community-wide burglary costs of at least $4.6 billion, and of at least $17.4 billion when considering the impact on all types of crime.

Key Words: Crime, Foreclosures, Instrumental Variables
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## 1. Introduction

Since 2005, the foreclosure rate has increased dramatically in the United States. Figure 1 shows that over the 1980 to 2005 period, the average annual increase in the ratio of U.S. foreclosure starts to outstanding mortgage loans was 7.2 percent. In comparison, between 2005 and 2008 the foreclosure start rate grew by 160 percent.[1] This sharp increase in foreclosure activity is expected to continue given the present housing and economic crisis. Current forecasts indicate that the number of foreclosures will rise to 8 to 10 million by 2012, affecting more than 15 percent of all mortgages (Dubitsky, Yang, Stevanovic, and Suehr, 2008; Colpitts, 2009). In addition to the direct costs realized by borrowers and lenders, an increase in foreclosures may lead to decreases in area property values (Calorimis, Longhofer, and Miles, 2008; Harding, Rosenblatt, and Yao, 2009), decreases in the stigma of defaulting on a mortgage (Guiso, Sapienza, and Zingales, 2009), and substantial administrative costs and lost property tax revenue for local governments (Apgar and Duda, 2005).

Another fear is that foreclosures lead to increased crime. A March 2009 Congressional Oversight Panel report states, "Communities with high foreclosure rates suffer increased urban blight and crime rates." Anecdotally, the media frequently report increases in burglary, vandalism, and organized criminal activity associated with concentrations of foreclosure-related unoccupied homes.[2] Law enforcement has expressed concern that foreclosed properties attract "gang activity, drug dealing, prostitution, arson, rape and murder." (Apgar and Duda, 2005).

The foreclosure crisis has prompted a number of public policy initiatives, including the Obama administration's "Making Home Affordable" loan modification program, Fannie Mae's 2009

[1] The foreclosure process varies across states, but generally consists of three stages: When a borrower becomes seriously delinquent on his mortgage, a lender may start the foreclosure process by registering a legal notice of default. If the matter is not resolved, the property then goes to public auction. If the property is not sold at auction, the lender takes possession of the property; at this point the property is deemed "Real Estate Owned."

[2] See for example, Millman (2009), Leinberger (2008), Christie (2007), or Elphinstone (2007a, 2007b). Millman (2009) report an increase in "drop houses," where Mexican gangs store drugs or illegally smuggled persons, in Arizona neighborhoods which have experienced substantial foreclosure activity.

"Deed for Lease" program that allows residents to remain in foreclosed properties and pay rent, and the Housing and Economic Recovery Act of 2008, which in part provides local governments community development block grants to purchase foreclosed properties. These initiatives hope to keep defaulting borrowers in their homes, or allow new residents to more quickly occupy previously foreclosed homes. Among the justifications for these programs is the assumption that keeping foreclosure-affected houses occupied will help to stem neighborhood blight and crime.

Following the seminal work of Becker (1968), an extensive empirical literature has developed examining the importance of factors theorized to affect crime. One body of literature focuses on the role of opportunity costs, such as the value of foregone legitimate market activities.[3] Another investigates the role of formal deterrence, namely, policing and incarceration.[4] However, little empirical work in the economics literature has addressed the role of informal deterrence on crime. In her influential work, Jacobs (1961) proposed that neighborhood "eyes on the street," particularly in dense urban areas, deter crime. We propose that an increase in foreclosures leads to a decrease in the surveillance by community residents, which we define as *passive policing*. Other things equal, a neighborhood which suffers increased rates of foreclosure will have more vacant housing units and lower homeownership rates, decreasing the capacity and incentive of remaining residents to engage in passive policing. For a potential criminal, this lowers the probability of detection and increases

[3] Chiricos (1987) and Freeman (1983) survey 63 and 25 studies, respectively, and generally find a positive but modest relationship between the unemployment rate and crime. More recently, Gould, Weinberg, and Mustard (2002) and Grogger (1998) focus on the market wage of young unskilled men to show that increasing wages play a larger role in decreasing crime. Using state variation in compulsory schooling laws, Locher and Moretti (2004) show that education reduces the probability of incarceration and arrest, while Jacob and Lefgren (2003) and Witte and Tauchen (1994) provide some evidence that physical time in school decreases criminal activity for youth.

[4] Surveying 22 studies, Cameron (1988) finds that 18 show no relationship or a positive relationship between police and crime. Over the past decade or so, a number studies have used novel identification strategies to overcome the inherent endogeneity of policing levels with respect to crime, generally providing causal evidence that increased formal policing decreases crime (Klick and Tabarrok, 2005; Di Tella and Schargrodsky, 2004; McCarry, 2002; and Levitt, 1997, 2002). Similarly, Levitt (1996) uses an instrumental variables strategy to address the endogeneity between prison populations and crime, to show that incarceration decreases crime levels.

the net payoff to crime. Therefore, we hypothesize that foreclosures increase crime. In particular, we expect foreclosures to have a strong effect on burglary rates.[5]

The goal of this paper is to empirically investigate whether foreclosures have an effect on crime rates. That foreclosures increase crime is typically presumed by public officials, the media, and even researchers.[6] However, to our knowledge only two peer-reviewed studies provide evidence in support of this presumption, both of which are limited by their cross-sectional nature and narrow geographic scope (Immergluck and Smith, 2006; Spelman, 1993).[7]

This study is the first, to our knowledge, to estimate the effect of foreclosures on crime using a nationally representative county-level panel dataset. We construct data from a variety of sources for the period 2002 to 2007. Using the Federal Bureau of Investigation's (FBI's) Uniform Crime Report (UCR) data, we focus on the FBI Index crimes of larceny, burglary, motor vehicle theft, robbery, aggravated assault, rape and murder. County-level "real estate owned" (REO) based foreclosure rates are computed using proprietary loan-level data from Lender Processing Services

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5 An occupied residence is likely a stronger deterrent to burglary than a structure vacated due to foreclosure. The criminal justice literature notes that burglary incidents are more likely to occur during the hours of the day and the days of the week when a home is unoccupied (Scarr, Pinksy, and Wyatt, 1973), and that burglary victimization rates are substantially higher for homes where occupants self-report being out of the home more than 35 hours per week (Repetto, 1974). In related work, sociologists Cohen, Felson, and Land (1980) and Cohen and Felson (1979) argue that changes in the post-World War II "routine activity" structure of American society, specifically, increases in female labor force participation and single-adult households, have increased the number of hours when homes are unoccupied, to which they attribute associated increases in rates of robbery, burglary, and motor vehicle theft. Other sociologists include the vacancy rate when investigating the effect of "social disadvantage" on crime (for example, McNulty and Holloway, 2000; Krivo and Peterson, 1996; Roncek, Bell and Francik, 1981; and Roncek, 1981). These cross-sectional studies, which likely suffer from omitted variable bias, generally find a positive and statistically significant association between vacancy and crime.

6 For example, Campbell, Giglio, and Pathak (2009) find larger foreclosure discounts for low-value houses and homes in poorer neighborhoods, which they conjecture is the result of higher relative fixed costs for banks to protect these homes from vandalism.

7 Immergluck and Smith (2006) examine the effect of tract-level foreclosure rates on contemporaneous crime rates for Chicago in 2001. While they find a one standard deviation increase in foreclosure rates increases violent crime by 6.7 percent, contrary to our intuition, they find a small and statistically insignificant effect on property crime. Spelman (1993) matched thirty-five blocks with abandoned buildings, some of which were foreclosed properties, to twenty-four control blocks with similar characteristics in Austin, TX, and found that blocks with unsecured abandoned buildings had drug and property crime rates twice as high as that of control blocks. He attributes the findings to abandoned buildings creating opportunities for unsupervised "criminal hangouts."

(formerly "McDash") and LoanPerformance.[8] Drawing from the economic literature on crime, we include a comprehensive set of controls for demographic, macroeconomic, and enforcement characteristics related to the costs and benefits of committing crime. The panel nature of our data allows us to include county-level fixed effects to control for persistent differences in unobserved characteristics that might give rise to spurious correlation between foreclosure and crime rates, an important innovation in our analysis.

Despite an extensive set of control variables, our ordinary least squares (OLS) estimate of the foreclosure effect on crime may be downwardly biased due to error in our measurement of foreclosure rates. Therefore, we also pursue an instrumental variables (IV) estimation strategy, using house price growth in the local metropolitan area and house price growth interacted with county unemployment rates, as instruments for foreclosure. Our choice of instruments is motivated by recent theoretical and empirical work on mortgage default which posits that negative home equity accompanied by a "trigger event," such as a spell of unemployment, provides a sufficient condition for default (Gerardi, Shapiro, and Willen, 2008, 2009; Foote, Gerardi, Goette, and Willen, 2009). Consistent with this premise, our first stage results indicate that our instruments are highly predictive of foreclosure rates. Because we directly control for the unemployment rate, per capita income, the wage rate of low-skilled workers, and in some specifications past crime rates, the remaining variation in our instruments is reasonably not related to crime rates except through its impact on foreclosures.

We find that foreclosures have a positive effect on burglary. This effect is statistically significant across a variety of estimation strategies and robust to a myriad of specification checks. Our preferred IV estimate indicates that a one percentage point increase in the one-year lagged

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[8] We include only those loans in REO status, meaning that the property is in possession of the lender as a result of foreclosure, in the numerator of our foreclosure rate computation. We feel this is the most accurate measure of foreclosures that result in the displacement of residents. Although the house may become vacant at any point during the foreclosure process, by the time a property reaches REO status it is likely to be vacant.

county foreclosure rate increases the burglary rate by 10.1 percent. Sensitive to sample period, we also find a statistically significant effect of foreclosures on larceny and on aggravated assault. Focusing on burglary, we explore potential mechanisms for the foreclosure effect, finding that it occurs primarily in densely-populated areas and that foreclosure on owner-occupants has a larger effect relative to non-owner-occupying households. These results support the premise that passive policing deters crime, and that the foreclosure effect depends on affected residents' incentives to engage in residential surveillance. We estimate that the spike in foreclosure starts between 2005 and 2008 will generate aggregate community-wide burglary-related costs of $4.6 billion, and of at least $17.4 billion when considering the impact on all crimes. We note that because our analysis period precedes the current foreclosure crisis, we interpret these as lower-bound estimates. If the effect of foreclosures on crime is non-linear, for example if the deterrence effect of neighborhood residents decreases sharply when foreclosures exceed a critical mass, we may underestimate the effect of foreclosures on crime in periods when foreclosures are high.

The remainder of this paper is organized as follows: Section 2 discusses our conceptual framework while Section 3 outlines the empirical specification. Section 4 describes the data and the limitations of our foreclosure data that necessitate the use of an IV strategy. Section 5 presents our OLS and IV results, discusses the validity of our instruments, and provides a sensitivity analysis. Section 6 explores the mechanisms through which foreclosures might affect burglary. Section 7 provides estimates of the economic costs of crime expected to result from the recent increase in foreclosure activity. Section 8 concludes.

## 2. Conceptual Framework

Becker (1968) proposes that potential criminal offenders are rational agents who respond to incentives in the form of criminal justice penalties. Expanding on this idea, Ehrlich (1973) includes rewards to criminal behavior, where a potential offender chooses the optimal mix of legal and illegal market activities. Ehrlich (1996) discusses the individual's "supply" of a crime and models in its most basic form the net payoff of committing crime as follows:

$$\pi_i = w_i - c_i - w_l - p_i f_i \quad [1]$$

Here the net payoff $\pi_i$ of committing crime *i* is a function of the monetary value of the "loot," $w_i$, less the direct cost $c_i$ of acquiring the goods, the opportunity cost $w_l$, or the foregone return from legitimate labor market activities, and the product of the deterrence parameters, the probability of conviction, $p_i$, and the penalty if convicted, $f_i$.

