Homeowner Borrowing and Housing Collateral: New Evidence from Expiring Price Controls

Online Appendices

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A. Additional Institutional Detail

TABLE A.1
MPDU Income Limits – 2014

Household Size	Adjustment Factor	Maximum Household Income	Minimum Household Income
1	0.70	$52,500	$35,000
2	0.80	$60,000	$35,000
3	0.90	$67,500	$35,000
4	1.00	$75,000	$35,000
5	1.08	$81,000	$35,000

Note.—This table shows the MPDU income limits for households of various sizes in 2014. For a four-person household, the maximum income limit is set at 70% of the area median income for the Washington, D.C. metropolitan area as published by the U.S. Department of Housing and Urban Development (HUD). That limit is then multiplied by the adjustment factor shown in the second column to determine the maximum income limits for households of other sizes. The minimum income limit is the same for all households and is set based on consultation with lenders in order to reflect the minimum income required to qualify for a typical mortgage on an MPDU home.

TABLE A.2
History of MPDU Control Period Rules

Date MPDU Originally Offered for Sale	Control Period (Years)	Control Period Resets on Resale
Before October 1, 1981	5	No
October 1, 1981–February 28, 2002	10/15	No
March 1, 2002–March 31, 2005	10	Yes
After March 31, 2005	30	Yes

Note.—This table shows the history of MPDU control period rules from the inception of the program to the present. The rules governing the length of the control period and whether the control period resets upon resale prior to expiration are determined based on the date the MPDU was originally offered for sale by the developer. For MPDUs originally offered for sale between October 1, 1981 and February 28, 2002, the length of the control period was 10 years with the exception of MPDUs located in one of several DHCA designated “Annual Growth Policy Areas” for which the control period was 15 years.

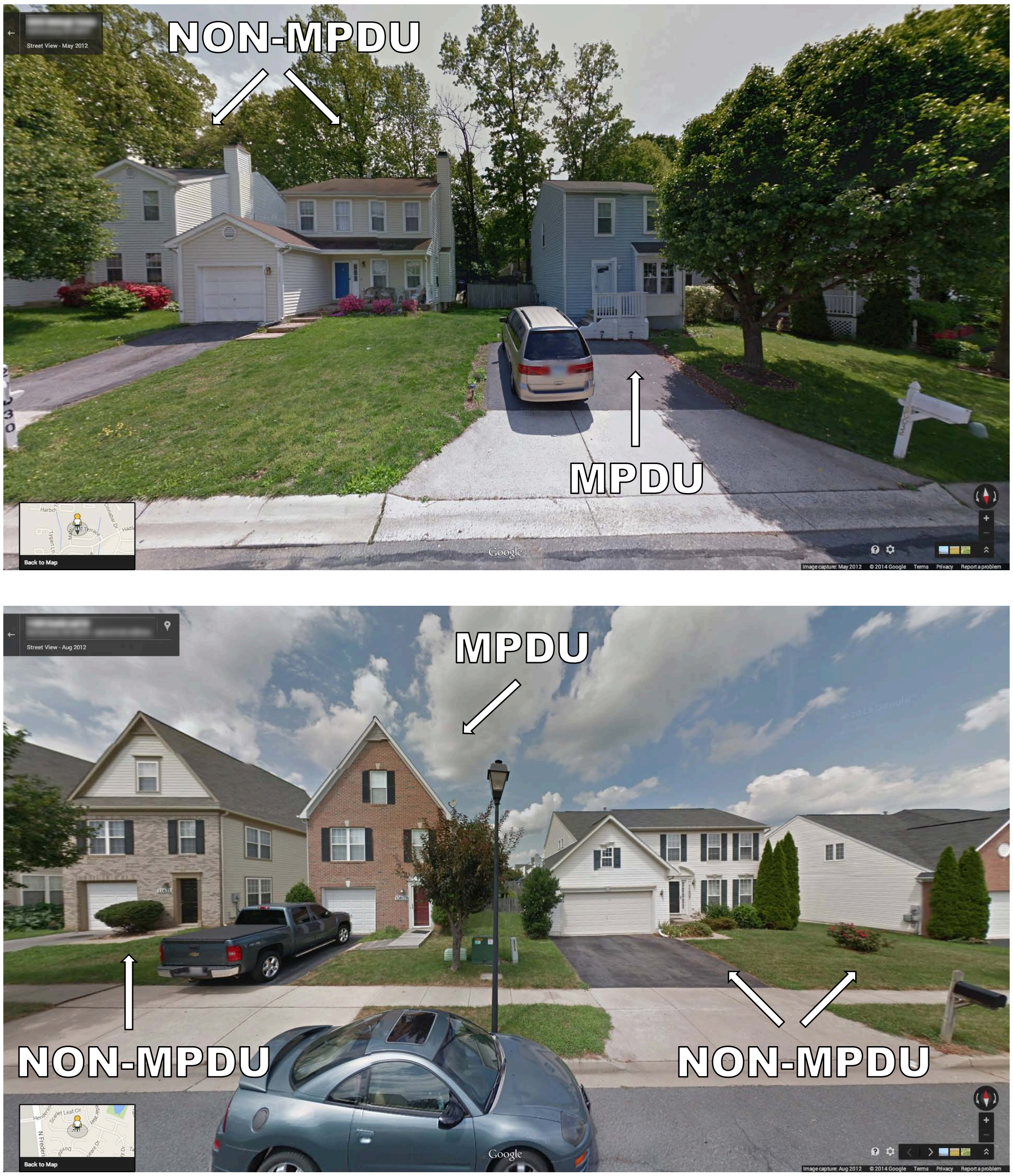


FIGURE A.1
Examples of MPDU Exterior Design

NOTE.—This figure presents images of the exterior of two representative MPDUs and nearby market-rate units. The MPDUs shown are located in different subdivisions. Images were accessed on June 24, 2014 and captured by Google Maps in May (top image) and August (bottom image) of 2012.

