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Bank Mergers and Crime: The Real and Social Effects of Credit Market Competition

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ABSTRACT

Using a unique sample of commercial loans and mergers between large banks, we provide micro-level (within-county) evidence linking credit conditions to economic development and find a spillover effect on crime. Neighborhoods that experience more bank mergers are subject to higher interest rates, diminished local construction, lower prices, an influx of poorer households, and higher property crime in subsequent years. The elasticity of property crime with respect to merger-induced banking concentration is 0.18. We show that these results are not likely due to reverse causation, and confirm the central findings using state branching deregulation to instrument for bank competition.

WE EXAMINE THE REAL AND SOCIAL IMPACT of local credit competition by documenting a link between the competitiveness of the local banking market, credit conditions, urban development, and crime. Several recent studies (Peek and Rosengren (2000), Cetorelli and Gambera (2001), Klein, Peek, and Rosengren (2002), and Burgess and Pande (2003)) tie credit market imperfections to diminished real economic activity and growth. Other studies connect the economic environment to property crime (Freeman (1999), Levitt (2001), Raphael and Winter-Ebmer (2001), and Nilsson (2004)). It is reasonable to hypothesize, therefore, that reductions in bank competition may lead to economic decline and, subsequently, increases in property crime. We empirically investigate the effects of bank consolidation and increased market power on credit availability and real activity using a unique sample of commercial loans and mergers between large banks in the 1990s. We provide micro-level evidence that

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neighborhoods that experience greater reductions in bank competition due to bank mergers are subject to future higher interest rates, diminished local construction, lower real estate prices, and an influx of poorer households. We then examine whether these changes impact the social environment by studying subsequent changes in crime rates across neighborhoods, and find an associated increase in property crime. Importantly, these results do not appear to be due to reverse causation since, among other evidence, our large bank mergers are not preceded by or contemporaneously correlated with crime increases or economic declines in the neighborhoods in which they operate.

Our empirical strategy considers only neighborhood (within-county) variation in bank loan market competitiveness induced by mergers between nonfailing banks of at least $1 billion in assets to capture exogenous changes in the competitiveness of local banking markets. The size, scope, and health of these banks make it unlikely that economic declines in the local neighborhoods we examine drive the mergers. We then analyze the local market effects of these mergers on subsequent loan supply, interest rates, property prices, investment activity, economic and demographic variables, and crime. It is worth emphasizing at the outset that we only examine the relation between changes in bank competition through large mergers and changes in economic conditions and crime across neighborhoods. Bank competition cannot explain the aggregate national decline in crime over the last few decades nor differences in the level of crime across regions. Indeed, we control for both the time trend and variation in crime levels across counties in our analysis.

Local financial development can be critical to economic growth even in a well-integrated financial environment (Guiso, Sapienza, and Zingales (2004)). The market for small commercial real estate loans is localized due to information and agency considerations (Garmaise and Moskowitz (2003, 2004)). For these reasons, merger-induced changes in the local competitive environment can have sizeable effects. We find that when loan competition declines (via large bank mergers), interest rates charged on loans rise significantly and borrowers receive smaller loans.[1] Using bank-specific fixed effects, we show that the same bank sets different loan terms in areas with different levels of competition. It is important to note, however, that these mergers lead to short-term distortions in the loan market. In the longer term, entry of new banks and increased loan supply by existing banks should lead to a restored competitive equilibrium (Berger et al. (1998)). Consistent with this notion, we show that the impact of mergers on the local competitive environment lasts about 3 years, ceasing to have any significant effect beyond that time.

It is during this short-term distortion of the local loan market that large banking mergers have substantial real economic implications. We find that both commercial real estate development, often a marginal investment for banks, and new construction activity fall significantly. In addition, local property prices

[1] Although several empirical studies examine the effects of bank mergers on loan markets, most analyze aggregate market data. An exception is Sapienza (2002), who provides evidence on individual loan contracts.

decline. Examining local demographic and migration data from the census, we then find that unemployment in the area rises, median income drops, and income inequality increases. Moreover, the income of new arrivals into the neighborhood is below that of long-term residents, suggesting an influx of lower-income households into the area.

These changing economic and demographic characteristics provide a plausible channel through which crime may rise. In particular, we find subsequent increases in burglary and, more broadly, property crimes in these areas. There appears to be, however, no significant increase in homicide or other personal crimes, which have a tenuous relation with economic activity; a general conclusion of the crime literature is that economic decline is linked closely to property crime but loosely, if at all, to personal/violent crime (see Levitt (2001), Raphael and Winter-Ebmer (2001), and Nilsson (2004)). Our results are consistent with bank mergers leading to economic deterioration, which in turn results in more property crime, but has no definite effect on violent crime. We find that the elasticity of property crime risk with respect to merger-induced bank concentration is 0.18. The crime literature reports elasticities of crime with respect to direct economic variables such as wages and unemployment in the range of 0.6 to 0.9 (Grogger (1998) and Rickman and Witt (2003)), so the magnitude of the elasticity that we find suggests an indirect effect of credit market terms on property crime via economic mechanisms. The effect on crime itself is small, but clearly important to local residents. Applying our results to national crime figures from the FBI's *Uniform Crime Reports*, a mean decline in banking competitiveness due to mergers from 1992 to 1995 is associated with approximately 24,300 more property crime offenses over the period 1995 to 2000.

We also find that the differential effect of mergers on property crime risk varies across neighborhoods. Areas that are already characterized by high banking concentration and low median income are more severely affected by a merger-induced reduction in loan competition. This suggests that the real and social costs of bank mergers can be much higher in fragile neighborhoods.

While we believe that this body of evidence suggests a causal (though indirect) link between bank mergers and crime, a natural alternative explanation for these results is reverse causation. Consider the hypothesis that future crime increases in some way prompt present-day mergers. We first note that commercial real estate loans are only a small part of banks' portfolios and, in any case, the future price effects we find correlated with mergers are small. In addition, we analyze only the mergers of very large nonfailing banks; such mergers are almost certainly not driven by the possibility of future neighborhood-level declines. Nonetheless, we investigate the hypothesis that some unobservable economic variables that affect future crime motivate present-day bank mergers.

Evidence against this alternative hypothesis is summarized in the following set of results. First, we find that while bank mergers antedate economic deterioration and increases in crime, economic decline and increases in crime are not correlated with either contemporary or future bank mergers. Second, while we employ only nonfailing banks in our merger variable, as a robustness

test we also compute a change in concentration measure from the mergers of failing banks only, since these are the most likely to be associated with economic declines in the neighborhoods in which they operate. We find, however, that mergers between failing banks do not predict future crime increases. While difficult to reconcile with reverse causation, this finding is consistent with mergers affecting competition, since the removal of a failing bank from the market probably has only a small competitive effect. Third, we also examine changes in the number of nonlending financial institutions such as insurance companies and securities brokers. These institutions, like banks, are affected by the current and past local economic environment; unlike banks, however, they should have little impact on local credit conditions. Consistent with this theory, we find no association between the consolidation of nonlending institutions and future crime. This finding fails to support reverse causation. Finally, when we consider mergers that should not affect the local competitive environment, for example, when outsider banks acquire insider banks or when mergers occur within the same bank holding company (BHC), such mergers do not predict changes in credit conditions or increases in future crime. In sum, this collection of results suggests that the reverse causation theory is not very plausible.

Although we focus on neighborhood variation in economic activity and crime, we also examine county- and state-level variation to obtain additional construction and crime statistics and show that an increase in bank mergers predicts reduced construction activity and higher burglary rates, but again, does not predict homicide rates. At the state level, we also employ another measure of bank competition using variation in state deregulation of bank branching. Branching deregulation had the opposite effect of mergers by improving credit competition. In the years immediately following deregulation of branching restrictions within a state, we find decreasing banking concentration, reduced loan rates, increasing loan sizes, rising property prices, and declining burglary rates, with no effect on homicide. These findings further support the link between credit conditions, economic activity, and property crime.

Our findings relate to the literature on the real effects of credit market imperfections.[2] They are also broadly consistent with the literature on finance and development.[3] Complementing this literature, we provide micro-level

[2] In a cross-country comparison, Cetorelli and Gambera (2001) find that concentrated banking industries depress growth. Peek and Rosengren (2000) and Klein et al. (2002) show that the declining position of Japanese banks in the 1990s led to significant reductions in construction activity and foreign direct investment in the U.S. Hancock and Wilcox (1997) demonstrate that the capital crunch of the early 1990s significantly reduced both commercial and residential real estate construction and sales. Black and Strahan (2002) and Jayaratne and Strahan (1996) show that new business incorporations and income growth followed U.S. branching deregulation. Burgess and Pande (2003) find evidence that the rural bank branch expansion program in India from 1977 to 1990 lowered rural poverty and increased nonagricultural output.

[3] King and Levine (1993), Rajan and Zingales (1998), and Demirguc-Kunt and Maksimovic (1998) show that financial development is important for real growth across countries and Guiso et al. (2004) find that local financial development is a key determinant of local economic success within Italy.

evidence of U.S. neighborhood credit markets having important impacts on local real activity. We further provide new evidence on spillovers from credit market competitiveness to social outcomes such as crime. More broadly, the potential connections among finance, real activity, and social activity are an intriguing area of further study.

The rest of the paper is organized as follows. Section I details the commercial loan, crime, construction, census, and bank merger data used to examine the real and social effects of financial access. Section II describes our empirical strategy to exogenously measure changes in banking competitiveness through large bank mergers. Section III analyzes the impact of bank competition changes (through mergers) on subsequent loan supply, economic and demographic variables, and crime risks. Section IV provides further evidence on the exogeneity of the bank merger variable and tests alternative theories such as reverse causation. Section V conducts county- and state-level analyses employing state deregulation of bank branching as another instrument for competition. Finally, Section VI concludes the paper.

I. Data and Summary Statistics

We briefly describe the variety of data sources used in the paper.

A. Transaction-Level Data from the United States Commercial Real Estate Market

Our sample consists of 22,642 commercial real estate transactions drawn from across the United States over the period January 1, 1992 to March 30, 1999 from COMPS.com, a leading provider of commercial real estate sales data. Garmaise and Moskowitz (2003, 2004) provide an extensive description of the COMPS database and detailed summary statistics. The data span 11 states: California, Nevada, Oregon, Massachusetts, Maryland, Virginia, Texas, Georgia, New York, Illinois, and Colorado, plus the District of Columbia.

Properties are grouped into three mutually exclusive types: apartments (defined as multifamily dwellings, apartment complexes, condominiums, and townhouses), vacant land, and commercial and industrial buildings. Panel A of Table I reports summary statistics on the properties in our sample. The average (median) sale price is $2.4 million ($600,000), and there are only 42 transactions involving Real Estate Investment Trusts (REITS) (less than 0.2% of the sample). Capitalization rates, defined as net income on the property divided by sale price, and property age are also reported.

The COMPS database provides detailed information about specific property transactions, including location, identity and location of market participants, and financial structure. For example, COMPS provides eight-digit latitude and longitude coordinates of the property's location (accurate to within 10 meters). From these, we construct characteristics of the local market in which each property resides by matching each property with local crime risks and census statistics.

Table I

Summary Statistics of Bank Mergers, Commercial Real Estate Transactions, and Crime Risk in the U.S. from 1992 to 1999

Panel A reports the distributional characteristics of the property transactions in the COMPS database over the period January 1, 1992 to March 30, 1999. The mean, median, standard deviation, and 1st and 99th percentiles of sale price, capitalization rate (net operating income divided by sales price), property age, loan size (loan-to-value), loan interest rate, and loan maturity are reported. Panel B reports statistics on the banks making loans in the COMPS database and on the bank merger and acquisition activity of those banks (obtained from the Federal Reserve's Commercial Bank and Bank Holding Company Merger Database and Call Report Database). Statistics are reported separately for large banks that have at least $1 billion in assets. Mergers are documented separately for those between two large banks, those involving a failing bank (where one of the banks required assistance from the FDIC, RTC, NCUA, state, or other regulatory agencies), and for those banks within the same bank holding company (BHC). These three classes of mergers are mutually exclusive. The mean and standard deviation of the actual Herfindahl concentration index of commercial real estate bank loans (from all banks) within 15 miles of the property (excluding the property itself) from year yr_1 to year yr_2 are also reported. Panel C reports local crime risk scores at three points in time (1990, 1995, and 2000) for homicide and burglary for the location in which the property resides. Crime scores relative to their national and county averages are obtained from CAP Index, Inc. Correlations between the Cap Index crime risks and the FBI's Uniform Crime Report (UCR) crime rates (reported number of crimes per capita) are also reported.