We propose that an increase in foreclosures decreases the level and quality of passive policing that may deter crime. When a home is foreclosed upon, its residents are typically evicted from the property, leaving behind a vacant structure which remains unoccupied for months or even years.[9] If the former occupants of the foreclosed property choose to relocate out of the local area, the vacancy rate increases and the number of "eyes on the street" that contributes to detection of crime falls. For former owner-occupants who remain in the area and rent a nearby residence, their incentive to engage in passive policing likely falls, since they are no longer homeowners.[10] Given their equity interests and longer average tenures , homeowners should be more likely than renters to engage in the neighborhood surveillance that deters crime (DiPasquale and Glaeser, 1999). In contrast to the "target hardening" security behavior expected of homeowners (Cook, 1986; DiIulio,

[9] Coulton, Mikelbank, and Schramm (2008) report that 50 percent of properties entering REO status in Cleveland and Cuyahoga County over the period 2000 to 2002 were sold to a private owner within four months. Coulton, Schramm, and Hirsch (2008) estimate that for properties entering REO in 2007, the time elapsed will be at least three times as long.
[10] As of August 2008, Fannie Mae guidelines prohibit a homeowner that is foreclosed upon from purchasing a home for five to seven years. Alternatively, residents evicted from a foreclosed property may choose to move in with friends or family, which would increase the area vacancy rate. A recent analysis of Census data attributes sharp increases in multi-generational households since 2007 to increases in unemployment and foreclosure rates (Taylor et al, 2010).

1996), foreclosure-related increases in vacancy and decreases in homeownership should result in what might be called "target softening" within an area.[11]

We hypothesize that foreclosures will have a particularly strong effect on burglary. An occupied residence is likely a stronger deterrent to burglary than a structure vacated due to foreclosure. In addition, burglary of a foreclosed or neighboring home may yield more valuable "loot" than available prior to the foreclosure.[12] We also expect foreclosure-related reductions in passive policing to lead to increases in the property-related crimes of larceny, motor vehicle theft, and robbery, although these effects are potentially mitigated by a concurrent decrease in the number and proximity of potential targets (Glaeser and Sacerdote, 1996; Kelly, 2000). To the extent that foreclosures allow for unmonitored drug, gang, and other organized activity that results in increased aggravated assault (Millman, 2009; Spelman, 1993), a positive effect for this violent crime is possible. Lastly, we do not expect to find an effect of foreclosures on rates of rape and murder. The role of passive policing in deterring such crimes is likely small, since these crimes typically occur between non-strangers (Bureau of Justice Statistics, 2010; U.S. Census, 2006). The pre-existing relationship between offender and victim suggests that non-pecuniary benefits, which may far outweigh the costs of apprehension, motivate these crimes.[13]

The raw data is consistent with our premise that foreclosures increase crime. Figure 2 illustrates trends in foreclosure and crime rates over our sample period, separately for the ten states

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[11] We verify that foreclosures are in fact associated with increased vacancy rates and decreased rates of homeownership. Using U.S. Postal Service data on quarterly county-level vacancy rates from 2008 to 2009 provided by U.S. Department of Housing and Urban Development, regression results indicate that there is a positive and statistically significant correlation between vacancy rates and lagged foreclosure rates. The relationship is particularly strong when focusing on long-term vacancies (residences vacant 12 months or longer). Using American Community Survey data on annual county-level homeownership rates between 2004 and 2008, our results indicate that homeownership rates are negatively correlated with lagged foreclosure rates at the county level. Details regarding the source data, empirical specifications, and estimation results are available from the authors upon request.

[12] Dornin (2008) reports that foreclosed homes are often burglarized for copper wiring, plumbing, major appliances, and other goods which perpetrators are unlikely to be able to obtain quickly from occupied homes.

[13] Gould, Weinberg, and Mustard (2002) describe this as the "interdependence of utility" between offender and victim.

with the highest rates of foreclosure in 2006 (solid lines) and the remaining forty states and District of Columbia (dashed lines).[14] Figure 2(a) shows that foreclosure rates in the top 10 foreclosure states nearly doubled between 2001 and 2002, remained high through 2004, and then increased again thereafter. In the other states the increase was not nearly as dramatic; after an initial jump from 2001 to 2002, foreclosure rates subsequently declined before moderately increasing again beginning in 2004. Figures 2(b) through 2(h) show trends for each crime type, where the crime rates are indexed to their respective initial levels. The figures indicate that the top foreclosure states experienced higher rates of crime growth relative to low foreclosure states. However, while the differences in crime rate trends may be caused by foreclosures, alternatively these trends may simply reflect changes in other factors, for example economic conditions, correlated with both foreclosures and crime rates. In our empirical specification we attempt to control for such factors.

## 3. Empirical Specification

We model the crime rate in county *i* in year *t* as follows:

$$crime_{it} = \beta_0 + \beta_1 f^*_{it-1} + X_{it}'\beta_2 + Y_{it}'\beta_3 + P_{it}'\beta_4 + t'\lambda + i'\theta + \varepsilon_{it} \quad [2]$$

The outcome $crime_{it}$ is the log of the crime rate.[15] We estimate the model separately for each crime type so that the coefficient estimates are allowed to vary. Cells are weighted by mean county population over the years in our sample.

The variable of interest is $f^*_{it-1}$, the true county-level foreclosure rate in the previous year, which we observe with error.[16] We lag foreclosure rates to ensure that the parameter $\beta_1$ represents

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[14] In 2006, the top ten states were: Michigan, Ohio, Colorado, Indiana, Georgia, Missouri, Tennessee, Texas, Kansas, and South Carolina. Note that some of the highest foreclosure states in the current crisis (for example, Florida, California, Nevada) are not on this list. Foreclosure rates in these states were still relatively low in 2006 but increased sharply thereafter.

[15] Logging the crime rates addresses concern about reporting error due to differences in reporting amongst jurisdictions; Ehrlich (1996) suggests taking logarithms since reported crime rates are likely to be proportional to true crime rates.

the effect of foreclosure on crime that occur only after the foreclosure actually takes place. Lagging the foreclosure rate addresses the possibility that causality runs in the opposite direction. Feinberg and Nickerson (2002) hypothesize that an increase in crime rates leads to an increase in default rates. While they do not find that property crime affects future default rates, they do find evidence that violent crime influences default rates three years into the future. Under the assumption that trends in crime rates are not persistent, use of the lagged foreclosure rate alleviates this issue, because the previous year's foreclosure rate is not affected by the current year crime rate. Because this assumption may be tenuous, we also estimate a specification that controls for aggregate property and violent crime rates lagged one, two, and three years relative to foreclosure rates.

Under the null, $\beta_1$ is equal to zero. Estimates of $\beta_1$ that are positive and statistically different from zero are consistent with the alternative hypothesis that increases in foreclosure rates increase crime rates in the following year.[17]

We include a comprehensive set of explanatory variables in the model to control for factors that plausibly affect the relative costs and benefits of committing a crime. The vector $X_{it}$ controls for local demographic characteristics, $Y_{it}$ controls for local economic conditions and other measures of the opportunity costs of committing crime, and $P_{it}$ controls for formal deterrence factors. In Section 4 we describe in detail the specific controls we incorporate, discussing the economic justifications for their inclusion, and note our data sources.

[16] We use the level of foreclosure rates rather than logged foreclosure rates to avoid dropping county-year observations with zero foreclosures. Estimates from a specification in which we use the log of foreclosure rates are comparable to those from the main specification.

[17] We note that while we observe the lagged completed foreclosure rate, we do not actually observe when and how long foreclosed houses may be vacant. In practice we aggregate foreclosure rates to the annual level, which may obscure any effects if foreclosures result in vacancies that last for a shorter interval, but still affect crime. It is also likely that any effect of foreclosure rates on crime rates is a very local phenomenon. Aggregating to the county-level captures only average effects across counties, and may conceal any heterogeneous effects that occur within counties at the neighborhood level.

Lastly, we include in the model year fixed effects to control for national macroeconomic trends, and county-level fixed effects to control for persistent differences in unobserved characteristics across counties. Our estimates of the effect of foreclosures on crime are therefore identified by within-county deviations from mean, after netting out national time trends, rather than through level differences in foreclosure rates across areas. Identification which relies on such cross-sectional variation is unattractive if foreclosure rates are correlated with unobserved characteristics that also influence crime.

The error term $\varepsilon_{it}$ contains all remaining unobserved idiosyncratic factors that affect county-level crime. Estimation of this model using OLS is valid if $\varepsilon_{it}$ is uncorrelated with the explanatory variables. However, as described in Appendix A our data appear to systematically overstate foreclosure rates. In practice, we observe a foreclosure rate $f_{it-1}$ that is likely a function of the true foreclosure rate, $f^*_{it-1}$. Under reasonable assumptions generated from the data, in Appendix B we show that the OLS estimate of $\beta_1$ is inconsistent and biased downward. Therefore, we pursue an IV estimation strategy to address this issue.

## 4. Measuring the Determinants of Crime

The analysis in this paper incorporates data from a wide variety of sources. Our crime data are from the Uniform Crime Reports, generated yearly by the FBI from voluntary reports collected from county, city, and state law enforcement agencies. Although there are limitations to the UCR data, no other nationally representative, geographically disaggregated source of crime data is available.[18] We include years 2002 through 2007, the most recent year covered in the UCR data. All crime rates are per 100,000 persons, based on population totals reported in the UCR.

We compute county-level foreclosure rates by aggregating loan-level data from LPS Applied Analytics (LPS) and LoanPerformance (LP). Because we use lagged foreclosures in our empirical analysis, our foreclosure data cover the period 2001 to 2006. As of 2008, LPS collected loan-level data from nine of the ten largest mortgage servicers in the U.S., covering roughly 58 percent of the total prime/near prime market and 32 percent of the subprime market (Immergluck, 2008).[19] Because LPS' coverage of subprime loans is relatively weak, and subprime loans are more likely to end in foreclosure (Gerardi, Shapiro, and Willen, 2008), we supplement our LPS data with LP data to ensure that subprime loans are well-represented. The LP data cover approximately 80 percent of the privately-securitized subprime and Alt-A mortgage loans in the U.S., and like LPS, contain loan-level payment records. To limit double-counting, we exclude privately-securitized subprime loans from LPS and include only these loans from LP.

We limit the sample in both LPS and LP to owner-occupied, first-lien purchase or refinance mortgages on 1-4 family homes, townhouses, or condominiums. We define the foreclosure rate in county $i$ and year $t$ as the count of mortgage loans in real estate owned status, divided by the count of all active loans.[20] We focus on the share of loans in REO status, rather than in any stage of the

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[18] See Levitt (1997) or Gould, Weinberg, and Mustard (2002) for a discussion of the strengths and weaknesses of the UCR data.

[19] See Immergluck (2008, 2009) for a detailed discussion of LPS data.

[20] Because the only geographic identifiers provided in the LPS and LP data are the state and zip codes of the property address for each loan, we aggregate to the county-level using a zip code to county crosswalk file produced by the Missouri Census Data Center MABLE program. Roughly 30% of zip codes map to more than one county. In these cases we assign the county identifier for the county that contains the highest percentage of the zip code's residents. The MABLE program is available on the web at http://mcdc2.missouri.edu/websas/geocorr2k.html.

foreclosure process, because we believe this is the most accurate measure of foreclosure-related vacancies.[21]

Despite the richness of the mortgage data we use, our measure of foreclosure rates likely suffers from error for at least two reasons. First, because the true foreclosure rate is very small in magnitude over our analysis period, even our large sample of loans yields estimates of the foreclosure rate at the county and year-level that are relatively imprecise. Second, our sample is not randomly selected from the universe of mortgage loans, especially considering the relative weakness of LPS' coverage prior to 2005.[22] Thus, as discussed in greater detail in Appendix A, our measure of foreclosure rates suffers from at least some degree of selection bias, which necessitates the use of an IV strategy.

We draw on the extensive empirical economic literature on crime to motivate the inclusion of the remaining data elements in our sample.

It is well-known that those involved in criminal activity are overwhelmingly young and male (Levitt, 1999, 2004; DiIulio, 1996; Freeman, 1996). In 2008, the incarceration rate of U.S. males was over 15 times that of females, while the rate for males aged 18 to 24 was 26 times that of males 65 years or older (Sabol, West, and Cooper, 2009). Further, a disproportionate number of both perpetrators and victims are black.[23] Levitt (1999) finds that changes in age and race distribution, while not an important factor for violent crime rates, accounted for one-sixth of the decrease in property crime observed for the 1990s. Therefore, we control for the county-level share of the

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21 Based on our sample of LPS and LP loans, only 38 percent of the loans which entered foreclosure in 2006 reached REO status within the next 24 months, suggesting a substantial share of loans in the earlier stages of foreclosure do not result in a vacancy.