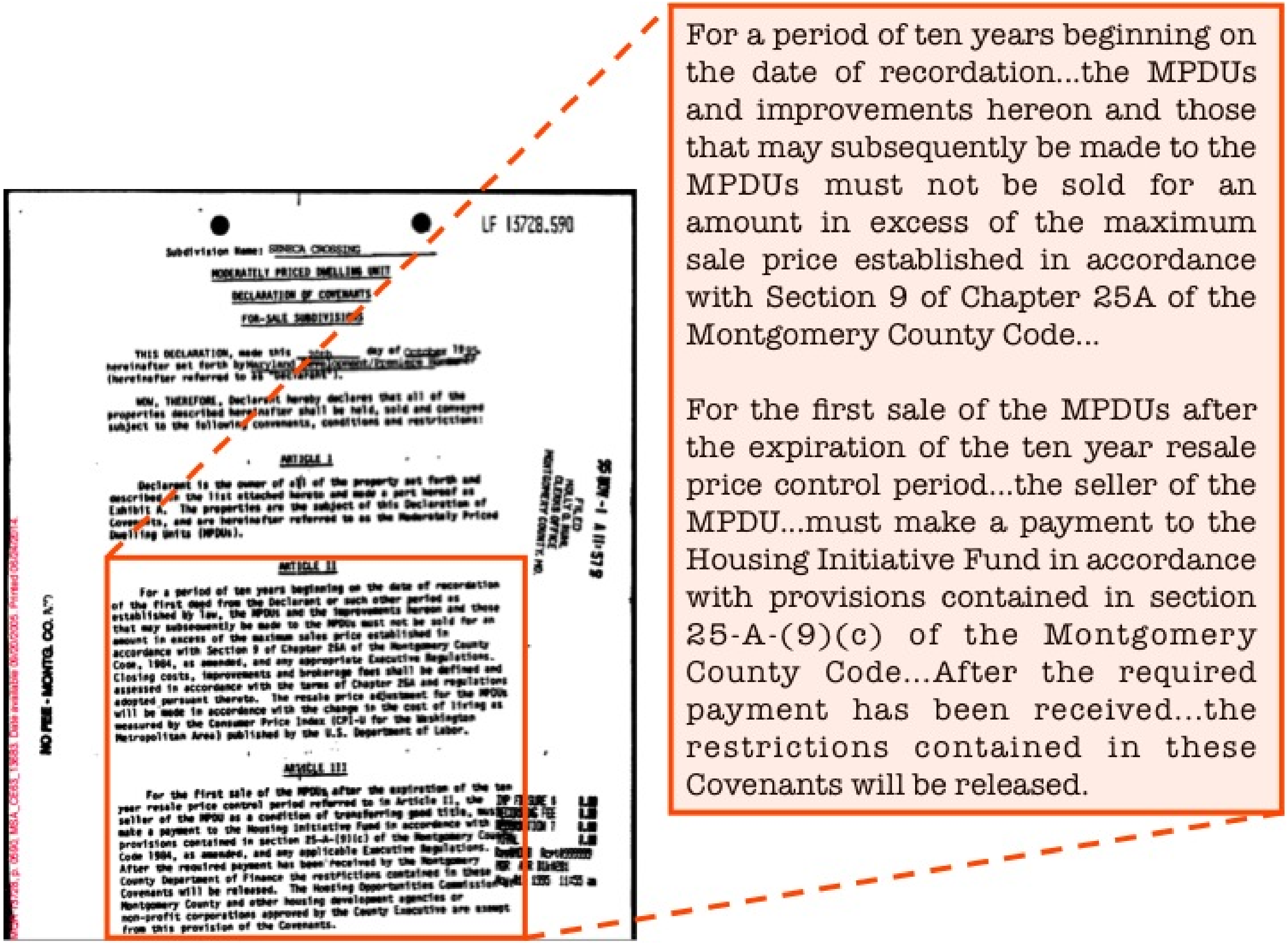


FIGURE A.2
Example Deed Restriction

NOTE.—This figure shows an example deed restriction for an MPDU originally sold by the developer on October 20, 1995. The deed was originally recorded in the Montgomery County Circuit Court (Land Records) MQR 13728, p. 0590, MSA_CE_63-13683 and was accessed online through MDLANDREC on June 24, 2014.

B. Data

B.1 Matching MPDU Properties to the DataQuick Assessment File

MPDU properties were matched to the DataQuick assessment file in several steps. Prior to matching, the raw addresses in both datasets were cleaned in order to correct obvious spelling errors, such as city names, and to standardize common abbreviations for street suffixes and compass directions (North, South, etc.). After being cleaned in this way, both sets of addresses were geocoded using an address locator service provided by the state of Maryland and developed in close collaboration with local jurisdictions that provides highly accurate geographic coordinates for addresses located throughout the state. Of the 8,289 MPDU properties, 91.8% were assigned an exact geographic location through this process. Many of the remaining 8% had expiration dates that occurred prior to 1980, suggesting that the reason they went unmatched was likely due to poor record keeping in the early years of the program. The DataQuick match rate was substantially higher. Over 99% of the properties were assigned unique geographic coordinates by the Maryland address locator. For the remaining unmatched DataQuick properties, I kept the geographic coordinates assigned by DataQuick, which uses a less accurate national address locator.

In the first step of the match, MPDU properties were assigned to DataQuick properties using exact geographic location. For each MPDU that was given a set of geographic coordinates, I first found the closest DataQuick property measured in straight-line distance. If the closest property was less than one foot away, the DataQuick property ID was assigned to that MPDU and considered a match.[1] All the remaining properties were considered unmatched and proceeded to the next step.

The remainder of the matching process used the actual address strings contained in both datasets. This part of the process proceeded in 13 iterations, re-matching unmatched addresses on increasingly lenient criteria at each step of the process. In the first step, properties were considered matched if all components of the address string—house number, street name, unit number, zip code, and city—perfectly agreed. In the next two steps, previously unmatched properties were considered matched if all components of the address agreed except for one of either city or zip code (but not both). In the fourth step, properties were matched only if the entire address agreed, but some spelling error in the street name was accommodated by using the soundex code for the street name.[2] Steps five and six again allowed either city or zip code (but not both) to

[1]The Maryland geocoding service uses a "composite" address locator which looks to several sources to identify the geographic coordinates for a particular address. Because of this, it is possible for there to be slight differences in the geographic coordinates for the same property if it is identified using a different source. This is likely what generates distances that are less than 1 foot but still greater than zero.

[2]Soundex is a phonetic algorithm that indexes words based on their pronunciation. The goal of the algorithm to

disagree while requiring an exact match on the soundex street name and the other components of the address. Steps 7–12 repeated steps 1–6, but allowing the unit number to differ at every step as long as one property had a missing unit number and the other did not. Explicit unit number disagreements were never permitted. In the final step, unit, city, and zip code were all allowed to disagree as long as the house number and street name perfectly agreed. At every step of the process, non-unique matches were randomly assigned.

Overall, the quality of the match is quite high. In total, 7,404 (90%) of the MPDUs were matched to a DataQuick property at some point in the process. Of these, only 2% were randomly assigned due to non-uniqueness. Eighty-two percent of matches occurred in one of the first two steps using either exact geographic location or the exact and complete address string. Ninety-five percent of the matches occurred in the first six steps, which required the unit numbers to agree.

B.2 Cleaning the Transaction and Loan-Level Data

The transaction and loan-level datasets were both cleaned in order to ensure that the transactions represent true ownership-changing arm's length transactions and that the loan information is accurate and consistent. The procedures for cleaning each dataset are described below.