Panel A: Summary Statistics of Sale and Loan Transactions

	Mean	Median	Standard Deviation	1st%	99th%
Sale price ($US)	2,387,000	600,000	10,481,000	110,000	27,000,000
Capitalization rate (%)	10.09	9.82	2.79	4.64	17.27
Property age (years)	35.55	30.00	30.53	1	106
Loan size (% of price)	75.83	77.77	15.99	20.00	100.00
Interest rate (%)	8.28	8.25	1.40	5	11.75
Maturity (years)	15.95	15.00	10.74	0.50	30.00

Panel B: Bank Merger and Acquisition Activity

	All Banks	Large Banks (> $1 billion in assets)		
No. of banks	769	210		
No. of loans made	8,364	5,092		
Value of loans made (billion)	$9.8211	$7.3092		
Minimum loan value	$1,318	$1,318		
Average loan value	$1,174,630	$1,435,720		
Maximum loan value	$220 million	$220 million		
Average value of property	$1,616,450	$1,986,632		
No. of states received loans	12	12		
No. of counties received loans	42	38		
No. of zip codes received loans	1,028	881		
	All	Large	Failing	Same BHC
No. of bank mergers and acquisitions	316	80	18	75
	Actual level of bank concentration: $BankHI^{actual\ level}$			
	1992:1995	1992:*saleyear*		1992:1999
Mean	0.0526	0.0576		0.0589
Standard deviation	0.0273	0.0291		0.0310

(continued)

Table I —*Continued*

Panel C: Summary Statistics of Crime Risk from Cap Index, Inc.

	Relative to National Average			Relative to County Average		
	Mean	Standard Deviation	Correlation with UCR	Mean	Standard Deviation	Correlation with UCR
Homicide risk$_{1990}$	1.95	1.85	0.33	1.49	1.06	−0.11
Homicide risk$_{1995}$	2.01	1.97	0.38	1.51	1.10	−0.12
Homicide risk$_{2000}$	2.02	1.95	0.37	1.52	1.09	−0.11
Burglary risk$_{1990}$	3.32	3.55	0.58	1.80	1.25	−0.05
Burglary risk$_{1995}$	3.04	3.04	0.56	1.81	1.15	−0.05
Burglary risk$_{2000}$	2.94	2.90	0.56	1.80	1.10	−0.05

A.1. Financial Structure and Terms

The COMPS data contain detailed financing information for each property transaction. We focus on the terms of the loan contract and the size and presence of loans. As Panel A of Table I indicates, the average loan size (from bank and nonbank institutions) as a fraction of sale price is over 75%. The data also contain rich detail on loan terms including the annual interest rate, the maturity of the loan, whether the loan rate is floating or fixed, whether the loan is amortized and the length of amortization, and whether the loan is subsidized by the small business administration (only 1.3% of loans).

Panel B of Table I describes the subset of loans issued by identifiable banks (for some loans, the source is not identified; other loans are made by insurance companies, investment banks, or other financial institutions). We identify 769 distinct loan-issuing banks in our sample. Of these, we classify 210 as "large banks", that is, those that have at least $1 billion in assets. We employ as our instrument for competition mergers among these large banks. The large banks extended 5,092 loans, comprising 60% of the total number of loans, but more than 74% of debt financings on a dollar basis.

B. Bank Merger Activity

Since we employ mergers between large banks as our instrument for changes in bank competition in the local area (discussed in detail in the next section), we report in Panel B of Table I summary statistics on bank merger and acquisition activity over our sample period. Information on bank mergers is obtained from the Commercial Bank Database of the Federal Reserve. Information on bank high holding companies, used to track asset levels and acquisitions, is obtained from the Call Report Database (Reports of Condition and Income) of the Federal Reserve, Federal Deposit Insurance Corporation, and Comptroller of the Currency.

We include only the following types of mergers and acquisitions in our study:

(i) Bank mergers (i.e., combinations in which one FDIC certificate is surrendered) between two banks each with at least $1 billion in assets on

the quarter-end preceding the merger. The merger must be classified as "nonfailing"[4] and cannot be between two banks owned by the same BHC.

(ii) Bank acquisitions in which a high holding company owning banks with total assets of at least $1 billion in assets acquires a bank with at least $1 billion in assets. The acquired bank must have previously been owned by a different high holding company or by no holding company. The acquired bank retains its FDIC certificate.

Of the 769 different banks that issued loans in our sample, 316 were involved in a merger or acquisition at some point during the period 1992 to 1999. Of the 210 large banks in our sample, 80 were involved in mergers and acquisitions that met both the criteria listed above. The large number of mergers and acquisitions over our sample period is critical for our empirical strategy to identify exogenous changes in bank competition. Examples of mergers and acquisitions included and excluded from our study are given in Table AI of the Appendix. An example of the first type of included union is the merger between Wells Fargo and First Interstate Bankcorp in April – June, 1996. The various banks owned by the First Interstate holding company were merged with Wells Fargo, and the acquired banks gave up their FDIC certificates. An example of the second type of included combination is the acquisition of First Chicago by the National Bank of Detroit (NBD Bank). The holding company that had previously owned NBD purchased First Chicago and the name of the holding company was changed to First Chicago NBD Corp. Both NBD Bank and First Chicago retained their separate FDIC certificates. For simplicity, henceforth we will refer to mergers as only those that meet the criteria above. Table AI of the Appendix also gives examples of mergers involving "failing" banks, mergers between two banks in the same holding company, and mergers involving banks with less than $1 billion in assets. These mergers are all excluded, though we employ them in "placebo" tests and tests of alternative theories later.

We select our merger inclusion criteria to minimize the possibility that any association we find between bank mergers and a subsequent worsening of credit terms, reduction in local real activity, and increase in crime is driven by endogenous factors such as forecasted neighborhood changes. We include only those mergers that are least likely to be driven by concerns over local decline; that is, mergers between very large nonfailing banks. These banks not only comprise a significant fraction of sample loans but also issue loans across a wide geographic area, both in our sample (across all 12 states, 38 counties, and 881 zip codes) and more generally across the United States. From Call Report data we estimate that the average large bank in our sample has $5.84 billion in assets and 16,804 branch offices in 41 states, 949 counties, and 6,644 different zip codes. Given the size and geographic diversity of these banks, it seems unlikely that local economic conditions of the small neighborhoods (within county) we

[4] "Nonfailing" mergers are those not requiring assistance from the FDIC, RTC, NCUA, state, or other regulatory agencies.

examine could be driving the mergers between these large institutions. Nevertheless, we investigate this alternative hypothesis in Section IV. Mergers within a BHC are excluded because they are unlikely to yield significant competitive effects. In Section IV, we provide evidence that large bank mergers serve as a valid instrument for future bank market competition.

C. Crime Data

We augment our sample with crime risk data from CAP Index, Inc. Cap Index computes a crime score for a particular location by combining data from police reports, the FBI's Uniform Crime Report (UCR), client loss reports, and offender and victim surveys with geographic, economic, and population data. The combination of crime risk information from several sources allows for a richer and more geographically refined measure of crime than that which can be provided by the FBI's UCR, for instance.[5] Cap Index supplies crime scores to businesses evaluating relocation options or banks considering automated teller machine locations. We match each property's eight-digit latitude and longitude coordinates with the crime score index for those coordinates. Hence, we obtain a property-specific crime score, rather than a county average or coarser crime rate. This is particularly useful since we can match crime risks with particular property transactions and therefore with particular financing characteristics associated with those transactions. However, we also recognize that crime scores of individual properties within an area will be highly correlated; we deal with this issue in our econometric analysis.

The crime scores measure the probability that a certain crime will be committed in a given location relative to national and county levels of crime. Thus, a crime score of one means that the likelihood of a particular crime being committed in a given location is the same for the location as the national (or county) average for that year. Crime scores range from 0.1 to 20. Cap Index scores the seven crimes listed in the FBI's UCR as Part 1 Offenses: homicide, rape, robbery, aggravated assault, burglary, larceny, and motor vehicle theft, where the first four are classified as "crimes against persons" and the last three as "crimes against property." Indices of personal crime and property crime, which are averages of the respective crime scores in those categories, are reported. We also consider two specific crime scores, homicide and burglary. Cap Index provides crime scores at three points in time: 1990, 1995, and 2000. We examine the changes in crime risk over the two periods before and after 1995.

We also supplement our analysis with county- and state-level crime rate data from the FBI's UCR for robustness. As Panel C of Table I indicates, the correlation between the nationally adjusted Cap Index crime score and county-level

[5] For example, UCR data are typically at the county or city level. Hence, a property on the south side of Chicago would have the same crime rate as one downtown, which would have the same score as any city in Cook county. Cap Index scores offer wide variation in crime risks within a city and even within neighborhoods. For example, for the 60610 zip code (downtown Chicago), our data provide 44 different burglary risk scores for the year 2000. While these scores are highly correlated, they provide additional micro-level variation over UCR crime rates.

UCR crime rates is fairly high (0.37 for homicides and 0.56 for burglaries in 2000). However, the county-adjusted Cap Index scores are virtually uncorrelated with UCR county crime rates, providing finer geographic variation in crime within counties.

D. Census and Construction Data

Finally, we supplement our sample with census statistics on income distribution, median housing value, unemployment, and population at the census tract level from the 1990 and 2000 U.S. Censuses conducted by the U.S. Census Bureau, as well as with new construction statistics, total construction value and units, from at the county level.

II. Using Bank Mergers to Measure Exogenous Competition Changes

We would like an exogenous measure of banking competition that is otherwise unrelated to any of our dependent variables, namely demand for financing, financing terms, prices, measures of real activity, and crime. Directly measuring changes in bank competition for commercial loans (e.g., changes in the concentration of bank market share with respect to commercial real estate loans) within a given area may be problematic since it is likely that the competitive environment will vary simultaneously with the performance of local real assets and activity. For instance, a decline in a district's property values might lead some banks to withdraw from lending activity in the area. Such endogeneity problems would lead to inconsistent coefficient estimates.

One approach to addressing the endogeneity problem is to satiate the regression with as many local market and property attributes as are available. We add a set of controls, including geographic, year, and property-type fixed effects, which attempt to do this. In addition, we employ lagged measures of competition to avoid simultaneity problems. However, unobservable differences in local real estate market conditions and property characteristics can also cause endogeneity problems that make detection of an effect difficult in the data. Therefore, we focus on identifying exogenous variation in banking competitiveness.[6]

A. Banking Market Mergers and Acquisitions

We use large nonfailing commercial bank merger and acquisition activity to measure changes in bank competition that are otherwise uncorrelated with subsequent neighborhood-level variation in loan rates, prices, investment, and crime. As argued earlier, the size and scope of the banks involved in these mergers make it unlikely that local economic trends motivate the mergers. Furthermore, Rhoades (2000) documents that mergers and acquisitions are

[6] Another approach is to use property fixed effects estimation to difference out the unobservables, which assumes the unobservable effects are constant over time. However, since we do not have panel data in the sense that almost no properties appear multiple times in our sample, this is not possible.

the dominant source of changes in banking structure from 1980 to 1998, as the number of new chartered banks and the number of banks that fail are considerably smaller than the number of mergers and acquisitions, where new and failed banks account for less than 2% of local market deposits.

For each property, we compute a measure of bank competition using a local bank loan Herfindahl concentration index. More competitive bank markets are those with lower Herfindahl measures.[7] We then consider the effects of bank mergers and acquisitions on these Herfindahl measures. As we discuss in Section II.B, the local nature of the commercial real estate market dictates that the local (to be defined shortly) bank concentration index is the relevant measure.

For any given property, j, irrespective of the date of sale, we first calculate for each year, yr, the actual concentration measure of bank loans for its local area

$$BankHI_{j,yr}^{actual} = \sum_{b \in B^{j,yr}} \left(\frac{\#Bank_{j,yr,b}}{\sum_{b \in B^{j,yr}} \#Bank_{j,yr,b}} \right)^2, \tag{1}$$

where $\#Bank_{j,yr,b} = \sum_{i \in N^{(j,yr,b)}} 1$, $N^{(j,yr,b)}$ is the set of properties within 15 miles of property j (excluding the property itself) that received a loan from bank b during year yr, and $B^{j,yr}$ is the set of distinct banks that issued loans to a property within a 15-mile radius of property j in year yr.

We then compute a second measure of bank loan concentration, $BankHI_{j,yr}^{merge}$, using the merger activity data for large nonfailing banks. Specifically, we recompute equation (1) assuming that all bank mergers that took place during the course of year yr actually occurred at the beginning of the year. This creates a hypothetical local bank concentration measure that treats future merged banks as a single entity in their previous deals. That is, if two banks merge during the year, we examine all of their deals before the merger and code them as coming from the same bank in that year. For example, if banks A and B merge in June 1994, we code all deals financed by A or B from January to June as coming from the same bank. We thus measure the hypothetical impact on banking concentration based on loans made before the merger and assuming that other banks do not change their market shares after the merger. Our measure underestimates the effect of the merger since the activity of the merged bank from June

[7] Our variable is similar to those used in the literature. Berger et al. (1998) examine the effect of bank mergers and acquisitions on small business lending, using bank mergers as a proxy for bank competition. Sapienza (2002) also employs bank mergers on a sample of Italian banks and examines the impact on loan contracts. Other studies that examine the link between merger activity and banking competitiveness are Akhavein (1997), Berger and Hannan (1989), Prager and Hannan (1998), Houston and Ryngaert (1994), and Walraven (1997). For evidence that more concentrated banking markets are less competitive, see Berger et al. (1999), Hannan (1997), and Berger, Rosen, and Udell (2001).

to December is included in $BankHI_{j,yr}^{actual}$; therefore, we thus ignore the actual decrease in competition caused by the merger ex post.