22 While LPS has expanded its coverage of the mortgage market in recent years by partnering with additional mortgage servicers, newly added servicers were reportedly not required to provide payment records in months prior to January 2005.

23 For example, in 2008 the incarceration rate per 100,000 for black males was 3,161 compare to 952 for white males (Sabol, West, and Cooper, 2009). In 2005, the homicide victimization rate for blacks was six times that of whites, while the homicide offending rate was seven times greater (http://bjs.ojp.usdoj.gov/content/homicide/ageracesex.cfm).

population which is male aged 15-24, and the share that is black. All of our demographic data are from county-level estimates provided by the U.S. Census Bureau.

The empirical literature investigating the role of opportunity costs on crime has primarily studied aspects of the labor market and participation in educational activities. Surveys of the literature generally find a positive but modest relationship between unemployment and crime (Chiricos, 1987; Freeman, 1983). Recent work has emphasized that because those who commit crime are predominately young and less-educated, opportunity cost measures are most relevant for those with low legitimate earnings (Freeman, 1996). Along these lines, Gould, Weinberg, and Mustard (2002) find that changes in the wage rate among low-skilled workers explain a substantial portion of variation in crime trends over the past few decades. Following their methodology, we use Current Population Survey data to control for the state wage rates of low-skilled workers; in Appendix C we detail the construction of this variable. We also control for county-level unemployment rates and per capita income using data from the Bureau of Economic Analysis.

Education and physical time spent in school should also decrease crime. In the long-term, education increases the future returns to legitimate work, or in other words, the future opportunity costs of engaging in illegal activity, and may change individuals' preferences towards criminal activity. Using state variation in compulsory schooling laws, Locher and Moretti (2004) find that education significantly reduces rates of incarceration and arrest. In the short-term, the quality of schooling may make it more worthwhile to substitute time from illegal to schooling-related activities. Jacob and Lefgren (2003) use variation in jurisdiction-level teacher in-service days to show increases in school days decreases property crime levels, while Witte and Tauchen (1994) use individual-level data to show time allocated to school decreases the probability of arrest. We control for education-related opportunity costs that may affect the crime supply decision by including state-level per capita expenditures on education, obtained from the U.S. Census Bureau State and Local Government

Finances data. Following Levitt (1997), we also control for state-level welfare expenditure, using data from the same source.

Becker's economic model formalizes the deterrent effect that police should have on crime. Given the endogenous nature of policing levels with respect to crime, where more police are assigned to high-crime areas, recent studies by Klick and Tabarrok (2005), Di Tella and Schargrodsky (2004), and Levitt (1997, 2002) devise novel identification strategies to generally find that increases in policing lead to lower rates of crime. Incarceration should also decrease crime, both by incapacitating existing criminals, and by deterring potential ones. Levitt (1996) uses prison overcrowding legislation to instrument for endogenous incarceration rates to demonstrate a negative relationship between incarceration and crime levels. We control for variation in formal deterrence using the state number of police officers per 1,000, obtained from Criminal Justice System, Expenditure, and Employment data, and the state prison population per 1,000, obtained from National Prison Statistics provided by the Bureau of Justice Statistics.[24] Because we do not address the endogeneity of these deterrence measures, we do not attach a causal interpretation to our related coefficient estimates. However, to the extent that these factors are correlated with our parameter of interest, lagged foreclosure rates, they must be included to mitigate any omitted variable concerns.[25]

We control for the share of home purchase and refinance loans within the county originated by lenders regulated by the U.S. Department of Housing and Urban Development, the primary regulator of non-bank, non-thrift mortgage companies. We include this measure as a proxy for subprime mortgage lending, since subprime borrowers are more likely than prime borrowers to default (Gerardi, Shapiro, and Willen, 2008). If institutions that specialize in subprime loans non-

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[24] State-level police data are currently available only through 2006. We assign the 2006 county-year value of police expenditures and number of sworn police officers to our year 2007 cells.

[25] For example, property tax revenue could decrease with increased foreclosures, consequently decreasing police expenditures, or more police could be assigned if increases in crime are expected to result from increased foreclosures.

randomly locate in areas with similar crime rates, then this measure controls for this potential spurious correlation. We compute this measure using annual Home Mortgage Disclosure Act (HMDA) data.[26]

Our last data element is the Metropolitan Statistical Area (MSA) five-year house price growth rate which we compute from the House Price Index ("Index") published by the Federal Housing Finance Authority.[27] We use this measure of house price growth as an instrument for foreclosure rates in our IV strategy. Because the Index is published at the MSA level, every county within an MSA is assigned the same house price growth in a given year.

All data are merged together at the county and year-level. Of 3,141 total counties, we drop 1,345 in rural (non-MSA) areas because residences in these counties may not be spatially dense enough to form neighborhoods. We drop an additional 727 counties in MSAs in which data on house price growth are not published. Of the 6,412 remaining county-year observations, we drop 394 county-year observations with less than 75 percent of police precincts reporting UCR data, 385 county-year observations with less than 100 total active loans from LPS and LP data, and 376 county-year observations due to missing data on crime rates, unemployment rates, or per capita income. Finally, we limit the sample to counties that appear in at least five out of the six years of the sample period. Our final analysis sample consists of 4,713 county-year observations, covering 799 counties over the years 2002 to 2007. Summary statistics are presented in Table 1.

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26 HMDA data are essentially the universe of home mortgage-related loans made in the United States. Banks and other lenders are obligated to report loan level data annually, in order for regulators to determine whether there are statistically different lending patterns by applicant race and property location. Since 1992, HMDA has also covered non-bank mortgage companies.

27 The Index measures the quarterly change in single-family house prices in nearly 300 MSAs. The Index is a weighted, repeat sales index based on conventional, conforming mortgages purchased for securitization by Fannie Mae or Freddie Mac. We annualize the data by averaging the quarterly values in each year. We also adjust the Index for inflation using the national Consumer Price Index series less shelter for all urban consumers.

## 5. Results

*A. OLS Analysis*

OLS estimates of the empirical model are presented in Table 2. Each cell shows the coefficient from a separate regression of the dependent crime outcome on the lagged foreclosure rate. Each estimation includes the full set of demographic, macroeconomic, and formal deterrence controls described in Section 4, as well as year fixed effects. Column 1 presents estimates when county-level fixed effects are excluded from the specification, and Column 2 presents estimates when county fixed effects are included. Panel A presents coefficients for property crimes and Panel B for violent crimes.[28]

The first column shows that, when county fixed effects are excluded, an increase in the lagged foreclosure rate is associated with a positive and statistically significant effect on total property crime.[29] Disaggregating by crime type, it is clear that this estimate is driven by the foreclosure effect on burglaries: a 1 percentage point increase in the lagged foreclosure rate increases the burglary rate by 10.4 percent. In contrast, the effect of foreclosures on larceny and motor vehicle theft is small and statistically insignificant. Once county fixed effects are included, the magnitude of the effect on aggregate property crime decreases substantially and is no longer statistically significant. While the estimated effect on burglary remains statistically significant, the magnitude decreases by a factor of nearly five. Turning to Panel B, OLS estimates from the model with county fixed effects indicate that foreclosures do not lead to an increase in violent crime. Only in the case of robbery is the estimate significantly different than zero, and surprisingly is negative.

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[28] To avoid dropping county-year observations with zero assaults, rapes, or murders, we add a one to all observations of each of these crime rates before taking logs. This transformation has little effect on our results; our estimates are quite similar if we use as the dependent variable the log of the observed crime rate, dropping all observations with crime rates equal to zero.
[29] All significance tests discussed in the text are at the five percent level unless otherwise noted.

Comparing the estimates in Columns (1) and (2), we find that including county fixed effects uniformly reduces the estimated magnitude of the foreclosure effect. For all crime categories except larceny, robbery, and murder, the difference between estimates is statistically different from zero.[30] The results substantiate our concern that in the model without county fixed effects, the estimated foreclosure effects are identified in part by level differences in foreclosure rates across areas. Therefore, our ability to control for time-invariant unobservable differences across geographic areas appears to be an important innovation over previous work. In the remainder of this paper we focus only on models that include county fixed effects.

Because estimates on the other covariates are not of central importance, we do not discuss them here. We refer the interested reader to Appendix D for a discussion of the full set of estimation results for aggregated crime rates.

### *B. IV Analysis*

We pursue an IV estimation strategy to address measurement error in foreclosure rates. We use two instruments for foreclosure rates: metropolitan area house price growth, and house price growth interacted with county unemployment rates. The validity of our IV approach rests on the assumption that the instruments are strongly correlated with local area foreclosure rates, and are not related to crime rates through any other channel.

It is well established in the economics literature that house price appreciation influences a mortgage holder's decision to default. Standard models depicting the option to default typically include interest rates and house values as the key state variables (for example, Foster and Van Order, 1984; Deng, Quigley, and Van Order, 2000). In these models, a necessary condition for borrower

[30] Standard errors on differences of coefficients across separate estimations are computed by bootstrapping, with 100 replications.

default is that the value of the home is less than the remaining mortgage balance, in other words, that the homeowner is "underwater."[31] However, in the absence of cash flow problems, an underwater borrower also has the option to continue making their mortgage payments, perhaps in anticipation of future increases in house prices. Therefore, being underwater is a necessary but not sufficient condition for default. Recent theoretical and empirical work explores the impact of "trigger events," temporary shocks to income such as an unemployment spell, on mortgage default (Gerardi, Shapiro, Willen, 2008 and 2009; Foote et al. 2009). The idea is that an underwater borrower who suffers a negative income shock is unable to continue paying the mortgage or to refinance, which provides a sufficient condition for default. The practical implication of these models is that house price growth, as well as its interaction with the unemployment rate, ought to be negatively correlated with mortgage defaults and ultimately with foreclosures.

The second key criterion on which our IV strategy rests is that the instruments are not correlated with crime through any channel other than foreclosures. While house price growth may be correlated with macroeconomic conditions that also affect crime rates, we explicitly control for the county-level unemployment rate and per capita income, as well as the wage rate for low-skilled workers in the state. To the extent that we have controlled for economic influences that are potentially both correlated with house price appreciation and affect crime, the remaining variation in house price growth rates can be attributed to location-specific demographic and geographic factors that are plausibly exogenous with respect to crime rates. Given that we directly control for the local unemployment rate in our specifications, we also reasonably assume that the *interaction* between unemployment and house prices is not correlated with the error term of the crime equation.

[31] Rather than defaulting, a homeowner with positive home equity can instead opt to sell the home and prepay the mortgage, thereby receiving a positive cash payoff at settlement and maintaining his credit rating.

However, if trends in crime rates are persistent, our identifying assumption that house prices are not correlated with crime through any channel other than foreclosure is tenuous. Specifically, house price growth may be correlated with current crime rates in part because house price growth is correlated with *past* crime rates. If this is the case then the identifying assumption is violated. However, our reading of the literature on the relationship between property values and crime is that crime rates, particularly property crime rates, are unlikely to have an important effect on house prices.[32] Nonetheless, to ensure our IV strategy is not invalidated by this potential issue, we present estimates from an alternative model in which we control for aggregate property and violent crime rates lagged one, two, and three years relative to foreclosure rates. Inclusion of these controls ensures that the remaining variation in our instruments is uncorrelated with past crime rates. If estimates from this model do not substantially differ from the specification that does not control for lagged crime, this indicates that our IV estimates are not identified by this invalid source of variation.