Cleaning the Transactions Data

The raw transactions dataset contains 249,264 transaction records that are coded by DataQuick as "arm's length" and involve one of the 286,484 single-family residential properties with non-missing housing characteristics contained in the assessment file. Starting with this sample, I first dropped 4,169 transactions where the year of sale preceded the year that the associated property was built (i.e. vacant land sales). A few transactions (197) recorded as having occurred in the last quarter of 2012 were dropped because the file provided by DataQuick was received in that quarter and only meant to cover up through the first three quarters of 2012. Many of the transactions recorded in 1997 were listed twice, with the first record containing the transaction price and no loan amount and the second record containing a loan amount with either no transaction price or a clearly erroneous transaction price (e.g. \$1.00). In these cases, all of the other transaction characteristics were identical including the dates, buyer and seller names, and lenders. To correct this, I replaced the missing loan amount in the first record with the loan amount from the second record, and dropped the second record. This dropped 6,431 erroneous "transactions." A similar issue was present for a smaller number of transactions recorded in other years. In these cases, two transactions were recorded on the same day for the same property with transaction prices that differed by less than 1% and only one record containing a positive or realistic loan amount. In these cases, I kept the record with the non-erroneous loan amount and dropped the 2,918 duplicate

encode homophones in a similar manner. For example, "Willow Road" and "Wilow Road" have the same soundex code.

records. A few transactions were exact duplicates of another transaction on property, date, price, and loan amounts, but differed along some other dimension (typically an alternate lender name). In these cases, one of the duplicates was randomly dropped. This dropped 375 records. In some cases, two transactions were recorded on the same day for the same property where the buyer for the first transaction was listed as the seller on the second transaction and the price for the first transaction was clearly not a market price. In these cases, the intermediary was typically a title company, escrow company, or some other similar entity and only the transaction containing the market price was kept (dropping 656 intermediary transactions). Finally, I randomly dropped 474 transactions that were exact duplicates of another transaction on property and date but differed along some other dimension for unknown reasons as well as 165 transactions recorded on the same property in the same week but differing in some other way. In these cases, one of the records was kept to serve as a "placeholder" documenting the change in ownership that occurred on that day or during that week. These transactions were used in determining change of ownership, but their prices were not included in any analyses. The final sample contained 233,879 transactions involving one of the 286,484 single-family residential properties with non-missing housing characteristics contained in the assessment file.

Cleaning the Non-Purchase Loans Data

The non-purchase loans dataset was cleaned in a similar way as the transactions data. The raw dataset contains 780,927 non-purchase loans recorded on one of the 286,484 single-family residential properties with non-missing housing characteristics contained in the assessment file. Starting with this sample, I first dropped the 267 records with non-positive loan amounts. I also dropped 1,477 records where the loan amount was listed as $1.00. All of these records listed the lender name as "HUD," and were typically recorded on the same day as another loan with a more sensible loan amount. The DataQuick data contains a variable indicating whether a loan involved multiple parcels. This can happen when a real-estate investor owns multiple properties and borrows against their full portfolio using a single loan. I dropped all 4,130 loans coded in this way. As with the transactions data, I also dropped 3,074 loans recorded in years prior to the year that the associated property was built and 781 loans recorded in the last quarter of 2012. In cases where multiple loans were recorded on the same property on the same date with the same loan amount, I randomly kept one of the duplicates and dropped the remaining 743 records. Visual inspection of these records suggests that in most cases all other characteristics of the loan were also identical except for slight variations in lender name. Finally, I recoded 1,366 non-purchase loans as purchase loans if they occurred in the same week as a transaction recorded on the same property with no positive loan amounts. In these cases, the recoded loans were removed from the non-purchase dataset and recorded as the first loan on the associated transaction in the transactions dataset. The final sample of non-purchase loans contains 769,089 loans secured against one of the 286,484 single-family residential properties with non-missing housing characteristics contained in the assessment file. These loans were used to construct the equity extraction measures used in the analysis.

B.3 Constructing "Debt Histories"

To accurately measure equity extraction, it is important to distinguish between three different types of non-purchase loans: (1) regular refinances, which replace an existing loan without extracting any equity; (2) cash-out refinances, which replace an existing loan with a *larger* loan, thereby extracting equity for the amount of the difference; and (3) new non-purchase originations, which directly extract equity for the amount of the new loan. In order to make this distinction, I construct a "debt history" for every property that records an estimate of the current amount of outstanding debt secured against the property at any point in time on up to two potential loans. Given this history, when a new loan is observed, I am then able to determine whether that loan represents a purchase loan, cash-out refinance, new non-purchase origination, or regular refinance by comparing the size of the new loan to the estimated outstanding balance on the relevant existing loan. This section describes the details of that procedure.

At each point in time, a given property can be thought of as having two potential "loan accounts," representing the current owner's first and second mortgage (I assume that owners carry at most two mortgages). The debt histories I construct are meant to estimate the remaining balance owed in each of these two accounts. The balances in the two loan accounts are initialized based on the first observed event in the property's history. For example, if the first observed event is a transaction with a $100,000 first loan and no second loan, then the balance in the first loan account will be initialized at $100,000 and the balance in the second loan account will be initialized at zero. If the first observed event is a non-purchase loan, then the balances are initialized based on a comparison of the loan amount with an estimate of the property's current resale value. Current resale values are estimated using quarterly constant-quality hedonic price indices constructed from the transactions data for each of 28 local planning areas designated by the Montgomery County Planning Department (see Appendix B.4 for details on how the price indices are constructed). These indices are used to adjust either the most recent transaction price or, for properties that never transact, the 2011 assessed value to the relevant quarter.[3] If the loan amount is greater than 50% of the estimated current resale value, then the loan is used to initialize the first account balance and the second account balance is initialized at zero. If the loan amount is less than 50% of the current resale value, then the loan is used to initialize the second account

[3] Since prices for MPDU properties are not permitted to appreciate faster than the rate of inflation during the control period, I use the Consumer Price Index for All Urban Consumers (CPI-U) to adjust prices for these properties before the end of the control period and the hedonic price indices afterwards. Only transactions on the relevant side of the expiration date are used to derive the current resale price for MPDUs.

balance and the first account balance is initialized at zero.

When a new transaction occurs, the balances in each account are replaced with the loan amounts associated with that transaction and a new ownership-spell is initiated. For non-purchase loans that occur between transactions, the balances in each loan account are updated based on a comparison of the new loan amount with the amortized balances remaining in the two accounts as of the date of the new loan. Since the DataQuick data does not contain information on loan terms or interest rates, all loans are amortized using the average offered interest rate on a 30-year fixed rate mortgage in the month that the loan was originated. Monthly average offered interest rates are taken from the Freddie Mac Primary Mortgage Market Survey (PMMS). Similarly, since the data does not distinguish between closed-end liens and HELOCs, all loans are treated as fully amortizing with an initial principal balance equal to the origination amount, which, for HELOCs, represents the maximum draw-down amount.

Several rules are used to determine whether the new loan updates the first loan account or the second loan account. When both accounts have a positive remaining balance, the new loan updates the account with the remaining balance that is closest to the new loan amount. If the new loan amount is at least 5% larger than the old loan, then the new loan is considered a cash-out refinance and replaces the old balance.[4] In this case, the difference between the two loans is counted as equity extraction. If the new loan is less than 5% larger than the old loan, then the new loan is considered a regular refinance and replaces the old balance, with no equity extraction recorded. If the second loan account has a zero remaining balance while the first loan account has a positive balance, then one of two things will happen. First, if the new loan is larger than 50% of the current first loan balance, then the new loan will replace the first loan and equity extraction will be determined using the same rules as above. Second, if the new loan is less 50% of the current first loan balance, then the new loan will replace the zero balance in the second loan account and be counted as a new loan origination. For new originations, the entire loan amount is counted as equity extraction. In the rare case in which there is a positive second loan balance and zero first loan balance the same rules are followed; in this case, however, the comparison is made with respect to the estimated current resale value rather than the first loan balance. Finally, if both loan accounts have a zero current balance, the new loan always replaces the first loan and is counted as a new origination.