The predicted change in banking competitiveness from year yr_1 to yr_2 caused by mergers, $\Delta BankHI_{yr_1:yr_2}$, is then defined simply as the sum of the individual year differences in Herfindahls, that is,

$$\Delta BankHI_{\,yr_1:yr_2,j} = \sum_{yr=yr_1}^{yr_2} \left(BankHI_{\,j,yr}^{\,merge} - BankHI_{\,j,yr}^{\,actual}\right). \tag{2}$$

Larger differences between $BankHI^{merge}$ and $BankHI^{actual}$ across properties (regions) indicate a more substantial potential impact of bank mergers on local commercial real estate loan competition. The range of years that we use in calculating $\Delta BankHI_{yr_1:yr_{2,j}}$ varies across our different tests. For example, since crime risks are available in 1990, 1995, and 2000, we compute $\Delta BankHI_{1992:1995,j}$ to determine the effect of bank mergers from 1992 to 1995 on the subsequent crime increase from 1995 to 2000. To determine the effect of mergers on the rates of loans used to finance individual sales, however, we calculate $\Delta BankHI_{1992:saleyear,j}$, where *saleyear* is the year in which the jth property transacted. We employ the same measure when examining the impact on property prices as well. Finally, since census data are only available for 1990 and 2000, we also compute $\Delta BankHI_{1992:1999,j}$ to examine the impact on census statistics such as income distribution.

Figure 1 reports summary statistics on $\Delta BankHI$ measures for various horizons. Focusing on the measure for the whole sample from 1992 to 1999, the mean and standard deviation of $\Delta BankHI_{1992:1999}$ are 0.0048 and 0.0068, respectively. Treating all banks equally, a mean plus one-standard deviation increase in $\Delta BankHI_{1992:1999}$ translates into a decrease in the number of banks in a local area due to mergers of large banks from an average of 17 to 14. Figure 1, which plots a histogram of $\Delta BankHI$ measures, indicates, however, that the majority of local banking markets experience little or no change in concentration, while a few experience substantial increases. The 90th percentile reduction in the number of banks is five, which is fairly substantial. The reduction is even more severe for the fraction of markets most affected by mergers. Such changes can have large marginal effects on loan competition in a local area. Our results are primarily driven by these few areas, and we will highlight the characteristics of these areas in the next section. Similar measures are reported in Figure 1 for $\Delta BankHI_{1992:1995}$ and $\Delta BankHI_{1992:saleyear}$, both of which generate smaller but still sizable changes in banking concentration, particularly in the most extreme areas.

It is worth noting that our study differs from much of the existing literature on U.S. banking competition, in that we make use of loan-based, rather than the more commonly adopted deposit-based, measures of concentration. In considering the terms of commercial real estate loans and the effects of those terms on real and social outcomes, a loan-based Herfindahl allows for the most

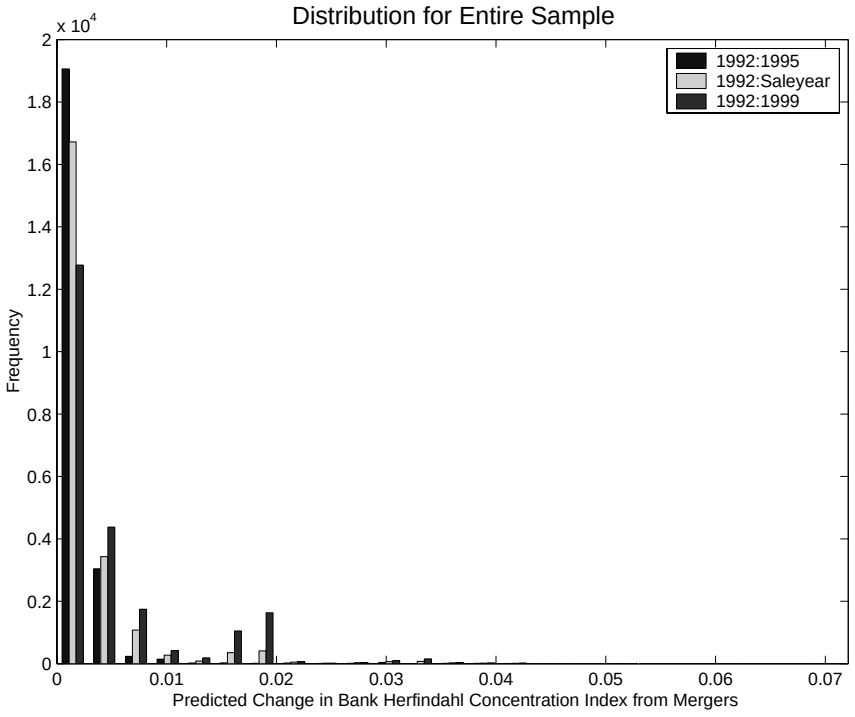

Figure 1. Distribution of predicted change in bank concentration from mergers of nonfailing commercial bank and bank holding companies with assets greater than $1 billion. The predicted change in banking competitiveness from year yr_1 to yr_2 caused by mergers, $\Delta BankHI_{yr_1:yr_2}$, is defined as the sum of the individual year increases in the Herfindahl concentration index, $\Delta BankHI_{\ yr_1:yr_2,j} = \sum_{yr=yr_1}^{yr_2}(BankHI_{j,yr}^{\ merge} - BankHI_{j,yr}^{\ actual})$, where $BankHI_{j,yr}^{\ actual}$ is the actual Herfindahl concentration index of commercial real estate bank loans within 15 miles of property j (excluding j itself) and $BankHI_{j,yr}^{\ merge}$ is the hypothetical concentration index assuming all nonfailing commercial bank mergers took place at the beginning of each year. The ranges of years used in calculating $\Delta BankHI_{yr_1:yr_{2,j}}$ are 1992 to 1995, 1992 to *saleyear*, where *saleyear* is the year in which the property transacted, and 1992 to 1999. The distribution and summary statistics for the entire sample and the subsample for which the predicted change in concentration is positive are reported.

direct analysis of the concentration of the local market for bank debt. While the overlap between local deposit-taking institutions and local loan-issuing institutions is substantial, it is not complete. Outside banks can be quite active in extending loans, and banks with sizable local deposits may invest in other areas. For example, the correlation between our loan-based concentration measure and a deposit-based concentration measure, calculated at the county level using 1995 data from the FDIC's summary of deposits, is 0.39 (significant at the 1% level). Our loan-based measure also allows us to consider within-county variation in banking market concentration.

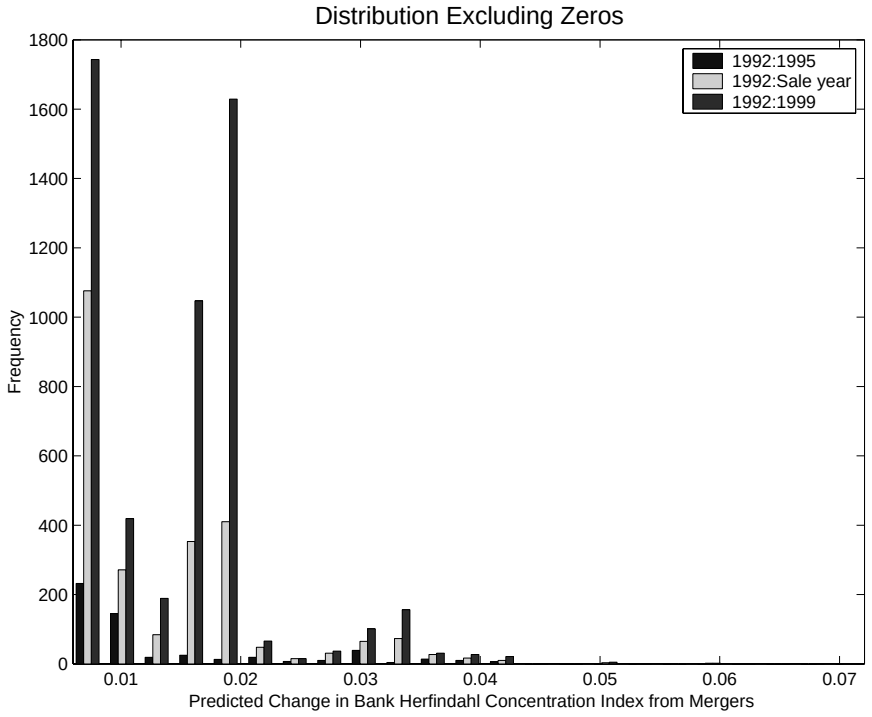

Figure 1.—*Continued*

	Summary Statistics				
	Mean	Std. Dev.	75th %	90th %	Max
$\Delta BankHI_{1992:1995}$	0.0010	0.0028	0.0011	0.0040	0.0417
those > 0	0.0031	0.0040	0.0040	0.0043	
$\Delta BankHI_{1992:saleyear}$	0.0025	0.0050	0.0030	0.0060	0.0721
those > 0	0.0051	0.0061	0.0052	0.0123	
$\Delta BankHI_{1992:1999}$	0.0048	0.0068	0.0055	0.0167	0.0721
those > 0	0.0073	0.0072	0.0088	0.0179	

B. What Is the Size of the Local Banking Market?

The standard definition of the local banking market in the literature is the local Metropolitan Statistical Area (MSA) or non-MSA county (e.g., Prager and Hannan (1998), Berger, Demsetz, and Strahan (1999), Rhoades (2000), and Black and Strahan (2002)). As described above, we define the local banking market as the area consisting of the 15-mile radius around a property, and in much of our empirical analysis we include county fixed effects. Our definition of the local banking market is finer than that usually employed and requires some explanation.

First, there is evidence from survey data for households and small businesses that banking markets are highly localized even within counties (see Kwast

et al. (1997) and Petersen and Rajan (2002)). Kwast et al. (1997), for example, show that the median and 75th percentile of the distances between small businesses and the financial institutions that provide their mortgage loans are 4 and 12 miles, respectively. This suggests that a definition of a local banking market that is narrower than the MSA-level or non-MSA-county-level is appropriate. Part of the rationale for the standard county definition of a local banking market relates to data availability. Our highly detailed data allow us to explore within-county variability in credit conditions. It is possible, however, to use a banking market definition that is too fine. For example, it is unreasonable to suggest that firms ignore banks located more than 5 miles away. The survey data and previous research in commercial real estate loan markets suggest that 15 miles is an appropriate choice of radius (Garmaise and Moskowitz (2004)).

Second, a principal advantage of our neighborhood-level variable is that we are able to include county fixed effects in our regressions. One might imagine many reasons for credit terms to evolve differently across counties (e.g., county or state-level regulatory changes), and our county fixed effects net such effects out. In addition, it is much less plausible that billion-dollar or larger bank mergers are driven by neighborhood-level changes in the economic environment within a county, giving us greater confidence in the exogeneity of our merger-induced concentration variable.[8]

Third, for comparability with the previous literature, in Section V we consider county- and state-level measures and find similar results. In addition, in unreported regressions, we find that our results are robust to using a local banking market radius of either 10, 20, or 25 miles.

III. Bank Mergers' Impact on Financial, Real, and Social Activity

In this section, we examine the impact on financial, real, and social activity of restricted credit access as a result of decreased bank competition from large mergers.

A. Loan Rates and Borrowing Activity

We begin by analyzing in Table II how the predicted change in the bank Herfindahl concentration measure generated through large nonfailing bank mergers is linked to actual future loan concentration, rates, terms, and size of the property-specific transactions in our sample.

In our first test, we show that bank mergers have a significantly positive effect on subsequent changes in banking market concentration. In the first column of Table II we display the results from regressing the actual change in the local banking market Herfindahl index from the first to the second half of our sample (from 1992–1995 to 1996–1999) on the merger-predicted

[8] It is interesting to note that our results are considerably strengthened when we discard the county controls.