Consistent with the theoretical and empirical literature on mortgage default, Table 3 shows that our instruments are strongly correlated with foreclosure rates. In Column (1) the excluded instrument is the five-year growth rate in the MSA-level real house price index (HPI). The coefficient is negative as expected, verifying that a decrease in HPI is correlated with increased

[32] Earlier studies typically find a negative relationship between crime and house prices (Thaler, 1978; Hellman and Naroff, 1979; Rizzo, 1979; Naroff, Hellman, and Skinner, 1980; Dubin and Goodman, 1982; Buck and Hakim, 1989), but these studies rely on cross-sectional datasets with small numbers of observations and include few covariates, and thus suffer from obvious omitted variable problems. An exception is Kain and Quigley (1970), who incorporate a comprehensive set of 39 house quality variables and other controls for neighborhood amenities, and find that crime has an insignificant effect on property values. More recent evidence suggests that increased property crime is actually associated with increased property values (Case and Mayer, 1996; Lynch and Rasmussen, 2001; Tita, Petras, and Greenbaum, 2006). Recognizing the potentially endogenous relationship between property crime and house prices, Gibbons (2004) uses an IV estimation strategy and finds that the burglary effect on house prices is statistically insignificant. We know of two studies that show violent crime may decrease house prices (Lynch and Rasmussen, 2001; Tita, Petras, and Greenbaum, 2006). However, Lynch and Rasmussen (2001) find an economically trivial effect. Tita, Petras, and Greenbaum (2006) use the tract-level murder rate to instrument for other violent crime rates and correct for measurement error in reporting; however, the strength of this instrument is questionable given that homicide is a relatively low-frequency event. Using time variation in the arrival of sex offenders to neighborhoods, Linden and Rockoff (2008) more convincingly demonstrate that concern over potential sex crime affects extremely localized house prices. Overall, the existing evidence suggests that home buyers respond to very local rather than aggregated risks of victimization, and are concerned with more serious crimes.

foreclosures. The F-statistic on a test of the null hypothesis that the instrument does not predict foreclosure rates is 74.4. In Column (2) the interaction between HPI and the logged county-level unemployment rate is added as a second instrument. The instruments are jointly significant in the first stage with an F-statistic of 35.2. The estimates in Column (3) are from the alternative specification that includes controls for lagged crime rates. As in the previous column, the instruments are house price growth, and house price growth interacted with unemployment. Here the first stage estimates are quite similar to those in Column (2).[33] In summary, the power of our instruments in predicting foreclosure rates indicates our IV estimates are not substantially biased due to a weak instruments problem.[34]

Table 4 presents our second stage IV estimation results. Each cell shows the coefficient on the lagged foreclosure rate from a separate regression of the dependent crime outcome, where the full set of controls, year dummies, and county fixed effects are included. Specification of the model and instruments varies across columns as in Table 3. Because the specifications shown in Column (2) and Column (3) are over-identified, we are able to present p-values from the Sargen-Hansen test of over-identifying restrictions, where the joint null hypothesis is that the instruments are not correlated with the error term in the crime equation. With the exception of motor vehicle theft and robbery, the p-values from these tests all exceed the 10 percent level, and we therefore fail to reject the null.

The difference in magnitudes between the IV estimates presented on Table 4 and the OLS estimates with fixed effects in Column (2) of Table 2 give credence to our concerns regarding downward bias resulting from error in our measure of foreclosure rates. With the exception of

[33] Evaluated at the mean level of log unemployment, for both Columns (2) and (3) the coefficient estimates on the instruments indicate that a one percentage point increase in the HPI is correlated with a decrease in the foreclosure rate of 0.011 percentage points, which is the same as when using only HPI as an instrument (Column 1).
[34] In every case, the F-statistic on a test of the null hypothesis that the instrument(s) can be omitted from the first stage exceeds the critical value benchmarks for weak identification provided by Stock and Yogo (2005).

rape, for each crime type the IV estimates are larger in magnitude than the corresponding OLS estimate.[35] For all crime categories, the estimates in Column (3) are quite similar to the estimates in Columns (1) and (2), alleviating the concern that our IV strategy is invalid due to persistence in crime rates.[36] Because the estimates in Column (2) are slightly more precise than those of Column (1), and because the interpretation of the model of Column (3) is not as straightforward due to the inclusion of controls for lagged crime values, we prefer the specification presented in Column (2). We focus on these estimates in our discussion.

Panel A shows a positive effect of foreclosures on aggregate property crime. Disaggregating by type, we find that foreclosures lead to statistically significant increases in burglary and larceny. As expected, the magnitude of the effect is largest for burglary. A one percentage point increase in lagged foreclosure rates increases burglary by 10.1 percent. This is a modest effect in terms of economic significance, given that a one percentage point increase represents a 2.7 fold increase in the mean foreclosure rate of our sample. The magnitude of the foreclosure effect on larceny is about half the size. No significant effect is found for the remaining property crime of motor vehicle theft.

Estimates for violent crime are shown in Panel B. Foreclosure rates increase aggregate rates of violent crime, driven by a relatively large and statistically significant effect on aggravated assault. For all other violent crime types, the estimates are small in magnitude and not statistically different from zero.

A useful comparison of our estimates of the foreclosure effect on crime, which we propose operates through its impact on passive policing, is with the effect of the formal deterrence provided

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[35] The differences between the IV estimates of Table 5, Column (2), and the OLS estimates of Table 3, Column (2), are statistically significant for aggregate property crime, larceny, burglary, aggregate violent crime, and aggravated assault.

[36] OLS results from this alternative specification (not shown) are quantitatively and qualitatively similar to the main OLS results.

by police. Levitt (2002) provides elasticity estimates of the impact of police on individual crimes, using the number of firefighters and municipal workers to instrument for police levels that are endogenous with respect to crime. His estimate of the elasticity of burglary with respect to police is -0.20. For the mean county in our sample, the IV semi-elasticity estimate of 10.1 in Table 4, Column (2), implies an elasticity of burglary with respect to foreclosures of 0.06, about a third of the magnitude of Levitt's estimate. On a per-unit basis, we estimate that an increase of 100 foreclosures yields 43.3 more burglary incidents, while a decrease in 100 police officers yields 64.8 more burglary incidents, a deterrence effect larger by a factor of 1.5.[37] In contrast, the per-unit police effect on motor vehicle theft is nearly 39 times larger than the foreclosure effect, and for murder the police effect is almost 20 times larger. We caution the reader that these comparisons are extremely rough, given that the crime types for which we estimate effects with some precision are those for which Levitt does not, and vice versa. Nonetheless, we believe that the exercise is useful in demonstrating that the magnitude of the effect of the informal crime deterrence provided by community residents is within a reasonable range of the effect of the formal deterrence provided by police.

*C. Sensitivity Analysis*

We conduct a battery of specification checks to ensure that our results are robust. Table 5 presents results from two sets of alternative specifications. To facilitate comparison, we present the estimates in terms of the elasticity of the crime rate with respect to the lagged foreclosure rate,

[37] We compute the per-unit effect of foreclosures and police on crime at the mean values of our sample. Based on Mortgage Bankers Association (MBA) data, in 2008 the total number of first lien 1-4 family residential mortgages being serviced was 45.4 million. Adjusting by a factor of 1.18 to account for the roughly 85 percent coverage rate of MBA data, the total number of mortgages in the U.S. is 53.6 million. The 799 counties in our sample contain 76 percent of housing units nationwide (based on the 2000 U.S. Census). Under the assumption that our sample also contains 76 percent of active mortgages, the mean county in our sample contains roughly 50.7 thousand mortgages. At the sample mean foreclosure rate, a one percent increase is equivalent to an increase in the number of foreclosed properties of 3.05. For the mean sample county, a one percent increase in the level of police officers is equivalent to an increase of 6.5 officers.

evaluated at sample means.[38] In Column (1) we show the elasticity estimates implied by our main IV results, where the foreclosure rate is computed using the unweighted sample of LPS and LP loans, and the regression is weighted by mean county population.

We first explore whether our results are sensitive to the use of alternative foreclosure measures. As described in Appendix A, subprime loans are over-represented in earlier years of our analysis data. Following Immergluck (2009), we compute an alternative REO-based measure of foreclosures incorporating weights to the LPS and LP data based on the state-year share of loans in the Mortgage Bankers Association's nationally representative National Delinquency Survey (NDS) that are subprime.[39] Elasticity estimates using this "weighted" foreclosure rate presented on Table 5, Column (2), are qualitatively similar to our main results. Our second alternative foreclosure rate measure incorporates in the numerator loans in any stage of foreclosure, rather than only those in REO status. Column (3) shows that although the signs and significance levels mirror those from our preferred specification, the estimates are generally larger in magnitude. This is surprising given that the "any stage" foreclosure rate is likely a noisier measure of the foreclosures that ultimately result in vacant properties.

The next set of results presented in Columns (4), (5), and (6) of Table 5 indicate that our findings for burglary are not driven by idiosyncratic foreclosure or crime activity in specific years of our sample. However, the results for larceny and aggravated assault do not hold for the earliest sample sub-period. For robbery, a significant negative foreclosure effect is actually found in the middle period, while the estimated foreclosure effect on rape varies in sign, magnitude and significance across sample sub-periods.

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[38] As with results presented earlier in the paper, underlying estimates are semi-elasticities, interpreted as the percentage change in the crime rate resulting from a one percentage point change in the lagged foreclosure rate. Semi-elasticity estimates are affected by mean levels of foreclosure rates, which varies depending on the measure used or the period of analysis. Therefore, elasticity estimates are more comparable across alternative specifications.

[39] See Appendix A for more detail on the NDS.

Other specification checks, not shown, indicate that our results are robust to alterative regression weighting schemes, where we use unweighted regression or weight cells by the number of loans used to compute the foreclosure rate, and also robust to using alternative periods to calculate house price growth as an instrument for the foreclosure rate. In all cases the magnitude and significance of the burglary results are similar, with implied elasticities ranging from 0.04 to 0.06.

In summary, across an exhaustive array of specifications, including alternative estimation strategies, foreclosure measures, sample periods, and regression weighting schemes, we find consistent evidence of a statistically significant and positive effect of foreclosures on burglary rates. In our IV estimations, sensitive to sample period, positive effects for larceny and aggravated assault are also found, with statistical significance of at least 10 percent across most specifications. We do not find robust evidence of a foreclosure effect on the remaining crime types.

## 6. Exploring the Foreclosure and Burglary Mechanism

Policy makers express concern that foreclosures result in increased crime, which we propose works through the decreased passive policing that is the consequence of increased vacancy and decreased homeownership. Some implications of our proposed hypothesis are that the foreclosure effect should be greater in areas for which passive policing plays a more important role in crime deterrence, and greater when foreclosures impact the community residents with the greatest incentive to engage in passive policing. In this section, we explore the validity of our reasoning, with a focus on burglary rates.[40]

The idea that the surveillance provided by community residents has a deterrent effect on crime, particularly in densely-populated urban areas, was originally put forth by Jacobs (1961).

[40] We focus on burglary because our main results provide clear evidence of a robust, positive effect of foreclosures on this crime. However the pattern of findings described in this section holds qualitatively for the other crime types (not shown).

Densely-populated areas where residents frequently interact naturally provide more monitoring of potential criminals than low-density neighborhoods.[41] If the decrease in "eyes on the street" is the mechanism by which foreclosures affect burglary, then the magnitude of the foreclosure rate impact should be greater in areas with higher population density. This is what we find. The first panel of Table 7 presents estimates of the elasticity of burglary rates with respect to foreclosure rates using our preferred IV specification, except that the sample is first stratified by county population density. In the first row the sample is restricted to counties in the top quartile of population density, and in the second row the sample includes only the remaining analysis counties.[42] For the high-density counties, the estimated elasticity is 0.10, compared with a statistically insignificant estimated elasticity of 0.02 for the remaining counties.

We also expect that the quality of passive policing varies by residents' "stake" in the neighborhood. We propose that homeowners are more likely than renters to engage in neighborhood surveillance, because they have a vested financial interest in their community, and may care more about neighborhood quality-of-life due to their longer average tenures (DiPasquale and Glaeser, 1999). Therefore, a decrease in owner-occupants due to foreclosure should result in more crime than foreclosure of non-owner-occupied households.[43] Results in the second panel of Table 7 are consistent with this hypothesis. Here, we compare how foreclosures on owner-occupied properties differ from foreclosures on non-owner-occupied properties in their effect on burglary.

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[41] We do not directly control for population density in our empirical specification, although our inclusion of county-level fixed effects controls for mean differences in density across counties. To the extent that foreclosures lead to increased vacancy and decreased density, inclusion of a control for population density will pick up some of the foreclosure effect on crime. Results from an alternative specification which controls for population density are qualitatively similar to those from our preferred specification.