Validating the Equity Extraction Measure

While the deeds data provide exhaustive coverage of all loans secured against a property, the assumptions needed to determine whether a new loan adds to or replaces existing debt introduce

[4]The 5% threshold is chosen to reflect the cutoff used by Freddie Mac in its definition of cash-out refinancing.

measurement error in the equity extraction variable.[5] To gauge the magnitude of this error, Figure B.2 presents aggregate time series evidence comparing my equity extraction measure against two external measures that were calculated using data from which it is possible to directly determine whether a new loan adds to a borrower's existing debt. Panel A. plots the yearly average probability of equity extraction. The dashed grey line plots a measure of equity extraction that was calculated by Bhutta and Keys (2014) using nationally representative data from the Equifax Consumer Credit Panel (CCP). The CCP data tracks individual debt obligations at a quarterly frequency and provides a near complete picture of the liability side of household balance sheets. Using this data, Bhutta and Keys (2014) define equity extraction as any instance in which an existing homeowner's total mortgage debt increases by more than 5%. The two solid lines were calculated using the DataQuick data and the measure of equity extraction discussed above. They plot the fraction of properties from which equity was extracted in each year for all properties in Montgomery County (orange circles) and for the restricted set of properties in my analysis sample (blue squares).[6] The two methods of measuring equity extraction generate remarkably similar time series. Both the DataQuick series and the Equifax series increase rapidly during the period 1999–2003 before reaching a peak of roughly 20 to 25% and eventually declining and leveling off at around 5% by 2010.[7] The correlations between the Equifax measure and the two DataQuick series are also reported in the figure and are greater than or equal to 0.95 in both cases. Panel B. provides an alternative way of validating my measure—in this case, plotting the fraction of all refinance loans that in the DataQuick data that I code as cash-out. The dashed grey line plots a similar series taken from Freddie Mac's Quarterly Cash-Out Refinance Report. This series reports the share of all refinance mortgages in Freddie Mac's portfolio that were at least 5% larger than the loan they replaced. Again, the two methods for measuring the cash-out share generate very similar time series. Both the DataQuick series and the Freddie Mac series show clear cyclicality in the early and late 2000s, mimicking the cyclicality of interest rates during that period. The correlations in this case are slightly lower but nonetheless still quite high (0.87 for the full sample and 0.82 for the analysis sample). Taken together, the evidence presented in Figure B.2 suggests that any measurement error in my equity extraction variable is not substantial enough to affect the ability of that variable to accurately measure changes in equity extraction over time.

[5] Under standard assumptions, such measurement error should only affect the precision of my estimates and not their accuracy. In particular, measurement error in the dependent variable does not introduce bias or inconsistency as long as the measurement error is uncorrelated with the explanatory variables. See, for example, Bound, Brown, and Mathiowetz (2001).

[6] The criteria used to select the analysis sample are described in detail in Section III.C..

[7] The series in Panel A. are only shown for 1999–2010 because Bhutta and Keys (2014) only report their measure for that period.

B.4 *Estimating Local House Price Indices*

The current resale values used to generate the "debt histories" for each property are estimated using quarterly constant-quality hedonic price indices constructed from the transactions data for each of 28 local planning areas designated by the Montgomery County Planning Department. I use planning areas to construct the house price indices because their boundaries are drawn in order to specifically take into account the homogeneity of interests, land use types, and local economic conditions of each respective area.[8] I use hedonic indices instead of repeat sales indices in order to maximize the number of local indices available since repeat sales indices generally require much more data and therefore need to be estimated over larger geographies.

To construct the price indices, I begin by estimating the following hedonic regression:

$$\log(P_{ijmt}) = \alpha + X_i'\beta + \gamma_m + \psi_j \times \eta_t + \epsilon_{ijmt}, \tag{B.1}$$

where P_{ijmt} denotes the transaction price of property i, in planning area j, that transacts in calendar month m and quarter t, X_i is a set of property characteristics, γ_m is a set of calendar month fixed effects, $\psi_j \times \eta_t$ is a set of fully interacted planning area by quarter fixed effects, and ϵ_{ijmt} is the error term. The property characteristics include a quadratic in the interior square footage of the home, dummies for the number of bathrooms and the number of stories, dummies for the year the property was built, and an indicator for whether the property is a condo or townhome as well as the interaction of that indicator with all the other characteristics. The property characteristics are included to control for changes in the composition of the transacted housing stock, while the calendar month fixed effects are included to control for the well-known seasonality of the housing market.

Having estimated this regression, I then obtain the (exponentiated) predicted values for each property, leaving out the contribution of the property characteristics and calendar month dummies. These predicted values, which are constant within planning area and quarter, are then used to construct the price index. Specifically, let $\widehat{P_{jt}}$ denote the predicted value for planning area j in quarter t. Then the price index for that planning area and quarter is given by

$$HPI_{jt} = 100 \times \frac{\widehat{P_{jt}}}{\widehat{P_{p0}}}, \tag{B.2}$$

where quarter zero is the base period used to normalize the index. Figure B.3 plots the price indices for all 28 planning areas normalized to 100 in the first quarter of 2000. In general, prices

[8] Planning areas also have the added advantage that they cover the entire county and are large enough to provide enough data to reliably estimate a local house price index. The median planning area contains approximately 8,000 properties, which is about six times more than the median census tract.

in Montgomery County evolved similarly to the national housing market over this period; however, there is substantial heterogeneity even within the county. Some rural areas barely saw any changes in prices over this period, while prices nearly tripled in some of the more volatile areas of the county.

B.5 Matching the DataQuick Transactions Data to HMDA

To gauge the economic and demographic representativeness of my sample, I match a subset of the DataQuick transactions data to data on mortgage applications reported under the Home Mortgage Disclosure Act (HMDA) of 1975. HMDA requires lenders to report loan-level information on all loan applications received in a given year for home purchases, home purchase pre-approvals, home improvements, and refinances involving 1 to 4 unit and multifamily dwellings. This data is made publicly available by the Federal Financial Institutions Examination Council (FFIEC). I match the HMDA data to the transaction-level data from DataQuick using information on the primary loan amount, lender name, loan type (Conventional, FHA, VA), census tract, and year in which the transaction occurred. This section contains an overview of the matching process.

Prior to matching the data, I first selected a subsample of eligible transactions and loan applications based on two criteria. First, I restricted each dataset to include only transactions or loan applications pertaining to single-family homes with a positive first lien amount.[9] Second, I restricted the HMDA data to include only home purchase loan applications for which a loan was actually originated and presumably resulted in a completed transaction.

I then matched the data using a straightforward iterative process that proceeded in steps, rematching unmatched transactions and loans on increasingly lenient criteria at each step. In the first step, each transaction was matched to a loan using the year in which the transaction occurred, the census tract number, the loan type (Conventional, FHA, VA), the exact lender name, and the exact loan amount. [10] In cases where there were multiple matches, one of them was randomly assigned as being the true match while the rest were considered unmatched.