Table II

The Impact of Bank Mergers on Competition, Loan Rates, and Borrowing Activity

The actual change in bank loan concentration, loan rate, loan frequency, and loan size are regressed on predictive measures of changing local bank competition through various sets of mergers, $\Delta BankHI_{yr_1:yr_2}$, which is the sum of the individual year differences between the actual Herfindahl concentration index of commercial loans within 15 miles of a property and the hypothetical concentration index assuming all bank mergers took place at the beginning of each year. *Saleyear* is the year in which the property sold. Loan frequency is the probability of obtaining a new loan, and loan size is the total loan-to-value ratio of the property from all sources (including banks and other financial institutions). All regressions include the current property crime risk (1995), recent change in property crime (from 1990–1995), local price variation, which is the cross-sectional variation of capitalization rates on properties within 15 miles, and recent growth in population, per capita income, and median home value for the census tract (between the 1990 and 2000 censuses). Loan interest rate regressions also include the loan-to-value ratio, an indicator for floating rates and for SBA-backed loans, and length of amortization and maturity. A constant and fixed effects for property type, year, and county are included in all regressions. Loan term regressions also include bank fixed effects. Coefficient estimates and their associated *t*-statistics (in parentheses) are reported with robust standard errors that assume groupwise clustering at the zip code level. Panel A employs mergers between all nonfailing banks and bank holding companies with at least $1 billion in assets. For the same set of mergers, Panel B employs a value-weighted concentration measure, Panel C examines only those deals in 1998 and 1999, and Panel D examines property deals worth at least $10 million. Panel E employs mergers between banks within the same bank holding company (BHC) and Panel F employs mergers between banks where at least one bank is failing, requiring assistance from the FDIC, RTC, NCUA, state, or other regulatory agency.

Panel A: Mergers between Large (assets > $1 billion) Banks and Bank Holding Companies

Dependent Variable	(1) Actual $\Delta BankHI$	(2) Loan Rate	(3) Loan Frequency	(4) Loan Size
$\Delta BankHI_{1992:1995}$	0.7802			
	(5.21)			
$\Delta BankHI_{1992:saleyear}$		32.9045	−2.0473	−2.2018
		(3.30)	(−1.60)	(−2.81)
Crime_{1995}	−0.0012	0.0004	0.0001	0.0001
	(−1.30)	(2.37)	(3.47)	(5.28)
$\%\Delta\text{Crime}_{1990:1995}$	0.0036	−0.0812	−0.0036	−0.0111
	(0.65)	(−0.45)	(−0.15)	(−0.61)
Price variation	−0.0015	0.2224	0.0303	0.0032
	(−1.52)	(1.96)	(1.97)	(0.41)
Population growth	−0.0001	0.0145	−0.0001	0.0001
	(−0.90)	(1.15)	(−0.07)	(0.26)
Income growth	0.0003	−0.0108	−0.0002	0.0001
	(12.88)	(−2.05)	(−0.61)	(0.23)
Home value growth	−0.0007	0.0158	−0.0021	0.0002
	(−2.80)	(0.75)	(−0.75)	(0.10)
Loan-to-value		0.1014		
		(0.56)		
Floating		−1.2284		
		(−15.97)		
Amortization		−0.0077		
		(−3.03)		

(continued)

Table II —*Continued*

Panel A: Mergers between Large (assets > $1 billion) Banks and Bank Holding Companies

Dependent Variable	(1) Actual $\Delta BankHI$	(2) Loan Rate	(3) Loan Frequency	(4) Loan Size
Maturity		−0.0186		
		(−5.72)		
SBA		0.7035		
		(1.44)		
Fixed effects:				
Year	No	Yes	Yes	Yes
Property type	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes
Bank	No	Yes	No	Yes
Adj. R^2	0.94	0.41	0.01	0.26
N	22,317	6,267	16,131	16,131
N cluster	1,147	915	1,162	1,162

Panel B: Value-Weighted ($) Concentration Measures

Dependent Variable	(1) Actual $\Delta BankHI$	(2) Loan Rate	(3) Loan Frequency	(4) Loan Size
$\Delta BankHI_{1992:1995}$	0.6206			
	(3.03)			
$\Delta BankHI_{1992:saleyear}$		31.8839	−1.9255	−2.0652
		(3.63)	(−1.69)	(−2.95)
Adj. R^2	0.91	0.41	0.01	0.26
N	22,317	6,267	16,131	16,131

Panel C: Only Deals from 1998 and 1999

Dependent Variable	(1) Actual $\Delta BankHI$	(2) Loan Rate	(3) Loan Frequency	(4) Loan Size
$\Delta BankHI_{1992:1994}$	−5.7718	27.4350	6.8218	1.0305
	(−2.54)	(0.54)	(1.21)	(0.33)
$\Delta BankHI_{1995:1997}$	2.8548	30.1367	−2.7793	−2.3124
	(2.45)	(2.75)	(−1.82)	(−2.48)
Adj. R^2	0.79	0.41	0.01	0.26
N	3,299	899	3,282	3,282

Panel D: Deals > $10 Million, Not Likely Affected by Local Competition

Dependent Variable	(1)	(2) Loan Rate	(3) Loan Frequency	(4) Loan Size
$\Delta BankHI_{1992:saleyear}$		−9.1930	−2.1470	−1.4040
		(−0.19)	(−0.43)	(−0.51)
Adj. R^2		0.26	0.01	0.24
N		94	471	471

(continued)

Table II —*Continued*

Panel E: Mergers within the Same BHC				
Dependent Variable	(1) Actual $\Delta BankHI$	(2) Loan Rate	(3) Loan Frequency	(4) Loan Size
$\Delta BankHI_{1992:1995}$	−0.1990			
	(−0.28)			
$\Delta BankHI_{1992:saleyear}$		5.7310	−0.6327	−0.1624
		(0.95)	(−0.28)	(−0.11)
Adj. R^2	0.79	0.40	0.01	0.26
N	22,633	6,267	16,131	16,131
Panel F: Mergers of Failing Banks				
Dependent Variable	(1) Actual $\Delta BankHI$	(2) Loan Rate	(3) Loan Frequency	(4) Loan Size
$\Delta BankHI_{1992:1995}$	6.8415			
	(1.82)			
$\Delta BankHI_{1992:saleyear}$		13.3604	185.0766	30.1756
		(1.33)	(1.25)	(0.37)
Adj. R^2	0.81	0.41	0.01	0.26
N	22,615	6,267	16,131	16,131

concentration measure, $\Delta BankHI_{1992:1995}$, and a set of control variables that include the 1995 property crime index risk, recent growth in crime (from 1990 to 1995), local price variation, defined as the cross-sectional variation of capitalization rates on all properties within 15 miles of the property (excluding the property itself), as well as growth in population, income, and median home value from 1990 to 2000 for the census tract in which the property is located. A constant and fixed effects for property type and county are also included. Regressions are run under ordinary least squares (OLS) with robust standard errors that assume groupwise clustering at the zip code level.[9] We find that $\Delta BankHI_{1992:1995}$ strongly predicts increased bank loan concentration over the subsequent 3-year period. This result indicates that bank mergers make the local market less competitive over the following 3 years, and justifies their use as an instrument for market concentration.

The effects of bank mergers on local loan market conditions are analyzed in columns two through four of Table II. We regress the interest rates on new loans on $\Delta BankHI_{1992:saleyear}$ and all the controls from the first regression. We also include a set of loan attributes as regressors, namely, the loan-to-value ratio,

[9] The number of clusters is reported at the bottom of the table along with the total number of observations. For Column 1 this means that although there are 22,317 data points, we treat the data as if there are only 1,147 independent observations, resulting in conservative standard errors.

indicator variables for floating or fixed rates, whether the loan is backed by the small business administration, the length of amortization, and loan maturity in years. In addition, we add year and bank fixed effects to the regression (for all banks, not just large banks) in order to measure deviations in loan rates from the average rate charged by a particular lender in a particular year. This controls for both unobservable quality differences or lending practices across banks as well as time effects. As the second column of Table II indicates, when bank mergers increase the concentration of the local banking market, loan rates increase. Thus, given the bank fixed effects, the same bank charges higher rates in less competitive areas than elsewhere.

To gauge economic significance, we consider the distribution of $\Delta BankHI_{1992:saleyear}$ over all data points for which $\Delta BankHI_{1992:saleyear}$ is positive, since the large fraction of the data for which there is no predicted change in concentration (e.g., $\Delta BankHI_{1992:saleyear} = 0$) is uninformative about the effects of mergers on loan terms. In addition, we consider only the conditional variation in $\Delta BankHI_{1992:saleyear}$ that remains once the variation explained by the fixed effects is removed. An increase from the 5th percentile to the 95th percentile in this residual variation is 0.0122, which translates into raising average loan rates at the margin by 40.1 basis points per annum, or, into a 5% increase over the mean loan rate of 8.1 percentage points. The magnitude of this price effect associated with an increase in market concentration is consistent with previous results in the literature (Hannan (1997) and Sapienza (2002)).[10] An increase of 40.1 basis points in loan rates likely discourages investment and may forestall projects. This suggests that the broader effects of mergers on local economic environments are likely to be most important in areas that experience the most extreme concentration increases.

Increases in bank concentration affect not only the price of credit, but its availability as well. In Column 3 we estimate the probability of obtaining debt (from either a bank or another financial institution) using a linear probability model with the same controls (including the fixed effects but excluding the loan terms) and standard errors clustered at the zip code level.[11] As Column 3 indicates, borrowers appear to receive financing less frequently when banking markets become less competitive, although this result is not statistically significant (t-statistic $= -1.60$). However, our data do not allow us to account for borrowers who are discouraged by credit conditions and deterred from seeking loans at all. Column 4 reports results from the regression of the magnitude of total debt (from bank and nonbank institutions) as

[10] The literature finds that mergers between small banks can benefit borrowers (Peek and Rosengren (1998), Strahan and Weston (1998), Sapienza (2002)), while Peek and Rosengren (1998) and Sapienza (2002) find evidence of market power effects when large banks are acquired. Since we focus on large bank mergers, our results appear to be in line with previous evidence.

[11] We have also employed a conditional estimator. Since the results from this model are qualitatively similar and generate the same conclusions for our paper, we only report the OLS estimation results for brevity.

a fraction of the purchase price on $\Delta BankHI_{1992:saleyear}$. The amount of debt significantly decreases when bank competition declines.[12] A 90th percentile increase in the conditional variation of concentration reduces the amount of debt by 2.5%.

A.1. Value-Weighted Concentration Measures

The concentration measure we analyze in Panel A is the one described in equation (1): a variable that equally weights all loan transactions. We adopt an equal-weighting approach because it is likely that small loans, which are typically serviced by local banks, will be most affected by merger-induced increases in local market concentration. Indeed, we provide evidence in support of this hypothesis below. As a robustness check, however, in Table II, Panel B, we report regressions of future banking market concentration and loan characteristics on a value-weighted merger-induced concentration measure. As Panel B shows, the value-weighted results essentially parallel the equal-weighted results, and provide additional evidence that large bank mergers have significant effects on future loan terms. Since results are similar using either measure, for brevity we report results for the equal-weighted measure throughout the paper.

A.2. Short-Term Distortions and Long-Term Equilibrium

Consistent with the results in Panels A and B, Sapienza (2002) finds that after mergers, banks engage in less small business lending. Berger et al. (1998, 1999), however, show that, in response, merged banks' competitors increase their small business lending so that the net effect on small business lending is unclear. We can reconcile these findings with our results by considering the timing of the sample transactions. We measure the effect of the change in competition on loan rates over the period 1992 to the sale year of the property. On average this is a 3.5-year period. We posit that a change in the regulatory or technological environment motivates two banks to merge, and that this merger then affects the competitive environment, resulting in higher loan rates and smaller loans being provided. We expect this short-term market distortion will provoke entry or loan supply increases by competing banks, but as Berger et al. (1998) note, these competitive effects require a minimum of 3 years to be realized. In the interim, the loan market performs inefficiently with a diminished level of financing — and this is the effect that we are capturing.

To test this idea in our sample, in Panel C, Column 1 of Table II, we regress the change in bank concentration from 1998 to 1999 on predicted changes in concentration both due to mergers 4–6 years ago ($\Delta BankHI_{1992:1994}$) and due to mergers in the most recent 3 years ($\Delta BankHI_{1995:1997}$). The 1998–1999 period is the only one for which a 6-year precursor period is available. All of the

[12] Since borrowers can substitute cash or seller financing for debt from a financial institution, some of the dependent variable's observations are zero. A truncated regression (Cragg (1971)) yields coefficient estimates similar to the OLS results reported.

previous control variables are also included in this regression. Merger-induced increases in banking concentration over the most recent prior 3 years lead to an increase in actual concentration: The coefficient on $\Delta BankHI_{1995:1997}$ is significantly positive. Increases from the more distant 3-year period, however, lead to decreases in actual concentration: The coefficient on $\Delta BankHI_{1992:1994}$ is significantly negative. These results provide strong evidence that recent mergers lead to short-term decreases in competition, but over the longer term, new entry and loan supply increases lead to a restored competitive equilibrium, consistent with results in the literature.