[42] Using county-level estimates provided by the U.S. Census Bureau, we categorize counties by population density based on the maximum value of population density for the county over the sample period. Within the top quartile, mean county density is 4,111 residents per square mile, compared with 243 residents per square mile for the remaining three quartiles.

[43] Alternatively, homeowners may have more valuable goods than non-homeowner households, which increases their incentives to passive police. In addition, the value of the loot available to potential burglars may increase with foreclosures, where the increase in net payoff is greater in areas with higher levels of homeownership.

The elasticity estimate in each row is from a separate specification.[44] For foreclosures on owner-occupied homes, the magnitude of the effect is almost twice that of foreclosures on non-owner occupied homes.

We find that the foreclosure effect on burglary is largest in high-density areas and amongst homeowners, which are the communities for whom we expect passive policing to play a more important role. While we acknowledge that this evidence is merely suggestive, on the whole it reinforces our supposition that an increase in foreclosures leads to an increase in crime---at least for burglary--by reducing the level and quality of passive policing provided by residents.

## 7. What Are the Estimated Social Costs of Crime Resulting from the Recent Increase in Foreclosures?

We now use our results to make back-of-the-envelope calculations of the impact that the recent spike in U.S. foreclosures will have on future crime rates, and use estimates from the criminal justice literature to quantify the associated social cost. Before we proceed, however, we note several important caveats. First, our period of analysis precedes the current foreclosure crisis. If the effect of foreclosures on crime is non-linear, for example if the deterrence effect of neighborhood residents decreases sharply above a critical mass of foreclosures, we may underestimate the effect of

[44] Our instruments do not provide enough variation to predict well both the owner-occupied and non-owner occupied foreclosure rates in the first stage, in part because the two foreclosure rates are highly correlated. We address this by estimating two separate models. In the first we instrument for the owner-occupied foreclosure rate, while directly controlling for the non-owner-occupied foreclosure rate in the model. Thus the estimate on the owner-occupied foreclosure rate is interpreted as the effect on crime holding constant the non-owner-occupied rate. We do not attempt to interpret the coefficient estimate on the non-owner-occupied rate in this specification (not shown), due to the measurement error issues delineated in Section 4 and Appendix A that necessitate the use of IV. In the second estimate, the reverse procedure is used, allowing us to estimate the effect of non-owner-occupied foreclosures on crime, holding constant the owner-occupied foreclosure rate.

foreclosures on crime in periods when foreclosure rates are high.[45] Second, crime is likely under-reported in areas with high growth in foreclosure rates. In the case of an unoccupied home that is burglarized, there is no longer an active resident to report the incident, or neighboring residents may not notice a crime has occurred. Third, we likely understate the cost of foreclosure-related burglaries. Dornin (2008) notes instances where the cost of a burglary incident at a foreclosed home significantly exceeds the cost of the typical burglary, and where the costs of the damage inflicted during these burglary incidents far outweigh the value of the stolen items.[46] Because each of these caveats suggests we understate the impact of foreclosures on crime during the current crisis, we interpret our calculations of crime-related social costs as lower-bound estimates.

As shown in Figure 1, the rate of foreclosure starts in the U.S. grew from 1.6 percent to 4.3 percent between 2005 and 2008, an increase of 2.7 percentage points. We project that this increase in foreclosure rates will eventually raise the foreclosure rate, as measured by our REO-based computation, by approximately 1.03 percentage points in the 24 months after 2008, other things equal.[47] Based on our preferred IV estimates of Table 5, Column (2), the expected percentage change in crime associated with a 1.03 percentage point increase in foreclosure rates for each crime type is presented on Table 7, Column (1). In the case of burglary, we estimate that the increase in

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[45] Over our period of analysis we do not find evidence of a non-linear foreclosure effect on crime. We estimate an alternative model that includes a quadratic term for the foreclosure rate, and find that across crime types the quadratic term is close to zero in magnitude and statistically insignificant.

[46] See Footnote 12. Dornin (2008) reports that a burglary in Atlanta of about $40 worth of copper wire resulted in damage to the property estimated at $15,000 to $20,000. In response to burglaries of this type, in 2008 the Atlanta police department formed a special vacant home burglary team. There are also reports that foreclosed-upon owners trash their homes in acts of revenge, or strip them of goods to sell before they depart (Phillips, 2008; CBS News, 2008). In response, some banks offer "cash for keys" programs, where former owners are given a cash payment in exchange for leaving the home intact.

[47] Since 38 percent of our composite data loans which entered foreclosure in 2006 reached REO status within 24 months of the foreclosure start, the 2.7 percentage point increase in foreclosure starts in 2008, relative to the 2005 baseline, is therefore expected to increase the share of loans in REO status by approximately 1.03 percentage points over the next 24 months. We note this may be an overstatement of the future increase in REO rates, to the extent that recently introduced foreclosure mitigation programs successfully reduce the number of borrowers displaced by foreclosure actions. However, to date these programs have been relatively small in scope and do not appear to have had a substantial impact (see for example, Mulligan (2010), and Adelino, Gerardi, and Willen (2009)).

foreclosure starts from 2005 to 2008 will result in an 10.4 percent increase in the burglary rate, all else equal. The estimated changes in crime per 100,000 residents, which is the product of Column (1) and the mean crime rates presented in Table 2, are listed in Column (2).

Quantifying the social cost associated with this increased crime depends crucially on one's definition of the scope of loss. Costs incurred directly by crime victims, estimated by Miller, Cohen, and Wiersema (1996) to include productivity losses, costs of medical and social services, and quality-of-life losses, have been used, for example, to evaluate the value of reduced crime from increasing the prison population (Levitt, 1996) or from increasing school attendance (Lochner and Moretti, 2004). However, critics point out that victim-based estimates do not capture the costs of crime to family members, potential victims, and society at large (Nagin, 2001; Anderson, 1999).[48] Another approach takes into account the community's willingness-to-pay (WTP) to reduce crime, for example, the value to the public of reducing gun violence (Ludwig and Cook, 2001). Cohen, Rust, Steen, and Tidd (2004) use this "contingent valuation" methodology to assess the societal cost associated with different types of crime.[49] We prefer this latter approach over ex-post victim-based losses since it captures the costs of crime as perceived by the community-at-large, and therefore provides an assessment of the value of potential policies to the general public. Nonetheless, for completeness, and to provide the reader with a range of estimates, we present costs based on both approaches.

[48] Specifically, the Miller, Cohen, and Wiersema (1996) estimates do not include costs incurred by the criminal justice system, expenditures and behavior changes undertaken by potential victims to avoid crime, and the overall impact of crime on neighborhood quality-of-life.

[49] Cohen et al. (2004) conducted a nationally representative survey which asked individuals how much they would pay to reduce specific crimes by 1 out of 10 in their community. The WTP estimates for the average household increase by seriousness of the crime, where the estimates for burglary, robbery, assault, rape, and murder are $104, $110, $121, $126, and $146, respectively. Aggregating these figures over the number of U.S. households and dividing by the number of crimes averted yields estimates for the public's willingness-to-pay per crime. A criticism of this approach is that individuals may overstate their true WTP when making hypothetical tradeoffs. However, the survey's hypothetical collective WTP estimate of $237,000 (2000 dollars) to prevent one rape is considerably less than the actual WTP of $1.2 million (2004 dollars) to avoid a sex offense that Linden and Rockoff (2008) derive from changes in house prices subsequent to the arrival of a sex offender to the neighborhood, in conjunction with the distribution of crimes committed by sex offenders against neighbors.

Column (3) of Table 7 lists the Miller, Cohen, and Wiersema (1996) estimates of victim-based per-crime costs, while Column (6) shows the Cohen et al. (2004) estimates of community-based costs, both in 2005 dollars. Column (4) lists the victim-based cost for the mean county in our sample, equal to the product of Columns (2) and (3), multiplied by the mean county population of 2.8 (expressed in 100,000s of residents). Column (5) presents the estimated aggregate victim-based costs of the increase in crime resulting from the recent spike in foreclosure rates, computed by multiplying Column (4) by the 799 counties included in our sample. These estimates are small in terms of economic magnitude. The foreclosure-related increase in burglary is estimated to cost victims as little as $340 million on aggregate. Summing across all crime categories, the total cost is roughly $4.1 billion. The estimates of the aggregate community-based social cost of foreclosure-related crime, computed similarly, are presented in Column (8). We estimate that the collective willingness-to-pay to prevent the increase in burglary incidents due to foreclosures is $4.6 billion. Aggregating across all crime categories, other than larceny and motor vehicle theft, for which Cohen et al. (2004) do not provide estimates, total willingness-to-pay is at least $17.4 billion.

How should we interpret these values? One possible comparison is to the cost of government intervention programs. However, these programs are motivated by more than prevention of crime and neighborhood blight, and they are as of yet not clearly effective in preventing foreclosures (Mulligan, 2010). Compared to the cost of programs such as Obama's Making Home Affordable program, to which $75 billion is allocated, the benefit to society from preventing foreclosure-related crime is dwarfed. Nonetheless, our findings of non-trivial economic costs suggest that concern regarding increased crime as a result of foreclosures is not unreasonable.

## 8. Conclusions

Following Becker's (1968) seminal article on the economics of crime, a large body of empirical work has been devoted to assessing the role of opportunity costs and the effectiveness of deterrence measures, such as formal policing, in influencing criminal behavior. By examining the effect of foreclosures on crime, this paper studies the role of informal passive policing, a form of deterrence little explored in the empirical economics literature on crime. We exploit a rich county-level panel dataset, including a comprehensive set of controls guided by the existing literature on the determinants of crime. The panel nature of our data, importantly, allows us to control for macroeconomic trends and persistent differences across geographic areas. We present results from OLS and IV estimations, where the IV estimates correct for downward bias due to likely measurement error in foreclosure rates that stem from limitations of the source data.

Consistent with our expectations, both our OLS and IV strategies show that foreclosures increase burglary. This effect is robust to a myriad of alternative foreclosure measures, sample periods, and weighting schemes. Reinforcing the idea that passive policing is the mechanism by which residents deter crime, we find that the effect for burglary is larger in magnitude in more densely-populated areas. Further, the burglary effect varies with residents' incentives to engage in residential surveillance, where owner-occupants appear to generate greater deterrence. Sensitive to sample period, our IV estimates also provide evidence of positive effects of foreclosures on larceny and on aggravated assault.

Noting several important caveats, we estimate that the spike in foreclosure starts between 2005 and 2008 will result in a community-wide economic loss of $4.6 billion due to increased burglary rates, and at least $17.4 billion when considering the impact on all crimes. While it is beyond the scope of this paper to evaluate the cost-effectiveness of foreclosure prevention programs, which are motivated by more than concern over crime, our findings indicate that intervention is at least partially justified by its potential to stem the social losses that arise from increased crime resulting from foreclosures.

## Appendix

*A. Measurement Error in Foreclosure Rates*

To assess the quality of our sample of loans and the accuracy of our foreclosure rate measure, we compare our data with national summary data from the Mortgage Bankers Association's National Delinquency Survey. The NDS is a widely utilized dataset based on surveys of approximately 120 lenders and servicers covering over 80 percent of outstanding 1 to 4 unit first-liens in the U.S., providing a reliable summary of the size and composition of the U.S. mortgage market (Immergluck, 2009). Appendix Table 1 indicates that our concern regarding sample selection is warranted. Nearly 45 percent of the loans in the composite data are from LP, and therefore subprime, in 2001. This figure far exceeds the 3 percent share of loans in the NDS that are subprime. By 2006, 14 percent of loans in our sample are from LP, comparable with the subprime share in the NDS.

Because the NDS does not publish REO-based foreclosure rates, we are unable to directly compare our measure to the NDS. However, the NDS does publish data on the *overall* foreclosure rate, defined as the number of loans in any stage of the foreclosure process divided by the number of all active loans. Since the over-representation of subprime loans in our sample may be problematic, for comparison we compute an alternative "weighted" measure of the overall foreclosure rate. Based on the methodology of Immergluck (2009), we use the share of loans in the NDS that are subprime by state and year to weight our composite LPS and LP data. In Appendix Figure 1 we present overall foreclosure rates by year from NDS data, using our "unweighted" LPS and LP data, and applying the alternative weighting scheme to our composite data. Relative to the NDS, the unweighted measure is consistently larger in magnitude over all years of the analysis period, suggesting our REO-based measure of the foreclosure rate is overstated. However, this measure captures the national time trend in mortgage defaults with reasonable accuracy.[50] In contrast, the weighted foreclosure measure is closer in level to NDS, but does not follow the time trend well. As we show in our sensitivity analysis of Section 5, our results are robust to the choice of weighting. However, we prefer the unweighted measure for our main specification because it more closely follows the time trend of NDS. Based on Appendix Figure 1, we assume our unweighted foreclosure rate exhibits positive-mean measurement error.