In the second step, remaining unmatched observations were then re-matched again based only on year, census tract, exact lender name, and exact loan amount, with multiple matches being randomly assigned as in the first step. Steps three and four repeated the first two but used a truncated version of the lender name containing only the first 5 letters. In steps five and six the lender name was omitted entirely. These six steps were then repeated allowing the loan amount

[9]HMDA only reports property type and whether a loan was a first or subordinate lien starting in 2004. However, this information is reported for all years in DataQuick. I restrict the DataQuick sample in all years and the HMDA sample in the years in which the information is available. There is no discernible difference in match rates or match quality between the years preceding and following 2004.

[10]HMDA rounds loan amounts to the nearest $1,000. Accordingly, I used a rounded version of the DataQuick loan amount to conduct the merge, but required that the rounded amounts agree exactly in both datasets. Census tracts were all converted to reflect boundaries as of the 2000 census using a crosswalk file provided by the U.S. Census Bureau.

to differ in increments of $1,000 up to a total allowable difference of $10,000 in the very last step. Any observations remaining after this process are considered unmatched.

In total, 79.19% of the 233,879 transactions in the cleaned DataQuick file were matched at some point in the procedure. Of those, approximately 70% were matched in the first step requiring an exact match on the full lender name and including the loan type. Of the matched observations, 13.8% were randomly assigned due to having multiple matches.

To validate the quality of the match among matched observations, I further merged the surnames provided in the DataQuick transactions file with a Census generated list of the 1,000 most common surnames in the U.S. that also tabulates the percent of people with each name by race. For each race—Black, White, Hispanic, and Asian—I then grouped the Census percentages into 5% bins and used the race reported in HMDA to calculate the fraction of matched transactions in that bin for which the buyer had the same race. Since race was not used as a matching criteria, a strong positive correlation between the Census shares and matched HMDA shares would imply that the HMDA/DataQuick match does a good job of identifying the demographic characteristics of a particular home purchaser. Figure B.4 plots these correlations separately for each race. In each panel the blue circles plot the fraction of matched transactions in each 5% bin who report having the indicated race on their loan application. The black line in each panel plots the fitted values from a regression estimated in the underlying unbinned microdata, while the slope coefficient from that regression and its standard error are reported in the top left of each panel. The dashed orange line is the 45-degree line. For each race, the correlations are close to one and precisely estimated, suggesting that the match successfully identifies the demographic characteristics of individual home buyers.

B.6 Residential Building Permits Data

The data used to measure residential investment activity was obtained from the Montgomery County Department of Permitting Services and includes address-level information on all residential building, home improvement, and mechanical permits issued by the department since 2000. Each record includes information on the date the permit was applied for, the street address for the associated property, the type of work to be performed, and the type of structure on which the work will be performed. The permits data covers all areas of the county except for the cities of Gaithersburg and Rockville, which have their own permitting departments. The data covers both new construction and any major renovations, alterations, improvements, or additions to a home as well as any work performed on the heating, ventilation, and air-conditioning (HVAC) system. I drop any records where the listed structure type is clearly non-residential (e.g. "Restaurant," "Commercial," "Industrial") as well as any records for which the listed work type is not related to the actual improvement or alteration to the property (e.g. "Inspect and Approve", "Information"). The remaining dataset contains 148,647 unique permit applications, which I match to the DataQuick assessment file using the same approach used to match the list of MPDU addresses (see Appendix B.1) but allowing for multiple permits to match to the same property. Of the original 148,647 permits, a total of 137,510 (92.5%) were matched to a DataQuick property at some stage of the matching procedure. These permits were then used to construct the annual property-level panel used in the analysis as described in Section VI.

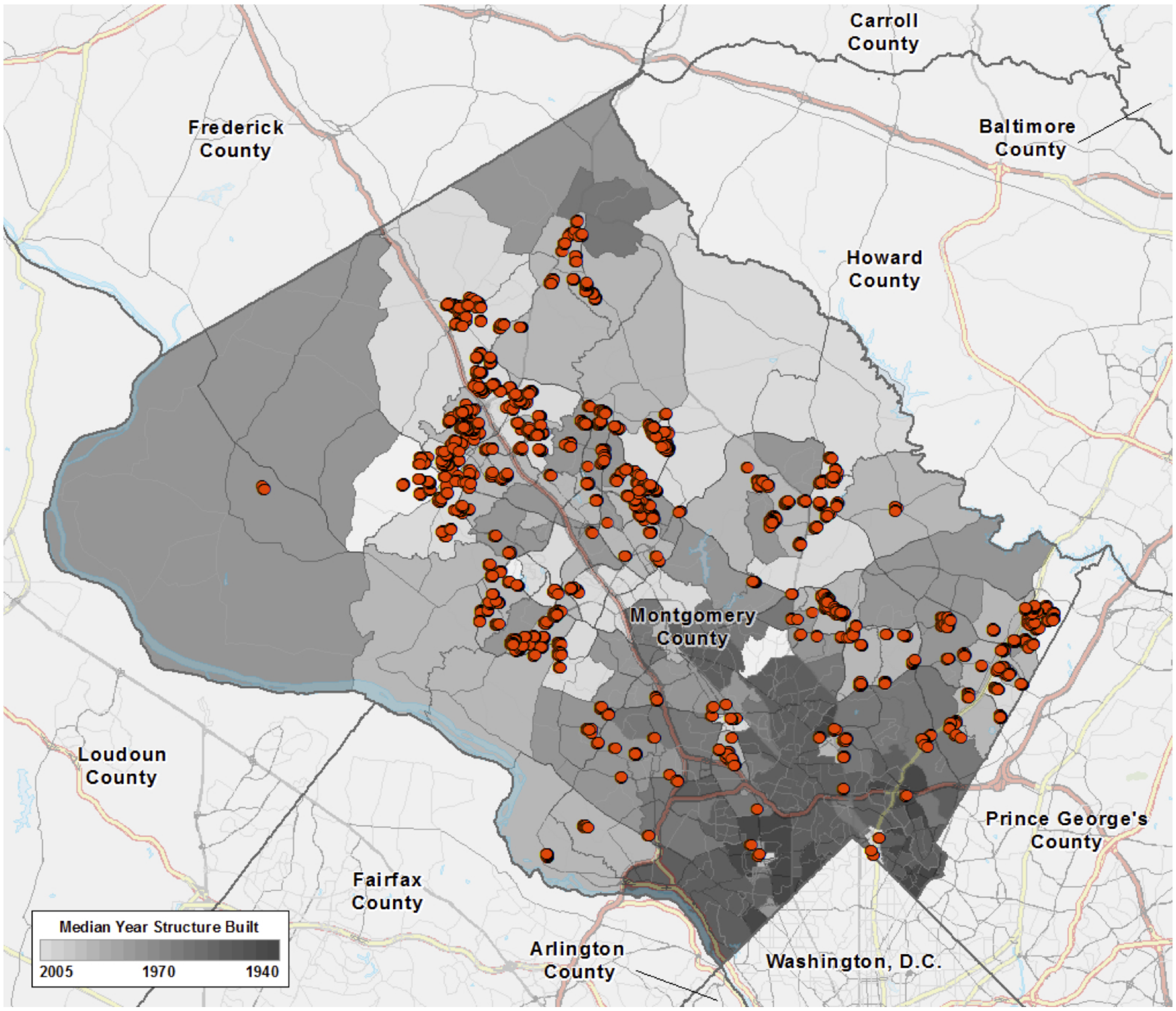
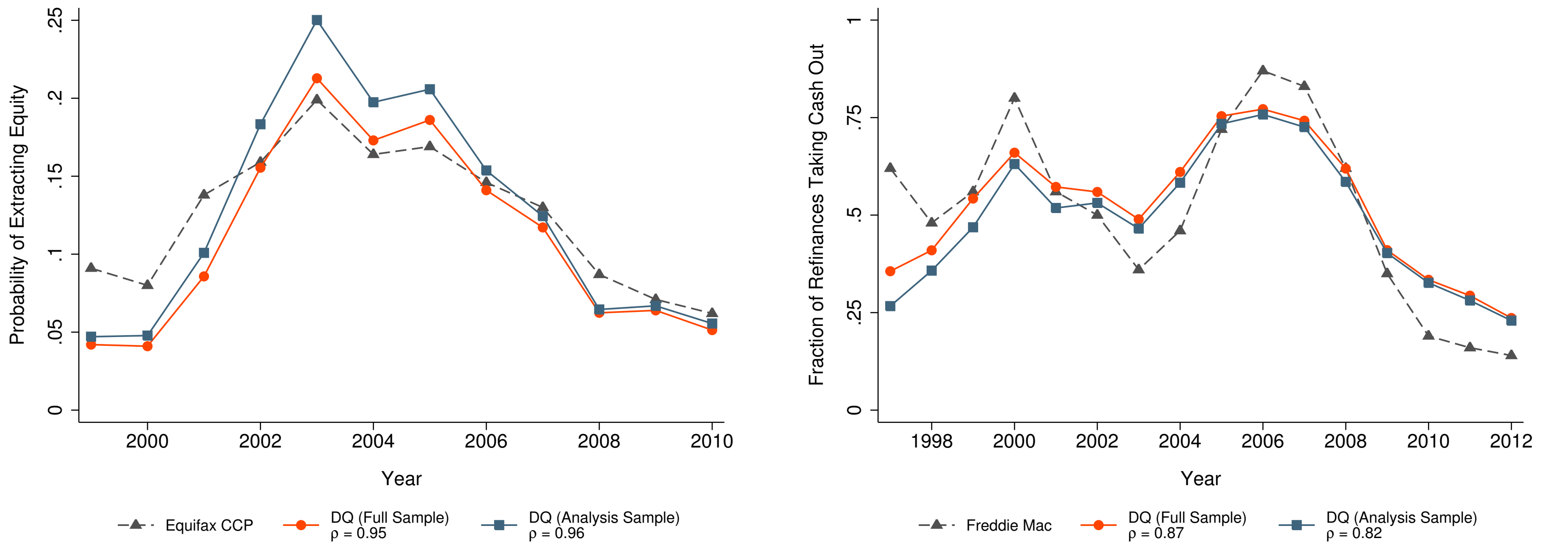