Columns 2 through 4 of Panel C provide compelling evidence from the loan markets that bank mergers cause a short-term disruption to the competitive landscape. We find that increases in $\Delta BankHI_{1995:1997}$ significantly increase interest rates, decrease loan frequency, and decrease loan size, but the effects of $\Delta BankHI_{1992:1994}$ are uniformly insignificant and of the opposite sign in the frequency and size regressions. Mergers in the last 3 years seem to worsen loan terms, but mergers from the more distant 3-year period appear to have no residual effect, consistent with a short-term competitive distortion.

A.3. Robustness: Deals and Mergers That Should Have No Competitive Effects

To further support our interpretation of these results, we examine a set of deals that should be unaffected by local competition and a set of mergers that should have little competitive impact and show that these do not affect actual loan concentration or credit conditions.

Panel D of Table II provides results on the effect of bank mergers on loan terms for the very largest deals, with property values of at least $10 million. Borrowers in the largest deals typically have access to a broader set of national banks and are thus less likely to be affected by changes in the competitiveness of the local banking market. Consistent with this, Panel D shows that an increase in local bank market concentration has no effect on loan rates, loan frequency, or loan size for the largest deals.

In Panel E of Table II, we consider the effects of a merger between two banks owned by the same BHC. We hypothesize that such a merger is essentially an internal corporate reorganization and therefore it should not have a competitive effect. Indeed, this is our motivation for excluding such mergers from our main competition measure. Calculating a new $\Delta BankHI$ measure using only within-BHC mergers, we find that such mergers have no effect on subsequent actual concentration, interest rates, loan frequency, or loan size, as predicted.

Finally, Panel F of Table II analyzes the effects of mergers of failing banks. We exclude such mergers from our main $\Delta BankHI$ measures both because of greater endogeneity concerns with regard to declining economic conditions and because the disappearance of a failing bank likely has only a small effect on competition. While we do find that mergers of failing banks lead to slightly increased concentration, such mergers have negligible effects on loan terms, consistent with failing banks having little competitive effect. This result also mitigates endogeneity concerns.

B. Real Activity and the Local Economy

Next we analyze the potential real effects of the substantial decrease in the provision of bank debt associated with large mergers in the local economy.

B.1. Investment and Development

The first two columns of Table III, Panel A consider two proxies for the level of development in the 15-mile radius surrounding each property. In the first column, we consider the change in the mean age of properties. In the second column, we consider the change in the fraction of properties less than 3 years old in this region (we exclude the property itself). The change in average age of properties sold in a local area indicates how much recent development has taken place in the area, since recently developed properties are young by definition. The fraction of properties less than 3 years old compares the level of new development from 1995 to 1998 to the level of development from 1992 to 1995. We regress these development variables on $\Delta BankHI_{1992:1995}$ and the set of control variables.

As the table indicates, the average age of properties rises significantly and the fraction of new/developed properties declines significantly when local banking markets become less competitive. A 90th percentile increase in the conditional variation of $\Delta BankHI_{1992:1995}$ raises the mean age by 1.01 years and reduces the fraction of new properties by 0.125%. Since the mean age of properties is 35.55 years and 10% of the properties are 3 years old or younger, this increases the mean age by 2.9% and decreases the fraction of new properties by 1.2%. These results linking investment to well-functioning credit markets are consistent with the cross-country work of King and Levine (1993), Rajan and Zingales (1998), and Demirguc-Kunt and Maksimovic (1998) and the within-country work of Guiso et al. (2004).

B.2. Property Prices

Neighborhoods with less new development will age and become less attractive over time as properties depreciate. This, in turn, may impact local real estate market prices and rents.

Column 3 of Panel A reports regression results of property prices on $\Delta BankHI_{1992:saleyear}$. The dependent variable is the property capitalization rate (net operating income divided by price). The measure $\Delta BankHI_{1992:saleyear}$ is appropriate for this test since we consider property prices reported in the year of sale. We include the controls from previous regressions.

The regression demonstrates that cap rates increase significantly, or prices per unit of income decrease significantly, when the banking market becomes more concentrated. A 90th percentile increase in the conditional variation of $\Delta BankHI_{1992:saleyear}$ generates a 0.10 percentage point increase in cap rates, which implies a 1% decrease in property prices (the mean cap rate of 10.09% is given in Table I). Given that the mean property value in our sample is \$2,387,000, this translates roughly into a \$23,870 decline.

Table III
The Impact of Bank Competition (through Mergers) on the Local Economy

Panel A reports regression results of real investment proxies and property cap rates on predicted changes in local bank competition measured through mergers. Panel B reports regression results of percentage changes from 1990 to 2000 in census-tract-level median income, income migration (the ratio of income of households that moved into the tract within the last 5 years to the income of households that have resided in the tract at least 5 years), unemployment, income dispersion (defined as the standard deviation of the fraction of households in the eight census income categories), and rental and vacancy rates on predicted changes in local bank competition measured through mergers. Predicted changes in local bank competition are measured as described in Figure 1 and Table II. All regressions include a constant, current property crime risk (1995), recent growth in property crime (from 1990 to 1995), local price variation, recent growth in population, income, and home value for the census tract, and property type and county fixed effects, which are omitted from the table for brevity. The cap rate regression also includes year fixed effects. Coefficient estimates and their associated *t*-statistics (in parentheses) are reported with robust standard errors that assume groupwise clustering at the zip code level for cap rate and age regressions and at the census tract level for the census statistics regressions.

Panel A: Investment, Development, and Prices

Dependent Variable	(1) ΔMean Age	(2) Δ%Age< 3 Years	(3) Cap Rate (E/P)
$\Delta BankHI_{1992:saleyear}$			8.2347
			(3.73)
$\Delta BankHI_{1992:1995}$	268.1843	−0.3286	
	(3.67)	(−3.41)	
Fixed effects			
Year	No	No	Yes
Property type	Yes	Yes	Yes
County	Yes	Yes	Yes
Adj. R^2	0.84	0.76	0.24
N	15,734	15,734	10,070
N cluster	873	873	978

Panel B: Income Distribution, Unemployment, and Vacancy

Dependent variable	(1) %ΔMedian Income	(2) Income Migration	(3) ΔUnemployment	(4) ΔIncome Dispersion	(5) ΔVacancy and Rental
$\Delta BankHI_{1992:1995}$		−7.3931			
		(−3.06)			
$\Delta BankHI_{1992:1999}$	−5.5486		10.3268	561.9380	41.7708
	(−2.58)		(3.52)	(2.99)	(9.79)
Fixed effects					
Property type	Yes	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.22	0.11	0.25	0.02	0.43
N	16,910	9,343	16,154	17,763	16,900
N cluster	3,060	1,360	2,938	3,157	3,057

B.3. Income Distribution, Unemployment, and Homeownership

Next we consider the effects on the income distribution of the neighborhood, making use of data from the 1990 and 2000 censuses. While the results in Table II and Table III, Panel A illustrate the effects of mergers on subsequent loan contracts, development, and prices, so any changes in these latter variables are interpreted as being caused by mergers, for the census data this is not possible; given the time frame of our data we can only measure contemporaneous changes in the merger and census variables. (One exception is the in-migration income test described below.) Therefore a measure of caution is warranted in attributing causality to the following results, though we think these findings are nonetheless interesting and supportive of our central hypothesis. We again defer a detailed examination of the general question of causality to Section IV.

In the first column of Table III, Panel B we display results from regressing the percentage change (from the 1990 to 2000 censuses) of median household income on $\Delta BankHI_{1992:1999}$. We find a highly significant negative relationship: A 90th percentile increase in conditional bank concentration reduces median income by about 3.9%. In the second column of Panel B we show that this income change is driven by the migration of poorer households into neighborhoods experiencing increases in bank concentration. We regress the ratio of the income of households that moved into the tract in the last 5 years to the income of households that have resided in the tract for at least 5 years on $\Delta BankHI_{1992:1995}$. We find that neighborhoods with an increase in bank concentration over 1992 to 1995 experience an inflow of relatively poorer residents in the period 1995 to 2000.

Consistent with this evidence, we also show in the third column of Table III, Panel B that unemployment in a property's census tract rises with an increase in bank concentration. In Column 4, we further show that income dispersion, defined as the standard deviation of the fraction of households in the eight census income categories, ranging from less than $15,000 to more than $150,000 per year, in the property's census tract increases significantly with bank concentration, despite the fact that median income drops. The final column of Panel B considers the change in total vacancy and rental rates from 1990 to 2000 in the census tract in which the property resides. The positive coefficient indicates that home ownership rates decline as bank concentration increases.

C. Crime

There is substantial evidence in the economics of crime literature of a positive relationship between unemployment and crime (e.g., Freeman (1999) and Freeman and Rodgers (1999)), though the association is not overwhelmingly strong (e.g., Cullen and Levitt (1999)). Grogger (1998) and Gould, Weinberg, and Mustard (2002) present clear evidence linking low legal wage opportunities to increased crime. Kelly (2000) and Fajnzylber, Lederman, and Loayza

(2002) document that crime increases with economic inequality. The results in Table III, in particular, that increases in bank concentration lead to less development and lower prices and are associated with an influx of poorer households and rising income inequality, suggest a potential causal connection between bank merger-induced concentration and increased crime. Moreover, given potential social interaction multiplier effects of the type suggested by Glaeser, Sacerdote, and Scheinkman (1996, 2002), the effect on crime could be significant. It is also a general finding of the crime literature that unemployment, income, and income dispersion are strongly related to property crime, while the association between economic variables and personal (violent) crime is much weaker and perhaps nonexistent (Levitt (2001), Raphael and Winter-Ebmer (2001), and Nilsson (2004)). Therefore, we examine how property crimes and personal/violent crimes each change when banking markets become less competitive through large nonfailing bank mergers. We do not expect to find much of an effect on the latter according to our proposed mechanism.

The dependent variable in the first column of Table IV, Panel A is the percentage change in burglary risk from 1995 to 2000 for the eight-digit latitude and longitude location of the property. The dependent variable in Column 2 is the analogous change in general property crime. In all regressions in this table we include the usual control variables. Since the change in banking competitiveness is measured using only past transaction data, it is not contaminated by simultaneity effects and is therefore less likely to exhibit endogenous correlation with the dependent variable, future crime risk. Since crime risks are likely to be serially correlated, however, we also include both the current level of crime (in 1995) and the most recent change in crime (from 1990 to 1995) as additional controls in the regression.

The first two columns of Table IV, Panel A show that $\Delta BankHI_{1992:1995}$ significantly increases burglary and general property crime risk from 1995 to 2000. Hence, when large nonfailing bank mergers cause the local banking market to consolidate and become less competitive, there is a subsequent rise in burglaries and general property crimes over the next several years.

Columns 3 and 4 of Panel A employ homicide and personal crime risk as dependent variables. As indicated in Table IV, $\Delta BankHI_{1992:1995}$ has an insignificant effect on both homicide and personal crime risks. This is consistent with the evidence from the crime literature described above, specifically, that there is little connection between economic variables and personal crime. Note, too, that the other local economic variables used as controls also have little effect on personal crime. The fact that mergers increase property crime risks while having no effect on personal crime risks is consistent with the idea that the mechanism through which mergers increase crime is through a negative effect on the overall economic environment, highlighted in Tables II and III.

For robustness, the fifth and sixth columns of Panel A include the level of bank concentration over the period 1992 to 1995 as an additional variable. The results are essentially unchanged by the inclusion of this control.

To gauge the magnitude of the effect of bank mergers on crime and to assess how it compares to documented economic variable-crime relationships

Table IV

The Impact of Bank Competition (Through Mergers) on Crime

The percentage change in future crime risk from 1995 to 2000 for the eight-digit latitude and longitude location in which each property resides is regressed on the predicted change in local bank competition induced by mergers from 1992 to 1995, $\Delta BankHI_{1992:1995}$, as defined in Figure 1 and Table II. Crime risk variables come from Cap Index, Inc. and pertain to burglary, a property crime index that includes burglary, larceny, and motor vehicle theft, homicide, and a crime index for personal crimes that includes homicide, rape, aggravated assault, and robbery. Panel A reports reduced-form regression results of crime risk changes directly on the merger-predicted concentration variable. Panel B reports elasticity estimates by regressing the percentage change in future crime risk on the percentage change in *BankHI* induced by mergers. Panel C reports results from regressions that include interaction terms between $\Delta BankHI_{1992:1995}$ and characteristics of the local market in which the property resides: the actual level of bank concentration from 1992 to 1995, the dispersion in income within the census tract from the 1990 census, the level of crime in 1995, and the median income in the census tract in 1990. All regressions include current crime risk (1995), recent growth in crime (from 1990 to 1995), local price variation, and recent growth in population, income, and home value for the census tract. A constant and property type and county fixed effects are also included. Coefficient estimates and their associated *t*-statistics (in parentheses) are reported with robust standard errors that assume groupwise clustering at the zip code level.