*B. Inconsistency and Bias*

Above, we present evidence of positive-mean error in our measurement of foreclosure rates. Here, we formally show the implications of this error for our OLS estimates of the effect of foreclosures on crime, under reasonable assumptions that arise from the analysis of the data.

In practice, we observe a foreclosure rate, $f_{it-1}$, that can be expressed as follows:

$$f_{it-1} = f^*_{it-1} + u_{it-1} + \mu_{it-1} \quad \text{[A.1]}$$

We assume that the true foreclosure rate, $f^*_{it-1}$, has mean $\bar{f}^*$ and variance $\sigma^2_{f^*}$. We assume that we have classical measurement error $u_{it-1}$ with mean zero and variance $\sigma^2_u$, that is independent of the error term $\varepsilon_{it}$ of the crime equation and of the true foreclosure rate, $f^*_{it-1}$. We also assume that there is a positive-mean error component, $\mu_{it-1}$, with mean $\bar{\mu}$ greater than zero and variance $\sigma^2_\mu$, that is independent of $\varepsilon_{it}$. We allow the positive-mean error to have covariance $\sigma_{f^*\mu}$ with $f^*_{it-1}$.

Rearranging [A.1] and substituting the true foreclosure rate, $f^*_{it-1}$, into equation [2] yields:

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[50] Since subprime loans are more likely to end in foreclosure and are over-represented in our sample, we expected to see an upward bias in our measure of foreclosure rates in earlier years and no bias in the later years of our sample. However, we do not observe this, perhaps because over this period of "easy credit," subprime borrowers may have been able to refinance out of problematic loans before defaulting.

$$crime_{it} = \beta_0 + \beta_1 f_{it-1} + X_{it}'\beta_2 + Y_{it}'\beta_3 + P_{it}'\beta_4 + t'\lambda + i'\theta + \upsilon_{it} \quad \text{[A.2]},$$

where $\upsilon_{it} = \varepsilon_{it} - \beta_1(u_{it-1} + \mu_{it-1})$. Both the observed foreclosure rate $f_{it-1}$ and $\upsilon_{it}$ depend on the measurement errors, $u_{it-1}$ and $\mu_{it-1}$, therefore, if the true $\beta_1$ is positive, then there is negative covariance between $f_{it-1}$ and $\upsilon_{it}$. Specifically, under our assumptions about the characteristics of the component measurement errors, the probability limit of the OLS estimate of $\beta_1$ is:

$$\beta_1\left[1 - \frac{\sigma_{f^*\mu} + \sigma_u^2 + \sigma_\mu^2 + \bar{\mu}\bar{f}^* + \bar{\mu}^2}{\sigma_{f^*\mu} + \sigma_u^2 + \sigma_\mu^2 + \sigma_{f^*}^2}\right]$$

While we allow the positive-mean error $\mu_{it-1}$ to have covariance $\sigma_{f^*\mu}$ with the true foreclosure rate, $f^*_{it-1}$, we believe this covariance is small. Analysis of our data indicates that there is likely a level difference between our foreclosure rate measure and the "true" foreclosure rate as measured by NDS, but not much difference in the time trend. Further, the population mean of the true foreclosure rate, $\bar{f}^*$, must be positive, and by assumption the population mean $\bar{\mu}$ is positive. Therefore, the last term of the expression is very likely positive. This implies that OLS estimates of $\beta_1$ are inconsistent, and if the true $\beta_1$ is positive, are biased downward. In particular, the larger the population mean of the positive mean error, the larger is the negative bias.

*C. Wages of Low-Skilled Men*

We estimate weekly log wage rates of low-skilled men by state using a methodology similar to the approach of Gould, Weinberg, and Mustard (2002) Appendix B. Our estimates are based on a sample of men aged 18 to 65 with no college education from outgoing rotation groups of the Current Population Survey (CPS) between 2002 and 2006. Self-employed men are dropped from the sample, as are men who usually work less than 35 hours per week. We make no correction for self-selection of men into the workforce. The wage measure we use is the predicted log weekly wage rate from a regression of wages on education, experience, experience squared, and controls for race and marital status. Years of experience is not directly reported in the CPS, so we define experience equal to age less years of schooling less six. As in Gould, Weinberg, and Mustard (2002), those with top-coded weekly earnings are assumed to have earnings equal to 1.5 times the top-coded value. The estimates are done separately by year. Because county-level geographic information in the CPS is sparsely populated, we compute the mean predicted log wage by state and year. All counties within a state are assigned the same predicted wage rate for each year. For ease of interpretation of our estimates, unlike Gould, Weinberg, and Mustard (2002), we include the predicted log wage directly in our empirical analysis rather than the mean state residuals from the wage regressions. Our results are quite similar if we use the state mean residual wage instead.

*D. Covariate Analysis*

In this section we provide a discussion of the full set of OLS coefficient estimates for aggregate property crime and aggregate violent crime, presented in Appendix Table 1. For brevity we omit results for disaggregated crime types; unless otherwise noted those results are similar.

For property crime, the coefficients are generally of the expected sign, or are not statistically different from zero. Based on the specification that includes county fixed effects (Column 2), the share of the county population that is black, share that is 15 to 24 year old males, and the unemployment rate are associated with increases in aggregate property crime rates. The state prison population is positively correlated with crime, which likely reflects the endogenous relationship between incarceration and crime rates. Lastly, our results indicate that subprime lenders are more likely to originate loans in areas with lower property crime rates. In results not shown, we find that education expenditures are associated with decreased motor vehicle theft.

Generally, the estimates for aggregated violent crime follow the same pattern. However, when disaggregating by crime type, we find the counter-intuitive results that the low-skilled wage rate and increased policing levels are positively correlated with rape, and education expenditures are positively correlated with murder.[51] For the property-related violent crime of robbery, however, an increase in policing levels has the expected negative effect.

[51] These counter-intuitive results are actually not at odds with some related studies. Raphael and Winter-Ebmer (2001) similarly find some evidence of a negative relationship between unemployment and rape, while Jacob and Lefgren (2003) find that increasing time in school increases violent crime, which they attribute to increased interaction among adolescents.

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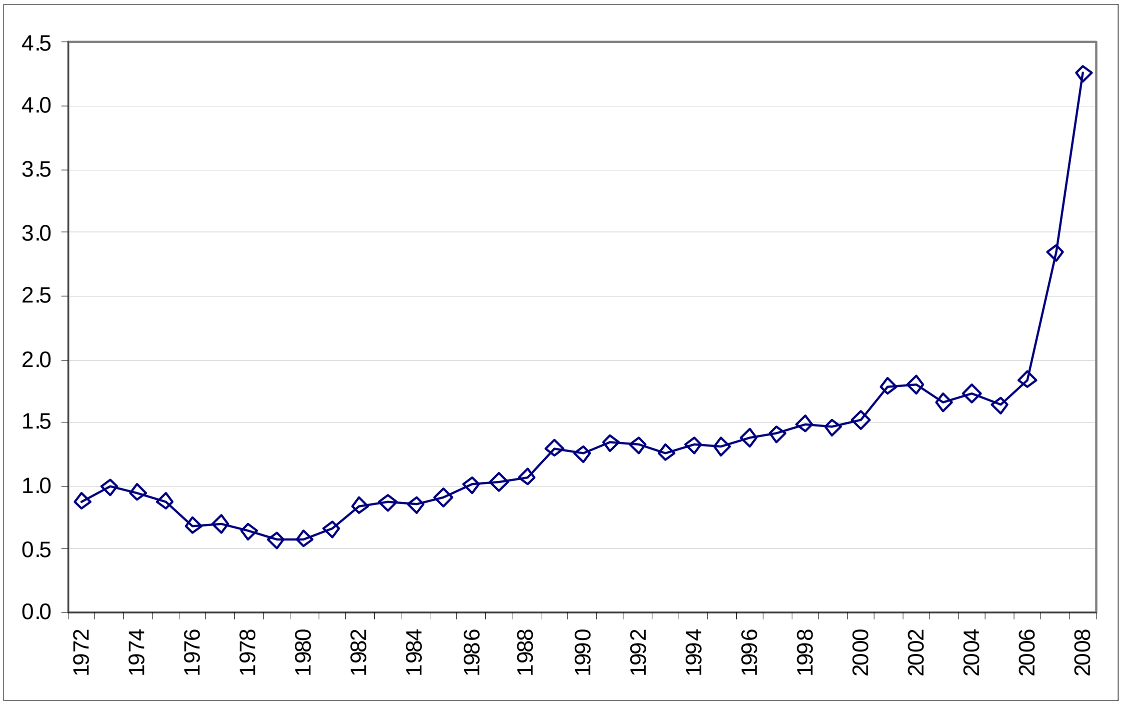
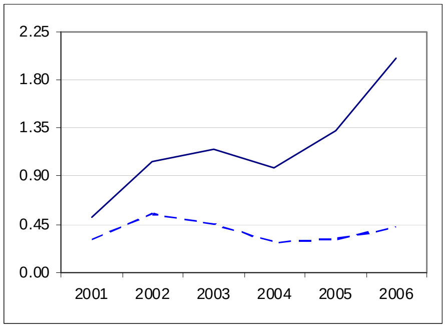
**Figure 1: U.S. Foreclosure Start Rate, 1972 to 2008**



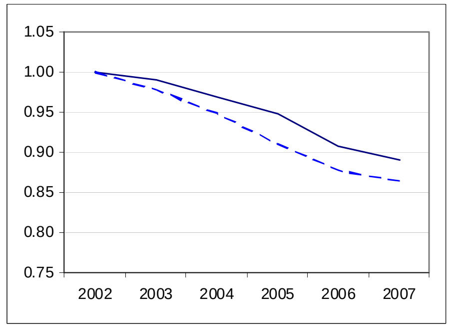

Notes: Data are from the Mortgage Bankers Association's National Delinquency Survey (NDS). As of 2008, the NDS covered includes about 45.4 million first-lien mortgages on 1-to-4 unit residential properties, or over 80 percent of outstanding first-liens in the United States.