FIGURE B.1
Geographic Distribution of MPDU Properties within Montgomery County, Maryland

NOTE.—This figure shows the location of all MPDU properties that were successfully matched to a property in the DataQuick assessment file (N=7,404). MPDU properties are marked with an orange circle. Census tracts within Montgomery County are shaded according to the median year built for all housing units in the census tract as reported in the 2010 American Community Survey.



Panel A. Probability of Extracting Equity

Panel B. Cash-Out Refinance Share

FIGURE B.2
Validating the Home Equity Extraction Measure

NOTE.—This figure provides evidence validating the accuracy of the home equity extraction measure derived from the DataQuick deeds records against nationally representative aggregate series derived from other sources. Panel A. plots the yearly aggregate probability of extracting equity. Panel B. plots the yearly fraction of refinance mortgages that were cash-out. In both panels, the solid lines were generated using the equity extraction measure for Montgomery County derived from DataQuick for the full sample (orange circles) and the analysis sample (blue squares). In Panel A., the dashed grey line was taken from Bhutta and Keys (2014) and constructed using nationally representative borrower-level data from the Equifax Consumer Credit Panel. In Panel B., the dashed grey line was constructed using data from Freddie Mac's Quarterly Cash-Out Refinance Report. The correlations between the DataQuick measures and the corresponding national aggregate measures in each panel are reported in the legend. The time span in Panel A. is shorter than that of Panel B. because Bhutta and Keys (2014) only report their measure for the period 1999–2010.