Panel A: Dependent Variable = Future Change in Crime Risk $\%\Delta\text{Crime}_{1995:2000}$

	(1) Burglary	(2) Property	(3) Homicide	(4) Personal	(5) Burglary	(6) Property
$\Delta BankHI_{1992:1995}$	0.3741	0.4377	0.3131	−0.0196	0.3720	0.4404
	(3.23)	(2.95)	(1.12)	(−0.12)	(3.17)	(2.95)
$BankHI^{actual\,level}_{1992-1995}$					0.0034	−0.0045
					(0.19)	(−0.17)
Crime_{1995}	−0.0001	−0.0001	−0.0001	0.0001	−0.0001	−0.0001
	(−31.88)	(−8.24)	(−2.91)	(5.03)	(−31.45)	(−8.28)
$\%\Delta\text{Crime}_{1990:1995}$	0.2309	0.2586	0.0653	0.1864	0.2309	0.2586
	(24.18)	(18.90)	(7.45)	(13.74)	(24.14)	(18.90)
Price variation	−0.0011	−0.0044	0.0036	0.0023	−0.0011	−0.0044
	(−2.44)	(−2.37)	(1.00)	(1.52)	(−2.49)	(−2.37)
Population growth	−0.0001	0.0001	0.0002	0.0002	−0.0001	0.0001
	(−1.24)	(0.73)	(1.37)	(1.49)	(−1.24)	(0.73)
Income growth	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001
	(4.15)	(1.50)	(1.44)	(1.27)	(4.15)	(1.50)
Home value growth	−0.0009	−0.0021	−0.0015	0.0001	−0.0009	−0.0021
	(−5.17)	(−4.79)	(−2.73)	(0.89)	(−5.17)	(−4.79)
Fixed effects						
Property type	Yes	Yes	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.87	0.86	0.75	0.89	0.87	0.86
N	22,317	22,317	22,317	22,317	22,317	22,317
N cluster	1,146	1,146	1,146	1,146	1,146	1,146

Panel B: Elasticity Estimates from Percentage Changes on Percentage Changes

	(1) Burglary	(2) Property
$\%\Delta BankHI_{1992:1995}$	0.1604	0.1842
	(3.82)	(4.36)
Fixed effects		
Property type	Yes	Yes
County	Yes	Yes
Adj. R^2	0.88	0.86
N	22,317	22,317
N cluster	1,146	1,146

Panel C: Interactions with Local Market Characteristics

	Burglary					Property				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\Delta BankHI_{1992:1995}$	1.3403	−0.3715	−1.1934	1.0090	0.9838	2.1244	0.0040	0.0185	2.1587	4.0530
	(2.98)	(−0.60)	(−3.05)	(3.07)	(1.62)	(3.23)	(0.01)	(0.07)	(3.99)	(3.87)
$\times BankHI^{actual\ level}_{1992-1995}$	0.0010				0.0031	0.0010				0.0054
	(1.82)				(1.83)	(1.96)				(2.26)
×Income dispersion		21.0024			9.7015		15.5402			−4.0742
in 1990		(2.16)			(0.57)		(2.52)			(−0.38)
×Crime level			0.0095		0.0005			0.0030		0.0011
in 1995			(4.54)		(0.28)			(2.01)		(1.00)
×Median income				−0.0001	−0.0001				−0.0001	−0.0001
in 1990				(−2.36)	(−1.92)				(−3.52)	(−3.47)
Fixed effects										
Property type	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.89	0.88	0.88	0.88	0.88	0.86	0.86	0.86	0.86	0.86
N	22,317	22,317	22,317	22,317	22,317	22,317	22,317	22,317	22,317	22,317
N cluster	1,146	1,146	1,146	1,146	1,146	1,146	1,146	1,146	1,146	1,146

in the literature, we compute the elasticity of crime to merger-induced bank concentration by regressing the percentage change in future crime on the percentage change in $\Delta BankHI_{1992:1995}$, defined as

$$\%\Delta BankHI_{1992:1995,j} = \frac{\sum_{yr=1992}^{1995} BankHI_{j,yr}^{merge}}{\sum_{yr=1992}^{1995} BankHI_{j,yr}^{actual}}. \quad (3)$$

The results are reported in the two columns of Table IV, Panel B. The elasticities of burglary and property crime with respect to bank concentration are 0.16 and 0.18, respectively. A 90th percentile increase in $\%\Delta BankHI_{1992:1995}$ (conditional on the fixed effects) raises the burglary risk by 0.56% and the property crime risk by 0.63%. The magnitudes of the crime effects that we document are in line with those in the literature. Grogger (1998), for instance, estimates an elasticity of property crime to wages of 0.6 to 0.9. Rickman and Witt (2003) find an elasticity of theft to unemployment of approximately 0.75. Given that mergers affect crime indirectly, through an economic mechanism, we expect the crime-bank concentration elasticity to be lower than that for direct economic variables such as unemployment and wages.

C.1. Interactions with Local Market Characteristics

It is interesting to investigate the types of neighborhoods most affected by credit supply shocks induced by mergers, particularly given the skewed distribution of these effects that is evident in Figure 1. In Table IV, Panel C, we consider differences across neighborhoods in the effect of a merger-induced increase in bank concentration on burglary and property crime. A priori, it seems reasonable that the most disadvantaged neighborhoods are most affected by a given increase in bank concentration since the provision of finance in these neighborhoods may already be restricted. We regress the change in burglary and property crime risks from 1995 to 2000 on $\Delta BankHI_{1992:1995}$ and $\Delta BankHI_{1992:1995}$ interacted with other local market variables. In the first and sixth columns of the table we show that an increase in $\Delta BankHI_{1992:1995}$ has the greatest effect in increasing crime in neighborhoods that previously had higher bank concentrations. Columns 2, 3, 7, and 8 show that an increase in bank concentration has a significantly greater effect on crime in neighborhoods with greater income dispersion and higher crime levels in 1990. The regressions described in Columns 4 and 9 show that the effect of mergers on crime is significantly greater in areas with lower 1990 median incomes as well. In Columns 5 and 10, we report results from regressions that include all the interactions. Interactions of our merger variable with previous bank concentration and median income remain significant in these regressions.

These results indicate that more fragile neighborhoods already experiencing concentrated loan markets and low median income are more affected by an

increase in bank concentration arising from mergers. Given the skewed distribution across neighborhoods of the effect of mergers on loan competition, it appears that our results are driven by a few extreme areas, already in a fragile state, with high initial loan concentration, where a large bank merger may direct the neighborhood into further deterioration. Clearly, mergers per se are not uniformly bad, but in delicate and marginal areas they seem to have significant competitive effects that generate economic and social consequences. Furthermore, given the potential social multiplier effects suggested in the literature, it may not take much to send a marginal neighborhood spiraling downward, though we provide no further evidence of such multiplier effects.

C.2. The Broader Implications of Bank Merger-Induced Crime

To put our crime results into perspective, consider the total number of property crimes committed each year in the United States. The FBI documents that the average number of property offenses was 11.1 million per year from 1995 to 2000, our sample period for crime changes. Assuming that the mean change in banking concentration through mergers that we observe in our data set from 1992 to 1995 (0.1%) applies nationally, this translates into roughly an additional 24,300 property crime offenses from 1995 to 2000 due solely to competitive effects from large bank mergers. Although this is a small fraction of the total property crimes, such increases clearly have significant utility and social consequences (see Becker (1968) and Ehrlich (1973)).

IV. Exogeneity of Competition Measure

We have argued that the use of large nonfailing bank mergers from the past provides an exogenous measure of future competition changes in local areas. We now provide further evidence on the exogeneity of the bank merger variable, particularly with regard to crime.

A. Does (Whatever Drives) Future Crime Spur Present-Day Mergers?

The results in Tables II, III, and IV point to a link between bank mergers, loan terms, real activity, and crime. We have been careful to link bank merger activity to subsequent increases in loan terms, economic activity, and crime. One interpretation of this link is a causal one, according to which mergers reduce financial access, which in turn leads to neighborhood declines and subsequent increases in crime. The leading alternative explanation for these correlations, however, is reverse causation, whereby unobservable variables that affect future crime drive present-day mergers. For example, local economic conditions that affect future crime risks may motivate bank mergers today. One version of the reverse causation theory is that banks in declining neighborhoods may anticipate further deterioration in their future lending positions. Concerned about their viability or falling below minimum efficient scale, the banks may seek merger opportunities. Another version posits that

banks in declining neighborhoods may understand that the equilibrium level of competition in such neighborhoods is likely to decline in the future as the profitability of the neighborhoods declines. Banks may therefore merge today to reduce future competition in these neighborhoods.

A.1. Arguments against Reverse Causality

We believe that the reverse causality interpretation of our results is less plausible for several reasons. First, commercial real estate loans typically comprise a small portion of a bank's balance sheet. For example, the FDIC's *History of the Eighties—Lessons for the Future* (p. 159) documents that among nonfailing banks, commercial real estate loans were only 11% of assets in 1993. Over the entire 1992–1999 period, the Federal Reserve estimates that total real estate loans (commercial and residential) comprised less than 13% of assets. Since we only consider nonfailing mergers, and since commercial real estate loans are a small fraction of bank assets, it is unlikely that changes in commercial property values or commercial real estate lending drive the mergers we study. Moreover, in Table III we show that even an increase in banking concentration at the 90th percentile produces only a 1% decrease in property values. The effect on the value of bank loans is even smaller. For these reasons it is unlikely that banks merge in anticipation of future decreases in property values and increases in crime.

Second, we include county fixed effects in our analysis. It seems unlikely that large bank mergers are motivated by neighborhood-level variation within counties in current or anticipated future crime risks or economic declines. This is especially true of the particular mergers we analyze, whose size (at least $1 billion in assets) and scope (operating in many neighborhoods) implies that the average neighborhood effect for these banks (controlling for county effects) is likely to be quite small, by the law of large numbers. Therefore, cross-neighborhood variation in real estate prices and crime is very unlikely to motivate the mergers of these large banks.

Third, in the next section we employ another measure of bank competition, using state branching deregulation as an instrument rather than mergers, and find very similar effects. It seems unlikely that both instruments would be contaminated by the same endogeneity concerns, lending strong support to the hypothesis that these variables capture exogenous competition changes.

A.2. Testing Reverse Causality Directly

To provide clear evidence for or against the reverse causation theory, we test several of its implications in Table V. First, if trending crime risks cause mergers, future crime would have to be a reflection of the current crime trend; in essence, current crime would spur contemporaneous bank mergers. We note that the recent change in crime (from 1990 to 1995) is positively correlated with future changes in crime (from 1995 to 2000). Hence, under this alternative hypothesis we should see a significant positive contemporaneous correlation between mergers and crime in the period 1990 to 1995. As the first two columns

Table V

Does (Whatever Drives) Future Crime Spur Current Mergers?

The first two columns of Panel A report results from regressions of the percentage change in the current crime risk from 1990 to 1995 on the contemporaneous bank merger variable for local bank concentration. Columns 3 and 4 report regression results of the actual future change in banking concentration from 1996 to 1999 on recent trends in crime risk, median income, and home value for the census tract. The first two (last two) columns of Panel B report regression results of the change in banking concentration due to mergers from 1996 to 1999 (probability of a merger) on recent trends in crime risk, median income, and home value. The first two (last two) columns of Panel C report results from regressions of future changes in crime from 1995 to 2000 on the predicted change in bank concentration due to mergers involving a failing bank (changes in the number of nonlending financial institutions from 1990 to 1995). Nonlending financial institutions are obtained from the County Business Patterns of the Bureau of Economic Analysis and are defined as the number of establishments with NAICS codes of 523, 524, and 525. All regressions include a constant and the control variables from Table IV. Coefficient estimates and their associated *t*-statistics (in parentheses) are reported with robust standard errors that assume groupwise clustering at the zip code level for all regressions except those for nonlending financial institutions, that assume groupwise clustering at the county level.