## Figure 2: Per Capita Crime Rates by Year, High versus Low Foreclosure States

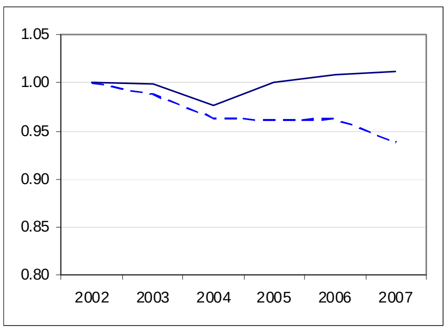
(a) Foreclosure Rate



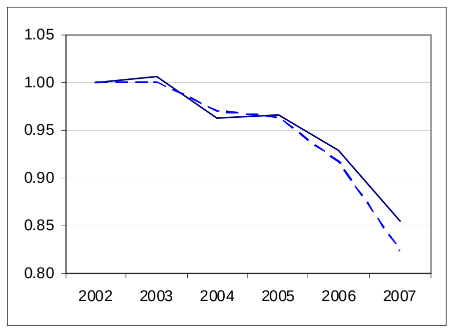

(b) Larceny



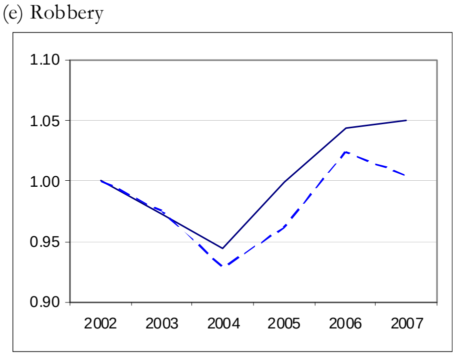

(c) Burglary



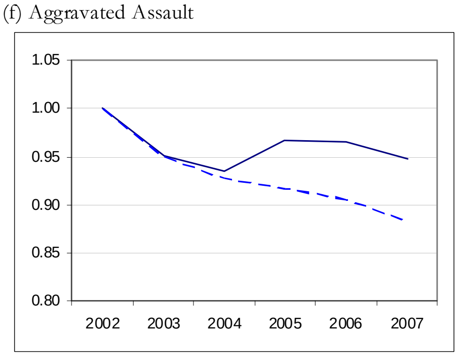

(d) Motor Vehicle Theft



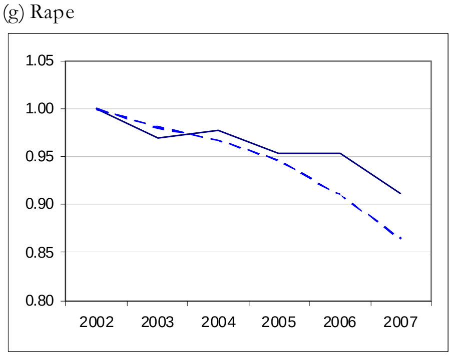

(e) Robbery



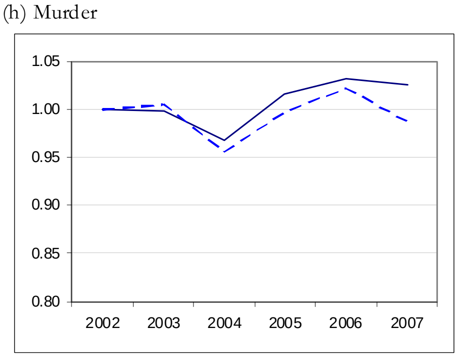

(f) Aggravated Assault




(g) Rape




(h) Murder




Notes: Solid line is the mean value over counties in the ten states with the highest foreclosure rates in 2006, weighted by mean county population. Dashed line is the weighted mean value across the remaining 40 states and the District of Columbia. The top ten foreclosure states in 2006 are MI, OH, CO, IN, GA, MS, TN, TX, KS, and SC. Foreclosure rates cover the 2001 to 2006 period. The foreclosure rate is defined as the share of active loans in REO status in each year, expressed in percentage points. Crime rates cover the 2002 to 2007 period. For Figures (b) through (h) the crime rate data presented in each line are indexed to their respective 2002 values.

**Table 1: Summary Statistics**

| Variable | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|
| Year | 2005 | 1.7 | 2002 | 2007 |
| Property Crime (per 100k) | 3,746.3 | 1,489.6 | 667.1 | 21,610.2 |
| Larceny | 2,488.8 | 976.2 | 398.3 | 15,847.5 |
| Burglary | 772.4 | 367.5 | 68.2 | 4,399.8 |
| Motor Vehicle Theft | 485.0 | 346.2 | 12.8 | 3,516.8 |
| Violent Crime (per 100k) | 541.2 | 326.2 | 24.4 | 4,830.5 |
| Robbery | 178.7 | 143.7 | 1.0 | 907.2 |
| Aggravated Assault | 324.3 | 201.7 | 0.0 | 4,364.4 |
| Rape | 31.8 | 17.4 | 0.0 | 213.6 |
| Murder | 6.4 | 5.7 | 0.0 | 51.2 |
| Lagged Foreclosure Rate (%) | 0.6 | 0.7 | 0.0 | 8.7 |
| Black (%) | 14.4 | 12.8 | 0.3 | 66.7 |
| Male and Aged 15 to 24 (%) | 7.2 | 1.2 | 4.0 | 19.9 |
| Unemployment (%) | 5.3 | 1.5 | 1.9 | 18.0 |
| Lagged Unemployment (%) | 5.3 | 1.5 | 2.0 | 17.1 |
| Income per Capita ($) | 35 | 6 | 16 | 49 |
| Log State Unskilled Wage ($) | 6.3 | 0.1 | 6.1 | 6.5 |
| State Expenditures on Education per Capita ($) | 2,403.6 | 309.9 | 1,735.0 | 3,891.2 |
| State Expenditures on Welfare per Capita ($) | 1,230.0 | 379.4 | 644.6 | 3,500.7 |
| State Sworn Police Officers (per 1k) | 2.3 | 0.7 | 1.5 | 12.5 |
| State Prison Population (per 1k) | 4.5 | 1.6 | 1.4 | 17.0 |
| HUD Regulated Entity Loan Share (%) | 5.2 | 4.8 | 0.0 | 28.2 |
| Lagged MSA Five-Year House Price Change (%) | 31.4 | 26.0 | -12.0 | 114.4 |

Notes: Cells defined by county and year, weighted by mean county population. 4,713 observations. All crime figures are measured per 100,000 persons. Lagged foreclosure rate is the percentage of mortgage loans in REO status in the previous year. Log state unskilled wage is the predicted log wage from regressing individual wages of non-college educated men from the CPS on education, experience, experience squared, and controls for race and marital status. Lagged five-year house price change is the change in the MSA-level house price index between years (t -6) and (t -1). All monetary figures are in 2005 dollars.

## Table 2: OLS Estimates, Effect of Lagged Foreclosure Rate on Crime

| | (1) | (2) |
|---|---|---|
| A. Property Crime | | |
| Aggregate Property | 0.043** | 0.011 |
| | (0.021) | (0.007) |
| Larceny | 0.019 | 0.010 |
| | (0.027) | (0.007) |
| Burglary | 0.104*** | 0.022** |
| | (0.028) | (0.009) |
| Motor Vehicle Theft | 0.038 | -0.005 |
| | (0.036) | (0.014) |
| B. Violent Crime | | |
| Aggregate Violent | -0.008 | -0.000 |
| | (0.028) | (0.010) |
| Robbery | -0.114*** | -0.022* |
| | (0.038) | (0.012) |
| Aggravated Assault | 0.015 | 0.002 |
| | (0.034) | (0.014) |
| Rape | 0.105** | -0.007 |
| | (0.046) | (0.015) |
| Murder | 0.001 | -0.001 |
| | (0.030) | (0.016) |
| County Fixed Effects | N | Y |

Notes: Each entry is the coefficient estimate from a separate regression of the crime outcome on the lagged foreclosure rate. All regressions include controls for the percent of population that is black, percent that is male aged 15 to 24, percent unemployed, income per capita, log state unskilled wage, state expenditures on education per capita, state expenditures on welfare per capita, state-level number of sworn police officers per 1,000 persons, state incarceration rate per 1,000 persons, percent of home purchase and refinance loans originated by HUD-regulated lenders, and year fixed effects. Coefficients in Column (1) are from the specification that does not include county fixed effects; coefficients in Column (2) are from the specification that includes county fixed effects. Cells are weighted by mean county population and standard errors are clustered by county. 4,713 observations. All crimes are in logs. To avoid dropping county-year observations with zero aggravated assaults, rapes, or murders, we add one to all observations of each of these crime rates before taking logs. Standard errors in parentheses. * indicates the estimate is significantly different than zero at the 10 percent level; ** five percent; *** one percent.

**Table 3: First Stage Estimates on Identifying Instruments, Dependent Variable is Lagged Foreclosure Rate**

| Instrument(s) | (1) | (2) | (3) |
|---|---|---|---|
| HPI | -0.011*** | -0.003 | -0.003 |
| | (0.001) | (0.004) | (0.004) |
| HPI * Log Unemployment | | -0.005** | -0.005** |
| | | (0.002) | (0.002) |
| Control for Lagged Crime | N | N | Y |
| F-Statistic on Excluded Instruments | 74.44 | 35.21 | 34.89 |

Notes: Entries in each column are the coefficient estimates from separate first stage regressions of the lagged foreclosure rate on the instruments. All specifications include the full set of controls listed in the notes of Table 2, and county fixed effects. In Column (3) the specification also includes aggregate property and violent crime rates lagged one, two, and three years relative to foreclosure rates. HPI is the five-year change in the MSA-level house price index, and HPI * log unemployment is the interaction of HPI and the log county-level unemployment rate, where both HPI and log unemployment are lagged one year. Cells weighted by mean county population and standard errors clustered by county, 4,713 observations. At the mean level of log unemployment (1.60), the coefficients in specifications (2) and (3) imply that a 1 percentage point increase in HPI is correlated with a decrease in the foreclosure rate of 0.011 percentage points. Standard errors in parentheses. See Table 2 for additional notes.

## Table 4: IV Estimates, Effect of Lagged Foreclosure Rate on Crime

| | HPI<br>(1) | HPI, HPI * Log Unemployment<br>(2) | HPI, HPI * Log Unemployment<br>(3) |
|---|---|---|---|
| A. Property Crime | | | |
| Aggregate Property | 0.069*** | 0.065*** | 0.066*** |
| Std. Error | (0.025) | (0.025) | (0.024) |
| Over ID test | - | 0.286 | 0.332 |
| Larceny | 0.058** | 0.056** | 0.057*** |
| Std. Error | (0.023) | (0.022) | (0.022) |
| Over ID test | - | 0.578 | 0.602 |
| Burglary | 0.104*** | 0.101*** | 0.102*** |
| Std. Error | (0.033) | (0.032) | (0.031) |
| Over ID test | - | 0.538 | 0.615 |
| Motor Vehicle Theft | 0.049 | 0.034 | 0.041 |
| Std. Error | (0.063) | (0.062) | (0.058) |
| Over ID test | - | 0.032 | 0.066 |
| B. Violent Crime | | | |
| Aggregate Violent | 0.090* | 0.094* | 0.091** |
| Std. Error | (0.049) | (0.049) | (0.046) |
| Over ID test | - | 0.364 | 0.320 |
| Robbery | -0.013 | -0.003 | -0.003 |
| Std. Error | (0.034) | (0.034) | (0.034) |
| Over ID test | - | 0.068 | 0.053 |
| Aggravated Assault | 0.149* | 0.146** | 0.140** |
| Std. Error | (0.076) | (0.074) | (0.070) |
| Over ID test | - | 0.624 | 0.612 |
| Rape | -0.015 | -0.006 | -0.008 |
| Std. Error | (0.046) | (0.043) | (0.044) |
| Over ID test | - | 0.230 | 0.170 |
| Murder | 0.041 | 0.036 | 0.032 |
| Std. Error | (0.043) | (0.041) | (0.039) |
| Over ID test | - | 0.548 | 0.452 |
| Control for Lagged Crime | N | N | Y |

Notes: Each entry is the coefficient from a separate regression of the crime outcome on the lagged foreclosure rate, the full set of controls listed in the notes of Table 2, and county fixed effects. Estimates in Column (1) are from the IV estimation in which the foreclosure rate is instrumented with HPI. Estimates in Column (2) are from the IV estimation in which the foreclosure rate is instrumented with HPI and HPI*log unemployment. Estimates in Column (3) use the same instruments as in Column (2), but the specification also includes aggregate property and violent crime rates lagged one, two, and three years relative to foreclosure rates. Over ID Test is the p-value from a Sargen-Hansen test of over-identifying restrictions. See Table 2 for additional notes.

## Table 5: Robustness of Implied Elasticity of Foreclosure Effect

| | | Alternative Foreclosure Rate | | Alternative Sample Periods | | |
|---|---|---|---|---|---|---|
| | Main IV (1) | Weighted by State-Year Subprime Share (2) | Rate of Loans in Any Stage of FC (3) | Period: 2002 to 2005 (4) | Period: 2004 to 2007 (5) | Period: 2002, 2003, 2006, 2007 (6) |
| A. Property Crime | | | | | | |
| Larceny | 0.03** | 0.02** | 0.09** | 0.00 | 0.04* | 0.03** |
| Burglary | 0.06*** | 0.04*** | 0.13*** | 0.06** | 0.04** | 0.06*** |
| Motor Vehicle Theft | 0.02 | 0.02 | 0.05 | 0.03 | 0.01 | 0.03 |
| B. Violent Crime | | | | | | |
| Robbery | -0.00 | -0.01 | -0.04 | 0.02 | -0.06** | 0.01 |
| Aggravated Assault | 0.09** | 0.06** | 0.11** | 0.05 | 0.11* | 0.09** |
| Rape | -0.00 | -0.01 | 0.04 | 0.08*** | -0.11** | 0.01 |
| Murder | 0.02 | 0.02 | -0.01 | -0.01 | 0.05 | 0.02 |

Notes: Each entry is the implied elasticity of the crime outcome with respect to foreclosures from a separate regression. In all cases the implied elasticity is derived from the underlying IV estimate on the lagged foreclosure rate, from a regression of the logged crime outcome on the lagged foreclosure rate, the full set of controls listed in the notes of Table 2, and county fixed effects. The foreclosure rate is instrumented with HPI and the HPI interacted with unemployment. Column (1) shows the elasticities implied by the IV coefficients of Table 4, Column (2), where LPS and LP loans are equally-weighted, and the regression is weighted by mean county population. See text for descriptions of the estimations used to generate the elasticities presented in Columns (2) through (8). See Table 2 for additional notes on the underlying regressions.