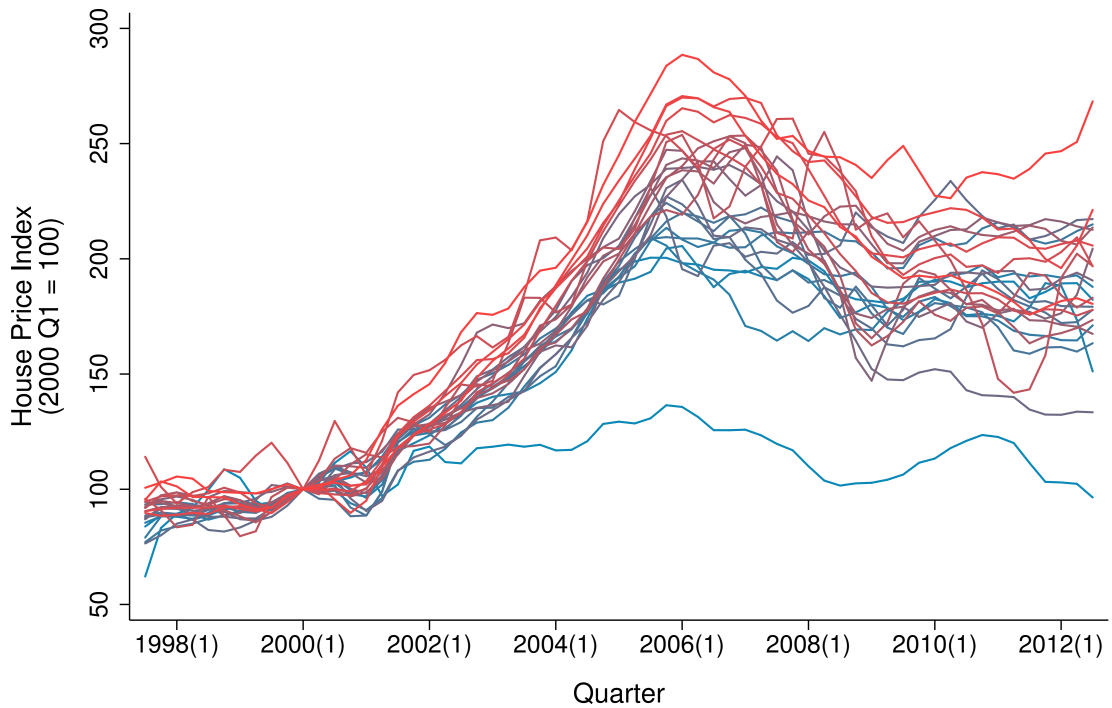
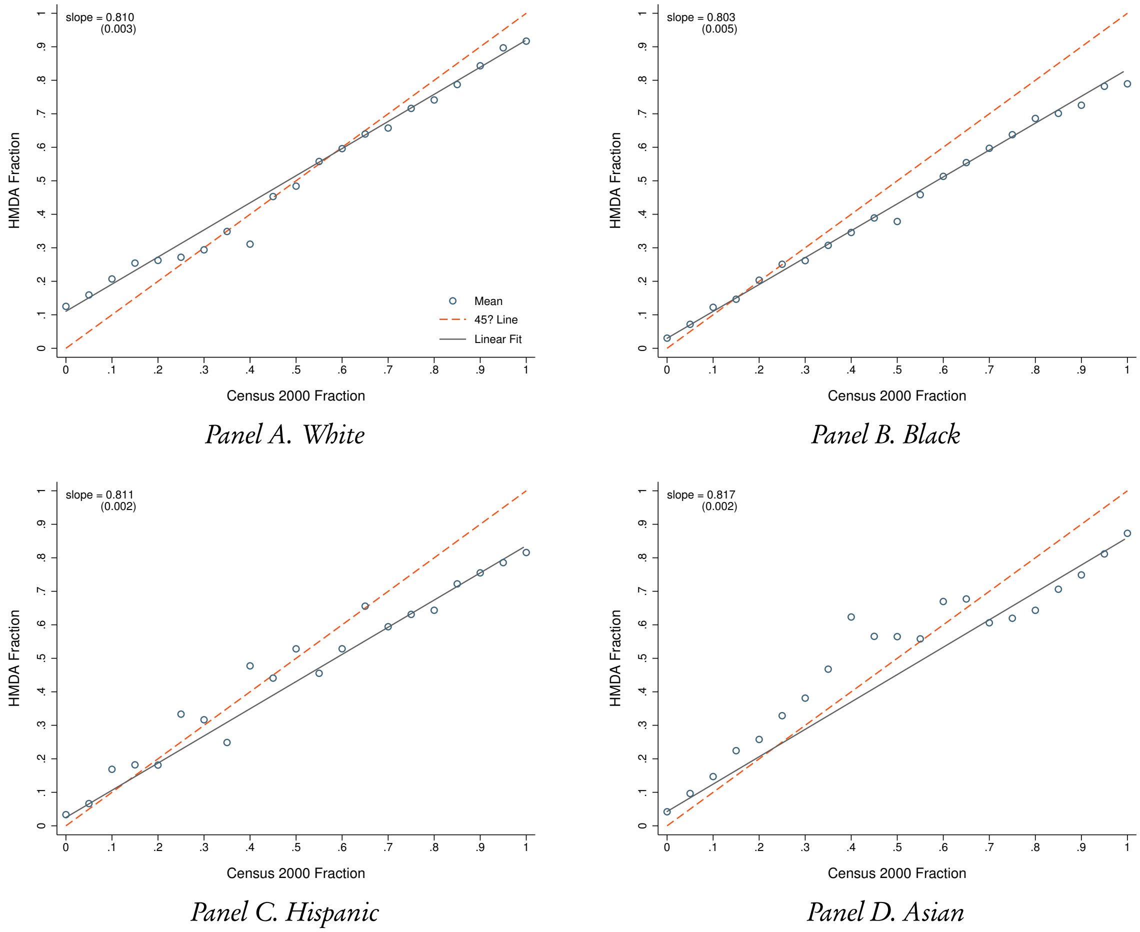


FIGURE B.3
Quarterly Hedonic House Price Indices for Montgomery County Planning Areas

NOTE.—This figure plots quarterly constant-quality hedonic price indices over the period 1997–2012 for each of the 28 local planning areas designated by the Montgomery County Planning Department. Each index was generated from the predicted values of a regression of (log) transaction price on a series of property characteristics, seasonal dummies, and planning area by quarter fixed effects as described in Appendix B.4. All series are normalized to 100 in the first quarter of 2000 and are shaded according to their maximum value obtained over the entire period.



Panel A. White

Panel B. Black

Panel C. Hispanic

Panel D. Asian

FIGURE B.4
Validating the DataQuick HMDA Match

NOTE.—This figure presents evidence validating the quality of the match between DataQuick housing transactions and HMDA loan applications. In each panel, the blue circles plot the fraction of matched transactions belonging to the indicated race as reported in HMDA on the y-axis against the fraction of households with the same surname as the home buyer who belong to that race as implied by the list of the 1,000 most popular surnames provided by the U.S. Census on the x-axis. The solid black line in each panel is the fit from a linear regression fit in the underlying microdata. The slope coefficient from that regression and its standard error are also reported in each panel. The dashed orange line is the 45-degree line.

C. Additional Results and Robustness Checks

C.1 The Effect of Expiring Price Controls on MPDU Turnover

One potential concern with implementing a difference-in-differences research design at the property level is that in addition to the increase in collateralized borrowing capacity, the expiration of the price control also creates incentives for MPDU owners to sell their homes, which could lead to a differential increase in turnover at MPDU properties. While this concern is explicitly addressed in the main analysis through the inclusion of property and ownership-spell fixed effects, it is nonetheless interesting to empirically gauge the magnitude of any changes in turnover induced by expiring price controls.

To do so, I construct an annual property-level panel which records for each property in the main analysis sample whether that property was sold in a given year. For properties built prior to 1997, the panel covers the full sample period from 1997–2012; for properties built afterwards, the construction year is used as the first year of observation. Using this panel, I then estimate regressions of the following form:

$$Sold_{ist} = \alpha_s + \delta_t + X_{it}'\gamma + \beta_1 \cdot MPDU_i + \beta_2 \cdot MPDU_i \times Post_{st} + \epsilon_{ist}, \qquad \text{(C.3)}$$

where $Sold_{ist}$ is an indicator for whether property i in subdivision s was sold in year t, and all other variables are as described in Section IV in reference to equation (10). The coefficient of interest is β_2, which measures the differential change in the turnover rate for MPDUs relative to non-MPDUs following the expiration of the price control and holding constant individual housing characteristics and aggregate differences in turnover rates across subdivisions and over time.

Table C.1 presents results from estimating this regression using various specifications. In the first column, I include only time-invariant property characteristics and fixed effects for both the year of observation and the age of the property in that year. The property characteristics include a quadratic in the interior square footage of the home, dummies for the number of bathrooms and the number of stories, and an indicator for whether the property is a condo or townhome as well as the interaction of that indicator with the year fixed effects and all of the other property characteristics including property age. In the second column I also include fixed effects for the subdivision the property is located in. In column 3, I further interact the subdivision fixed effects with a linear time trend to allow for differential aggregate trends in turnover across subdivisions. In the fourth column, I include property fixed effects, causing the time-invariant property characteristics and the MPDU main effect to drop out. In this specification, the effect of expiring price controls is identified by comparing within-property changes in turnover probabilities for properties that are and are not MPDUs. Finally, in columns 5 and 6, I dispense with the linear probability model and report probit and logit marginal effects using the same specification as in column 3.