Panel A	Are Crime and Merger-Induced Bank Consolidation Contemporaneously Related?		Does Crime or Economic Activity Predict Future Bank Consolidation?	
	$\%\Delta$ Crime$_{1990:1995}$		$\Delta BankHI^{actual}{}_{1996:1999}$	
Dependent Variable	(1) Burglary	(2) Property	(3) Burglary	(4) Property
$\Delta BankHI_{1992:1995}$	0.0398	−0.0432		
	(0.20)	(−0.37)		
$\%\Delta$ Crime$_{1990:1995}$			−0.0061	−0.0346
			(−0.35)	(−1.54)
Income growth			0.0005	0.0003
			(0.47)	(0.24)
Home value growth			−0.0006	−0.0006
			(−1.65)	(−1.92)
County fixed effects	Yes	Yes	Yes	Yes
N	22,317	22,317	22,317	22,317
N cluster	1,146	1,146	1,146	1,146
Panel B	Does Crime or Economic Activity Predict Bank Consolidation Due to Mergers?		Does Crime or Economic Activity Predict Merger Activity?	
	$\Delta BankHI_{1996:1999}$		Probability of a Merger	
Dependent Variable	(1) Burglary	(2) Property	(3) Burglary	(4) Property
$\%\Delta$ Crime$_{1990:1995}$	−0.0008	−0.0005	−0.0935	−0.0897
	(−1.54)	(−0.76)	(−2.59)	(−2.08)
Income growth	0.0001	0.0001	0.0058	0.0064
	(0.98)	(1.09)	(0.65)	(0.72)
Home value growth	0.0001	0.0001	−0.0027	−0.0025
	(1.20)	(1.24)	(−0.74)	(−0.70)
County fixed effects	Yes	Yes	Yes	Yes
N	22,317	22,317	22,317	22,317
N cluster	1,146	1,146	1,146	1,146

(continued)

Table V —*Continued*

Panel C	Do Mergers of Failing Banks Predict Future Crime Risk?		Do Changes in Numbers of Nonlending Financial Institutions Predict Crime Risk?	
	$\%\Delta$ Crime$_{1995:2000}$		$\%\Delta$ Crime$_{1995:2000}$	
Dependent Variable	(1) Burglary	(2) Property	(3) Burglary	(4) Property
$\Delta BankHI^{failing}{}_{1992:1995}$	0.2750	−0.3223		
	(0.98)	(−1.87)		
Δ Nonlending$_{1990:1995}$			−0.0006	−0.0186
			(−0.24)	(−1.51)
County fixed effects	Yes	Yes	No	No
N	22,317	22,317	22,317	22,317
N cluster	1,146	1,146	53	53

of Table V, Panel A indicate, however, we find no such relation in the data. The contemporaneous correlations between burglaries or property crimes and our merger variable are statistically no different from zero.

We also examine in Columns 3 and 4 of Table V, Panel A whether recent changes in crime or economic activity, income and home value growth, predict actual future bank concentration, capturing the effects of both mergers and market consolidation occurring for other reasons. Burglary, property crimes, and income growth do not predict future bank concentration, but home value growth at the census tract level predicts reduced future bank concentration with marginal statistical significance. This is consistent with a version of the reverse causation hypothesis.

In our analysis, however, we are careful not to use the actual future change in bank concentration, but rather the concentration increase induced by previous mergers—it is precisely because of our concern that future bank concentration may be endogenously related to crime that we use previous large mergers as an instrument. In Columns 1 and 2 of Table V, Panel B, we regress the measure of future merger-induced concentration $\Delta BankHI_{1996:1999}$, as opposed to actual concentration, on past crime, income growth, and home value growth. None of these variables predicts an increase in merger-generated concentration. These results stress the importance of using an instrument for bank competition related to large nonfailing bank mergers and undermine the reverse causation theory.

In Columns 3 and 4 of Table V, Panel B we directly address whether crime predicts merger activity by regressing the probability of a merger of any bank active in a property's neighborhood (linear probability model) on the change in crime, income growth, and home value growth. Crime risks predict significantly fewer mergers, while the economic variables are insignificant. This suggests that areas suffering from crime increases are less likely to experience a merger, which is contrary to the reverse causation hypothesis. Thus, there is no evidence that large bank mergers take place due to neighborhood declines.

B. Failing Banks and Nonlending Institutions

Although our merger variable is constructed only from mergers between large nonfailing commercial banks, in Columns 1 and 2 of Table V, Panel C we consider exclusively the effects of mergers that involve failing banks. If bank mergers increase subsequent crime rates due to a competitive effect, then these assisted mergers should have little impact on future crime. Failing banks are not likely offering much competition for other banks before the merger, so their removal from the market should not have a significant effect. On the other hand, if the association between bank mergers and future crime arises from mergers of weak banks in declining neighborhoods (reverse causality), then the effect should be strongest for these assisted mergers since failing banks are more likely to be active in declining neighborhoods. Our data contain an additional 18 failing banks that merged in our sample. We recompute the bank merger concentration measure using these banks only and regress future crime risk changes on it. We find an insignificant coefficient for burglary, and a marginally significant but negative coefficient for property crimes, contradicting the reverse causality theory. These results are consistent with those in Table II showing that failing bank mergers have little competitive effects on loan markets, and thereby lend further support for a causal link between nonfailing bank mergers and future crime.

For further robustness, Columns 3 and 4 report results from regressing the 1995 to 2000 changes in crime risks on changes in the number of nonlending financial institutions from 1990 to 1995, obtained from the *County Business Patterns* of the Bureau of Economic Analysis.[13] Under reverse causation, economic decline should cause both a decrease in the current number of nonlending financial institutions and a future increase in crime, in turn leading to a spurious negative correlation between the two. If, on the other hand, our conjecture that the merger of banks causes an increase in crime through poorer credit conditions, then a decrease in nonlending financial institutions should not have any effect on future crime, since these institutions themselves do not affect loan supply. As the last two columns of Table V indicate, there is no evidence of an association between changes in the number of nonlending financial institutions and future crime risks. This does not support the reverse causation theory, but is consistent with our story of credit access, development, and social spillovers.

C. Separating the Merger Effect from the Competition Effect

As an additional test of the exogeneity of our measure, we separate the effect of the merger per se from the effect of reduced competition caused by mergers. Specifically, we add a dummy variable equal to one if a merger took place in the local banking market to our regression model. Panel A of Table VI reports

[13] Nonlending financial institutions are defined as establishments with North American Industry Classification System (NAICS) codes of 523 (securities, commodity contracts, and other financial investments), 524 (insurance carriers and related activities), and 525 (funds, trusts, and other financial vehicles), obtained at the county level.

Table VI

Separating the Merger Effect from the Competition Effect

Panel A reports results from regressions of the actual change in bank loan concentration, loan rate, loan size, and future change in crime risk on the predicted change in local banking competitiveness measured through mergers between nonfailing banks and bank holding companies with at least \$1 billion in assets, $\Delta BankHI_{yr_1:yr_2}$, and a dummy variable for the presence of a merger. The former variable captures the predicted competitive effects of mergers and the latter captures the effect of the merger per se, irrespective of its competitive effects. Panel B repeats the regressions on the merger dummy variable alone for only those properties residing in an area in which mergers have no predicted effect on competition (i.e., where the continuous measure $\Delta BankHI_{yr_1:yr_2} = 0$). All regressions include a constant and all of the control variables from Tables II and IV, including property type and county fixed effects as well as year and bank fixed effects for the loan term regressions. Coefficient estimates and their associated *t*-statistics (in parentheses) are reported with robust standard errors that assume groupwise clustering at the zip code level.

	Changes in Future Crime			
			$\%\Delta\text{Crime}_{1995:2000}$	
Dependent Variable	Loan Rate (1)	Loan Size (2)	Burglary (3)	Property (4)
Panel A: All Property Transactions				
$\Delta BankHI_{1992:1995}$			0.3825	0.3991
			(2.29)	(2.49)
$\Delta BankHI_{1992:saleyear}$	22.2663	−1.3444		
	(2.76)	(−1.76)		
Merger dummy	−0.0588	0.0168	−0.0003	0.0013
	(−1.12)	(2.82)	(−0.27)	(1.38)
Fixed effects				
Year	Yes	Yes	No	No
Property type	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes
Bank	Yes	Yes	No	No
Adj. R^2	.039	0.25	0.87	0.86
N	6,267	11,809	22,317	22,317
N cluster	915	1,100	1,146	1,146
Panel B: Mergers with No Predicted Competitive Effects, $\Delta BankHI = 0$				
Merger dummy	0.0552	0.0278	0.0001	0.0001
	(0.47)	(8.72)	(0.09)	(0.10)
Fixed effects				
Year	Yes	Yes	No	No
Property type	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes
Bank	Yes	Yes	No	No
Adj. R^2	0.45	0.26	0.84	0.78
N	3,593	5,958	14,544	14,544
N cluster	580	688	801	801

the effect of the merger itself (the dummy variable) on loan rates, loan size, and crime risks. Controlling for the effect on the competitive environment, mergers per se have no statistically significant effect on loan rates, burglary risks, and property crime risks and have a positive effect on loan size, which is inconsistent with the reverse causation hypothesis. The coefficients on $\Delta BankHI$ show that it is the extent of the merger-induced decrease in competition, not the fact of the merger, which predicts economic and social activity.

Panel B reports results for those mergers with no predicted competitive effects, where we examine the merger dummy variable conditional on $\Delta BankHI = 0$. This considers all mergers in which outsider banks purchase an insider bank operating in a neighborhood, where the ex ante expected change in local bank concentration is zero. Hence, according to a causal theory, there should be little effect on local competition associated with these mergers. On the other hand, if mergers are motivated by declining neighborhoods (reverse causation), then these outsider–insider mergers should be associated with the outcome variables. As Panel B details, mergers that have no predicted local competitive effects, indeed do not significantly predict changes in loan rates, burglary risks, or property crime risks, and are associated with larger, rather than smaller, loans. These findings are inconsistent with the reverse causation hypothesis and support the exogeneity of competition being captured by our measure.

V. County- and State-Level Regressions

For robustness, we supplement our neighborhood-level evidence with county- and state-level evidence from bank competition. This provides an additional test of our hypothesis, with variables not available at the neighborhood level, and out-of-sample evidence on crime that is obtained from another data source and over another time period. In addition, state-level data allow for the use of another exogenous measure of competition using state branching regulation.

A. County-Level Construction and Crime

While migration is likely to be less important at the county level than at the neighborhood level, a merger-induced constriction of credit should still have implications for construction and crime at the county level. One advantage of using county-level data is that explicit construction estimates are available. We are also able to use the FBI UCR at the county level for comparison with the Cap Index crime scores. This provides further robustness.

In the first two columns of Table VII, Panel A we regress the county-level changes in number of construction companies and employees from 1995 to 2000 on $\Delta BankHI_{1992:1995}$, the level and change in property crime risks, and the usual controls. The construction data come from the Bureau of Labor Statistics and the Bureau of Economic Analysis County Business Patterns. For these county-level regressions, we include state fixed effects and compute standard errors assuming groupwise clustering at the county level. The results show

Table VII

County-Level Effects and FBI Uniform Crime Reports

County-level regressions of changes in future real activity and crime on predicted changes in local bank competition through bank mergers of nonfailing commercial bank and bank holding companies with at least $1 billion in assets are reported. Panel A reports results for construction and business activity and Panel B for crime rates from the FBI's Uniform Crime Reports. The dependent variables in the first two columns of Panel A are the county-level changes from 1995 to 2000 in number of construction employees and companies, obtained from the Bureau of Labor Statistics and Bureau of Economic Analysis County Business Patterns. The last two columns of Panel A employ county-level changes from 1995 to 2000 in total value of new construction ($1,000) and number of new units constructed as dependent variables, obtained from F.W. Dodge Division of the McGraw-Hill Companies, Inc. The dependent variables in Panel B are county-level changes from 1995 to 2000 in reported homicides and burglaries per capita. All regressions include a constant, the current crime rate (1995), recent growth in crime (from 1990 to 1995), local price variation, recent growth in population, income, and home value (between the 1990 to 2000 censuses), and state fixed effects. In addition, for the crime rate regressions, results are reported employing the average abortion rate from 1982 to 1986 for the state in which the property resides and the log of the average state imprisonment rate (number of prisoners per capita) from 1994 to 1995. Coefficient estimates and their associated t-statistics (in parentheses) are reported with robust standard errors that assume groupwise clustering at the county level.

Panel A: County-Level New Construction Activity

	ΔNumber of Construction		ΔNew Construction	
Dependent Variable	Employees (1)	Companies (2)	Value (3)	Units (4)
$\Delta BankHI_{1992:1995}$	−18.6751	−0.7493	−6.3611	−6.4089
	(−5.09)	(−4.54)	(−7.29)	(−11.97)
State fixed effects	Yes	Yes	Yes	Yes
Adj. R^2	0.41	0.69	0.09	0.63
N	21,940	21,940	21,968	21,968
N cluster	38	38	41	41

Panel B: County-Level Crime Rates from the FBI Uniform Crime Reports

	Changes in Crime Rate from 1995 to 2000			
Dependent Variable	Burglary (1)	Homicide (2)	Burglary (3)	Homicide (4)
$\Delta BankHI_{1992:1995}$	2.5076	−2.2819	5.5948	0.6431
	(4.08)	(−0.70)	(2.16)	(0.12)
Abortion rate$_{1982:1986}$			−1.0510	−0.5596
			(−5.05)	(−2.12)
log(imprisonment rate)$_{1994:1995}$			0.2838	0.2175
			(2.44)	(1.73)
State fixed effects	Yes	Yes	No	No
Adj. R^2	0.97	0.92	0.62	0.30
N	21,958	21,461	21,958	21,461
N cluster	39	38	39	38

that increased merger activity between 1992 and 1995 strongly predicts a reduction in both the number of construction companies and construction employees in the subsequent period.