## Table 6: Implied Elasticity of Foreclosure Effect on Burglary

| Stratification | Elasticity |
|---|---|
| Counties by Population Density | |
| Population Density in Highest Quartile | 0.10** |
| Population Density not in Highest Quartile | 0.02 |
| Foreclosures by Owner-Occupancy Status | |
| Foreclosure Rate on Owner-Occupied Homes | 0.09** |
| Foreclosure Rate on Non-Owner-Occupied Homes | 0.05** |

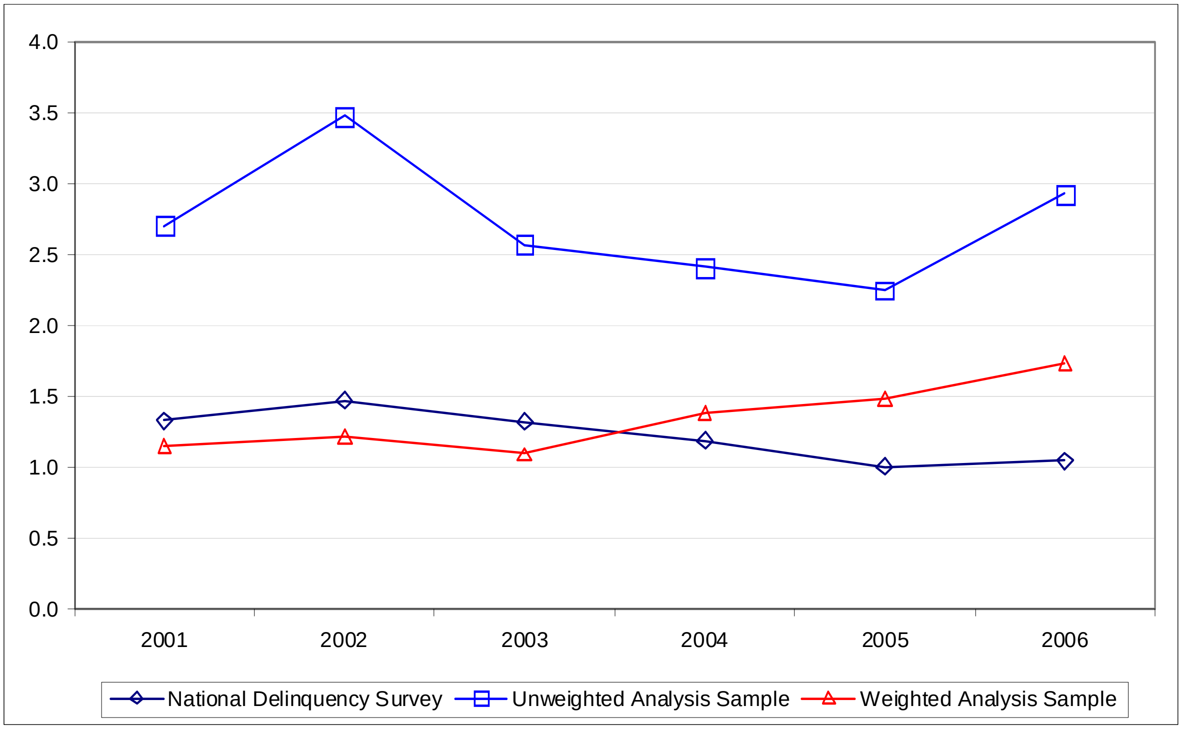
Notes: Each entry is the implied elasticity of the crime outcome with respect to foreclosures from a separate regression. In all cases the implied elasticity is derived from the underlying IV estimate on the lagged foreclosure rate, from a regression of the logged crime outcome on the lagged foreclosure rate, the full set of controls listed in the notes of Table 2, and county fixed effects. The foreclosure rate is instrumented with HPI and the HPI interacted with unemployment. In the first panel, the estimate in the first row is based on the sample of counties with population density greater than or equal to the 75th percentile (486.9 persons per square mile), N = 1,211; in the second row, the estimate is based on the sample of counties with population density less than the 75th percentile, N = 3,577. In the first row of the second panel, we instrument for the foreclosure rate on owner-occupied homes while including the foreclosure rate on non-owner-occupied homes as an additional control; in the second row, the reverse procedure is applied. See Table 2 for additional notes on the underlying regressions.

## Table 7: Estimated Costs of Crime Resulting from Increase in Foreclosure Starts between 2005 and 2008

| | | | Victim-Based Costs | | | Community-Based Costs | | |
|---|---|---|---|---|---|---|---|---|
| | Estimated Percentage Change in Crime Rates from 2005-2008 Foreclosure Start Increase (1) | Estimated Change in Crime per 100k (2) | Costs Per Crime (2005 $) (3) | Estimated Mean Cost Per County (4) | Estimated Aggregate Cost for Analysis Counties (Mill $) (5) | Costs Per Crime (2005 $) (6) | Estimated Mean Cost Per County (7) | Estimated Aggregate Cost for Analysis Counties (Mill $) (8) |
| A. Property Crime | | | | | | | | |
| Larceny | 5.7 | 143.0 | $500 | $200,950 | $161 | | | |
| Burglary | 10.4 | 80.0 | $1,892 | $425,615 | $340 | $25,773 | $5,798,144 | $4,633 |
| Motor Vehicle Theft | 3.5 | 16.9 | $5,000 | $237,778 | $190 | | | |
| B. Violent Crime | | | | | | | | |
| Robbery | -0.3 | -0.6 | $10,811 | -$16,712 | -$13 | $239,175 | -$369,738 | -$295 |
| Aggravated Assault | 15.0 | 48.6 | $12,703 | $1,734,505 | $1,386 | $72,165 | $9,853,845 | $7,873 |
| Rape | -0.6 | -0.2 | $117,568 | -$64,588 | -$52 | $244,330 | -$134,227 | -$107 |
| Murder | 3.7 | 0.2 | $3,972,973 | $2,630,729 | $2,102 | $10,000,000 | $6,621,562 | $5,291 |

Notes: Column (1) is the product of the estimates presented on Table 4, Column (2), and 1.03, the estimated percentage point increase in the REO-based mean foreclosure rate implied by the increase in foreclosure starts between 2005 and 2008. Column (2) is the product of Columns (1) and mean crime rates from Table 2. Column (3) shows estimated average victim losses per crime provided by Miller, Cohen, and Wiersema (1996). Column (4) is the product of Columns (2) and (3), multiplied by the mean sample county population of 2.8, expressed in units of 100k persons. Column (5) is the product of Column (4) and the 799 counties of our analysis sample. Column (6) shows estimates of community-wide willingness-to-pay to avert one crime, provided by Cohen, Rust, Steen, and Tidd (2004). Column (7) is the product of Columns (2) and (6), inflated by 2.8. Column (8) is the product of Column (7) and 799. All monetary amounts are in 2005 dollars.

**Appendix Figure 1:  Share of Loans in Any Stage of Foreclosure, by Data Source**




Notes:  Share of loans in any stage of foreclosure is equal to the total number of loans in the legal process of foreclosure divided by the total number of active loans; only first-lien mortgages on one-to-four unit residential properties are included in the calculation.  "National Delinquency Survey" data are based on annual averages of quarterly national summary data published by the Mortgage Bankers Association.  "Analysis Sample" data are national aggregates of our unweighted measure of foreclosure rates, computed using our composite sample of LPS and LP loans.  "Weighted Analysis Sample" data are national aggregates of our weighted measure of foreclosure rates, computed using our sample of LPS and LP loans weighted as in Immergluck (2009).  See Figure 1 for notes on the NDS.

## Appendix Table 1: Active Loans by Type and by Data Source

| | Aggregated Loan-Level LPS and LP | | | | National Delinquency Survey | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Year | LPS | LP | Total | Share of Total from LP (%) | Non-Subprime | Subprime | Total | Share of Total from Subprime (%) | Total LPS + LP as Share of Total NDS (%) |
| 2001 | 1.0 | 0.8 | 1.7 | 44.9 | 31.8 | 0.9 | 32.7 | 2.7 | 5.3 |
| 2002 | 1.7 | 1.3 | 3.1 | 43.6 | 32.6 | 1.3 | 33.9 | 3.8 | 9.0 |
| 2003 | 5.4 | 2.1 | 7.5 | 28.3 | 34.1 | 3.1 | 37.2 | 8.4 | 20.2 |
| 2004 | 10.4 | 3.1 | 13.5 | 22.7 | 34.5 | 4.9 | 39.3 | 12.3 | 34.3 |
| 2005 | 20.1 | 3.1 | 23.3 | 13.5 | 35.7 | 5.5 | 41.2 | 13.4 | 56.4 |
| 2006 | 20.9 | 3.4 | 24.3 | 14.0 | 37.5 | 6.0 | 43.5 | 13.7 | 55.9 |

Notes: All counts are in millions of loans. The first two columns show the count of owner-occupied, first-lien purchase or refinance mortgages on one-to-four unit residential properties from LPS and LP data, respectively. LPS data include only loans that are not privately-securitized subprime; LP data includes only loans of this type. See Figure 1 for notes on the NDS.

**Appendix Table 2: OLS Estimates, Effect of Lagged Foreclosure Rate on Aggregate Crime**

| | Property | | Violent | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Lagged Foreclosure Rate | 0.043** | 0.011 | -0.008 | -0.000 |
| | (0.021) | (0.007) | (0.028) | (0.010) |
| Percent Black | 0.012*** | 0.012** | 0.028*** | 0.037*** |
| | (0.001) | (0.006) | (0.002) | (0.013) |
| Percent Male 15 to 24 | 0.022** | 0.042*** | -0.003 | -0.029 |
| | (0.010) | (0.015) | (0.015) | (0.022) |
| Log Unemployment | 0.090 | 0.065* | 0.345*** | 0.116*** |
| | (0.083) | (0.036) | (0.101) | (0.042) |
| Lagged Log Unemployment | 0.187** | 0.043 | 0.434*** | 0.043 |
| | (0.087) | (0.030) | (0.114) | (0.042) |
| Log Income per Capita | -0.300*** | -0.049 | 0.073 | -0.102 |
| | (0.104) | (0.159) | (0.140) | (0.181) |
| Log State Unskilled Wage | -0.427 | -0.294 | -2.283*** | 0.510 |
| | (0.392) | (0.230) | (0.551) | (0.370) |
| Log State Expenditures on Education per Capita | -0.806*** | -0.067 | -1.332*** | 0.064 |
| | (0.143) | (0.115) | (0.202) | (0.176) |
| Log State Expenditures on Welfare per Capita | -0.230*** | -0.091 | 0.061 | -0.144 |
| | (0.085) | (0.067) | (0.176) | (0.105) |
| Log Sworn Police Officers per 1k | -0.288*** | -0.155 | -0.007 | -0.160 |
| | (0.103) | (0.119) | (0.171) | (0.114) |
| Log Prison Population per 1k | 0.004 | 0.038** | -0.021 | 0.059** |
| | (0.014) | (0.018) | (0.023) | (0.025) |
| HUD Regulated Entity Loan Share | 0.001 | -0.002** | 0.002 | -0.004*** |
| | (0.002) | (0.001) | (0.003) | (0.002) |
| County Fixed Effects | N | Y | N | Y |
| R-Squared | 0.505 | 0.965 | 0.527 | 0.968 |

Notes: Cells weighted by mean county population and errors clustered by county, 4,713 observations. All specifications include year fixed effects. See Table 2 for additional notes.