Columns 3 and 4 examine the impact of bank mergers on development by employing construction data from F.W. Dodge Division of the McGraw-Hill Companies, Inc. The change from 1995 to 2000 in the total value of construction (in $1,000) and in the number of units under construction is regressed on $\Delta BankHI_{1992:1995}$ and the standard controls. As the table indicates, both the value and number of units decline after local banking markets become more concentrated.

Panel B reports results from regressions of county-level crime rates (reported crimes per capita) from the FBI's UCR on our measure of merger-generated increases in bank concentration. In Column 1, we show that the number of reported burglaries per capita increases with our bank concentration variable, and in Column 2 we report that the number of reported homicides per capita does not. Both findings are consistent with our previous evidence from neighborhood-level regressions using Cap Index data. These results support the hypothesis that bank mergers increase future crime through their negative effect on the economic environment, which the crime literature shows is closely linked to property crimes such as burglary but not violent crimes such as homicide.

In Columns 3 and 4 of Panel B, we introduce control variables for the lagged average state abortion rate (the average number of abortions per 1,000 live births) from 1982 to 1986 and the log of the imprisonment rate (number of prisoners per capita) from 1994 to 1995 in place of state fixed effects.[14] The results show that merger-induced bank concentration increases burglaries and has no significant effect on homicide, even after controlling for the lagged abortion and imprisonment rates, both of which have been shown to capture substantial variation in crime (Levitt (1996) and Donohue and Levitt (2001)).

B. State Branching Deregulation: An Out-of-Sample Test

As a final test, we employ another exogenous measure for changing bank competition using state variation in bank branching regulation. Black and Strahan (2002) use changes in intra and interstate banking and branching regulation to instrument for banking competitiveness and examine the impact on small business lending as proxied by the number of incorporations in a state. Most of this regulation takes place in the 1970s and 1980s; however, unrestricted branching was adopted in some states during the 1990s, providing some variation on

[14] The abortion data come from the Alan Guttmacher Institute surveys from the Bureau of the Census United States Statistical Abstract and the prisoner population data come from Correctional Populations in the United States from the Bureau of Justice Statistics. These measures are employed by Donohue and Levitt (2001) and were graciously supplied by these authors.

state bank regulation over our sample period of 1992 to 1999.[15] Specifically, we employ the number of years since the state first allowed unrestricted bank branching, which usually meant that a bank could open a new branch anywhere in the state. This variable takes on the value of zero if the state had yet to institute unrestricted branching at the time of the property sale. Unrestricted branching should increase competition in local bank markets, hence we expect this variable to have effects opposite those of mergers.

The first column of Table VIII, Panel A reports results from regressions of local bank concentration on unrestricted branching in a state. The usual controls for each regression apply, but standard errors are calculated assuming groupwise clustering at the state and year levels. As the table indicates, unrestricted branching decreases subsequent bank concentration levels in neighborhoods. Hence, deregulation seems to increase competition. The next three columns show that, consistent with our earlier findings, loan rates decrease with competition, and loan sizes and prices increase with competition. The magnitude of the effects is smaller, though similar, to those obtained via the merger variable for competition.

In Panel B, we examine crime rates at the state level in conjunction with branching deregulation. Since we use state-level branching deregulation and crime rate data from the FBI's UCR, we are no longer restricted to the *COMPS* database. Thus, we employ crime rate data from all 50 states plus the District of Columbia over the extended period 1978 to 2001.[16] We employ a richer (both cross-sectionally and time-series) state-bank-branching measure since states adopted unrestricted branching throughout the 1970s, 1980s, and 1990s. Moreover, this analysis provides an out-of-sample test of our hypothesis using new data from new regions and time periods, and employing a new measure of credit competition.

The dependent variable in Panel B is the year-to-year change in crime (reported homicides and burglaries per capita) from year $t-1$ to year t, regressed on a post-unrestricted-branching indicator that takes on the value of one if the state adopted unrestricted branching within the last 5 years. Other control regressors include the lagged annual change from year $t-2$ to year $t-1$ in state population, employment per capita, social services employees per capita, income, and the imprisonment rate. We also employ the lagged annual change in the state abortion rate from 16 years ago (i.e., from $t-17$ to $t-16$) as an additional regressor. Due to data limitations, regressions that include the lagged abortion rate begin in 1987. Because we are interested in cross-sectional differences in changes in crime rates across states, we examine first differences in our regressions. Furthermore, because we focus on cross-sectional differences in differences, we run the regressions in the style of Fama and MacBeth (1973),

[15] California, the District of Columbia, Maryland, Nevada, and New York all adopted unrestricted branching in the early-to-mid 1970s; Massachusetts, Oregon, Texas, and Virginia adopted it in the late 1980s; Illinois in 1993; and Colorado and Georgia had not adopted this policy by 1997.

[16] Although crime rate data go back to 1960, many of the control variables we employ, such as state income and social services employees, date back only to 1977. Since we regress changes in crime on lagged changes in these variables, the sample starts in 1978.

Table VIII

The Impact of State-Level Bank Branching Regulation on Bank Concentration, Loan Activity, Prices, and Crime

Panel A employs commercial property transaction-level data from COMPS from 1992 to 1999. Regressions of the level of local bank concentration, loan rate, loan size, and capitalization rate of properties on the number of years since the state first allowed unrestricted branching (takes on the value of zero if the state had yet to institute unrestricted branching at the time of the property sale) are reported. Regressions include a constant, current crime risk (1995), recent growth in crime (from 1990 to 1995), recent growth in population, income, and home value, and property-type fixed effects. Loan rate regressions also include the loan attributes from Table II. Coefficient estimates and their associated t-statistics (in parentheses) are reported with robust standard errors that assume groupwise clustering at the state and year level. Panel B employs state-level data on crime rates (reported homicides and burglaries per 1,000 people) from the FBI's Uniform Crime Reports from 1978 to 2001. The year-to-year percentage changes in crime rates are regressed on a state-level bank branching indicator, which takes on the value of one if the state had adopted unrestricted branching within the last 5 years. Regressions also include a constant, the 1-year lagged change in state population, employment, social services (the number of social service employees per capita), income, and imprisonment rate (number of prisoners per capita), as well as the 16-year lagged change in the state abortion rate ($\times 1,000$), which is the change in the number of abortions (16 years ago) per 1,000 live births. Cross-sectional regressions across all 50 states plus the District of Columbia are run each year from 1978 to 2001 (1987 to 2001 when including abortion rates) in the style of Fama and MacBeth (1973), where the time-series average of the yearly cross-sectional regression coefficient estimates are reported along with their time-series t-statistics (in parentheses).

Panel A: Transaction-Level Loan Activity and Prices from *COMPS*

Dependent variable	Actual *BankHI* (1)	Loan Rate (2)	Loan Size (3)	Cap rate (E/P) (4)
Years of unrestricted branching	−0.0006	−0.0152	0.0015	−0.0230
	(−3.19)	(−2.02)	(4.25)	(−3.37)
Price variation	−0.0074	0.0410	−0.0147	0.6660
	(−2.24)	(0.36)	(−1.46)	(5.97)
Population growth	0.0002	0.0051	−0.0003	−0.0059
	(1.78)	(0.46)	(−0.35)	(−0.69)
Income growth	0.0012	0.1401	−0.0001	−0.0499
	(2.72)	(1.34)	(−0.66)	(−0.40)
Home value growth	0.0001	−0.0003	−0.0001	
	(2.20)	(−0.39)	(−2.11)	
N	22,317	6,267	16,131	10,070
N cluster	96	96	96	96

(continued)

where each year a cross-sectional regression (across the 50 states plus D.C.) is run and the time-series average of the yearly cross-sectional coefficient estimates and their associated time-series standard errors are calculated.[17]

[17] The Fama–MacBeth (1973) procedure produces conservative standard errors by allowing anything that drives variation in the year-to-year coefficient estimates to show up in the standard error. This effectively ignores any cross-sectional residual correlation from the yearly regressions, resulting in robust standard errors. For further detail regarding this method and its merits see Fama and MacBeth (1973) and Cochrane (2001).

Table VIII —*Continued*

Panel B: State-Level Changes in Crime Across All 50 States from the FBI				
	Fama-MacBeth Cross-Sectional Regressions			
Dependent Variable = $\%\Delta\text{Crime}_t$ per 1,000 People	$\%\Delta\text{Homicide}_t$ (1978–2001) (1)	$\%\Delta\text{Homicide}_t$ (1987–2001) (2)	$\%\Delta\text{Burglary}_t$ (1978–2001) (3)	$\%\Delta\text{Burglary}_t$ (1987–2001) (4)
Post unrestricted branching indicator	−0.0094	−0.0143	−0.0150	−0.0100
	(−0.87)	(−1.39)	(−3.48)	(−3.40)
$\Delta\text{Population}_{t-1}$	−0.0001	0.0001	−0.0001	−0.0001
	(−0.53)	(0.49)	(−0.48)	(−1.08)
$\Delta\text{Employment}_{t-1}$	5.5718	4.8934	0.7494	0.5595
	(5.03)	(5.39)	(2.71)	(2.12)
$\Delta\text{Social services}_{t-1}$	−0.0070	−0.0056	−0.0029	−0.0042
	(−4.21)	(−5.72)	(−6.25)	(−9.36)
$\Delta\text{Income}_{t-1}$	−23.981	−14.509	−3.4942	2.8806
	(−3.31)	(−2.48)	(−1.70)	(2.32)
$\Delta\text{Imprisonment rate}_{t-1}$	−0.0261	−0.0106	0.0129	0.0088
	(−1.29)	(−1.09)	(2.68)	(2.35)
$\Delta\text{Abortion rate}_{t-16}$		−0.3173		−0.1007
		(−5.75)		(−6.08)
N (no. of years)	24	15	24	15

As Panel B of Table VIII indicates, the post-state-branching variable has a significant negative effect on burglary, with point estimates of small, though similar, magnitude to those obtained from our merger variable in the *COMPS* sample. The effect on homicide is negative, but statistically insignificant, as in our previous results. These findings, on a new data set over a different sample period, across all states, and using a new exogenous measure of competition bolster our contention that there are links among credit market competitiveness, real activity, and crime.

VI. Conclusion

In this paper, we provide evidence that neighborhoods most affected by bank mergers subsequently experience significantly worse credit terms, less development and investment, lower real estate prices, an inflow of poorer households, and greater increases in property crime in subsequent years. These results are confirmed by our finding that state branching deregulation leads to more credit supply and fewer future property crimes. Our results highlight that credit market imperfections can have important spillover effects on both economic and social outcomes.

These findings suggest that in evaluating the impact of bank mergers, regulators would do well to consider not only the present bank concentration of potentially affected markets, but also the social fragility of these markets. Neighborhoods that already suffer from low median incomes and poor loan

competition experience greater increases in crime after a merger-induced reduction in banking competition. We do not argue that every bank merger has a negative impact on local credit conditions, but our results do suggest that it would be reasonable to apply greater scrutiny to mergers affecting delicate and marginal areas, where they seem to have significant competitive effects that generate economic and social consequences.

We propose and provide evidence on one simple mechanism that connects crime and real activity to the market for finance, but a more general link among financial access, development, and social outcomes, including, for example, education and health care, may be worth pursuing.

Appendix: Examples of Mergers Included and Excluded from the Competition Measure

Table AI presents examples of the bank and bank holding company mergers included and excluded from our measure of the predicted change in bank loan concentration through mergers. Only mergers between nonfailing banks or bank holding companies with at least $1 billion in assets are included in our competition measure. Two examples of bank mergers included in our measure are provided along with two examples each of bank mergers excluded from our measure due to failure (requiring assistance from either the FDIC, RTC, NCUA, state, or other regulatory agency), of mergers within the same holding company, and of mergers involving assets less than $1 billion.

Table A1.
Examples of Mergers Included and Excluded from the Competition Measure

Acquirer	Target	Type of Union	Date	New Name	Included/ Excluded	Reason
National Bank of Detroit	The First National Bank of Chicago	Holding company acquisition	December, 1995	First Chicago NBD Corp.	Included	Met all criteria
Wells Fargo Bank	First Interstate Bankcorp	Merger	April-June, 1996	Wells Fargo Bank	Included	Met all criteria
California Federal Bank	Pacific Heritage Bank	Merger	July, 1995	California Federal Bank	Excluded	Required assistance (failure)
Union Bank	Bank of Newport	Merger	August, 1994	Union Bank	Excluded	Required assistance (failure)
The Bank of San Diego	Coast Bank	Merger	December, 1992	The Bank of San Diego	Excluded	Same holding company
Corus Bank	Lincoln National Bank	Merger	June, 1996	Corus Bank	Excluded	Same holding company
Mountain Parks Bank-East	The Kiowa State Bank	Merger	August, 1996	Mountain Parks Bank-East	Excluded	Acquired bank had $41 million in assets
U.S. Trust	Somerset	Merger Savings Bank	July, 1998	U.S. Trust	Excluded	Acquired bank had $533 million in assets

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