The estimated effects are relatively stable across specifications and imply that expiring price controls lead to an increase in the annual turnover rate at MPDU properties of roughly three to five percentage points. These effects are large relative to the pre-period average annual turnover rate of 4.8% among MPDUs reported in the bottom panel of the table. Comparing the MPDU main effect with the interaction term shows that expiring price controls close between 50 to 80% of the gap in turnover rates between MPDUs and non-MPDUs that existed during the period of price control. However, the estimates are small in absolute terms, reflecting the fact that turnover is a relatively rare event. For example, adding the estimated three percentage point increase in column six to the pre-period mean turnover rate among MPDUs implies an annual post-expiration turnover rate of 7.8%. At that rate, it would take almost 13 years for the entire MPDU housing stock to turnover. In fact, over 80% of the MPDU owners living in the home in the year before the price control expired still lived there as of the end of the sample period.

To give a sense of the dynamics of the turnover effect, Figure C.1 plots estimates from a version of equation (C.3) that allows the effect of the price control to differ separately for MPDUs and non-MPDUs by year relative to the first control period expiration (as described in the discussion of equation (11) in Section IV). Specifically, the series in orange squares plots the coefficient estimates on a set of dummies indicating whether the year of observation falls in a given *relative* year as measured from the year the first MPDU in the relevant subdivision expired (relative year zero). This series measures the trend in the turnover rate for non-MPDU properties around the time the price control expired. Relative year −1 is the omitted category so that all estimates should be interpreted as relative to the year prior to when the first price control in the subdivision expired. Similarly, the series in blue circles shows the trend for MPDU properties. This line plots the sum of the relative year main effects (the series in orange squares) and the interaction of those effects with an indicator for whether the property is an MPDU. The figure also reports the 95% confidence interval for that sum. All controls included in column 3 of Table C.1 were also included in the regression. As the figure makes clear, the turnover rate at MPDU properties exhibits a sharp departure from it's pre-period trend precisely in the year the first price control expires while there is no corresponding change for non-MPDU properties. Moreover, the trends for MPDUs and non-MPDUs are statistically indistinguishable in the period prior to the expiration of the price control and only diverge beginning in the year of expiration.

While the effect of the expiring price control on turnover at MPDU properties is relatively small, it could still potentially affect the estimates if the owners who choose to stay in the home for many years post-expiration are drastically different from those who sell sometime shortly thereafter. I directly explore this possibility in Table C.2 by comparing the characteristics of those who sell and those who do not. To do so, I restrict attention to the set of MPDU ownership spells that began prior to the expiration and that either lasted until the end of the sample or ended with a transaction sometime following the expiration. Using this sample of ownership spells, I then run regressions of a given property or ownership spell characteristic (e.g. square footage, initial LTV, etc.) on a constant and a dummy for whether the ownership spell ended in a transaction that occurred after the expiration but before the end of the sample. The coefficient on this dummy variable for whether the owner sold after the expiration reveals whether there are significant differences in the characteristics of "stayers" versus "sellers."

As can be seen from the second row of Table C.2, there are essentially no differences between the homes or characteristics of those who sell post-expiration and those who do not. The homes of those who sell have nearly identical square footage, number of bathrooms, and number of stories and are only modestly newer than the homes of those who remain in the house until the end of the sample (columns 1–4). Those homes were also purchased for nearly identical prices, at the same initial LTV, and by households with similar incomes (columns 5–6).[11] This suggests that there is limited scope for differences in the characteristics of those who sell post-expiration and those who do not to affect the magnitude of my estimates.

Finally, to provide further evidence that the estimates are not affected by changes in the composition of the sample resulting from home sales, I also conduct a simple bounding exercise. To do so, I first identify the set of all MPDU owners who bought their homes prior to the expiration and sold afterwards. For each of these owners, I then extend their ownership spell to instead last all the way through the end of the sample and set both the equity extraction indicator and the total amount of equity extracted per year to zero in these extra years. Using this extended sample, I then re-estimate the borrowing responses reported in Table III and Table IV. By introducing a disproportionate number of zeros for MPDU owners in periods of time following the expiration, this exercise places an upper bound on the extent to which my estimates could potentially be inflated by the fact that some MPDU owners who would not have extracted equity had they stayed in the home exited the sample early.

Table C.3 and Table C.4 report the results from this exercise for the extensive margin probability of extracting equity and the total amount of equity extracted per year, respectively. In all cases, the estimates are only slightly smaller and are statistically indistinguishable from their analogs in Table III and Table IV. This suggests that turnover among MPDU owners following the price control expiration was simply not a large enough phenomenon to affect the estimates,

[11]The loss of observations moving from column 4 to column 5 is a result of having to drop ownership spells that began prior to the beginning of my sample since I do not observe the initial transaction price or loan amount for these ownership spells. The loss of observations moving from column 6 to column 7 reflects the fact that some transactions cannot be matched to a HMDA loan application.

even under the extreme assumption that all owners who left the sample as a result of selling their homes would not have extracted equity in any future years had they stayed.

C.2 Propensity Score Matching Estimates of the Borrowing Response

Another potential concern with the main difference-in-differences estimates provided in Section V is that they rely on standard OLS estimation, which can be sensitive to differences in the distribution of covariates across treatment and control groups and relies heavily on extrapolation in areas where the covariates do not overlap (Imbens, 2004). In this section, I explore the sensitivity of the main results to an alternative estimation approach which restricts attention to the set of properties with overlapping characteristics and constructs the counterfactual outcome for each MPDU property using a locally weighted average of the outcomes among the non-MPDU properties whose characteristics are most similar. Specifically, I provide estimates based on the local linear propensity score matching difference-in-differences estimator developed in Heckman, Ichimura, and Todd (1997) and Heckman et al. (1998).

Adopting the notation in Smith and Todd (2005), let t' and t denote the time periods before and after the expiration of the first price control within a property's subdivision. Let Y_{1ti} denote the observed outcome for property i if it receives the "treatment" in period t, where here the treatment is defined as being an MPDU in the post-period. Similarly, let Y_{0ti} denote the outcome for property i without treatment. Further, let the dummy variable $D_i = 1$ if a property is an MPDU and $D_i = 0$ if a property is not an MPDU. Given a vector of fixed property characteristics, X_i, the propensity score is defined as the conditional probability that a property is an MPDU, $P(X_i) = \Pr(D_i = 1|X_i)$. A property is said to be in the region of common support, S_P, if its propensity score has positive density in both the MPDU and non-MPDU distributions of propensity scores: $S_P = \{P : f(P|D = 1) > 0 \text{ and } f(P|D = 0) > 0\}$.

Having established this notation, the propensity score matching difference-in-differences estimator can be expressed as:

$$\hat{\Delta}^{DID}_{D=1} = \frac{1}{n_{1t}} \sum_{i \in I_{1t} \cap S_P}^{n_{1t}} \left\{ Y_{1ti}(X_i) - \hat{E}\left(Y_{0ti}|P(X_i), D_i = 0\right) \right\} - \frac{1}{n_{1t'}} \sum_{j \in I_{1t'} \cap S_P}^{n_{1t'}} \left\{ Y_{0t'j}(X_j) - \hat{E}\left(Y_{0t'j}|P(X_j), D_j = 0\right) \right\}, \tag{C.4}$$

where $I_{1t'}$ and I_{1t} denote the set of MPDU properties with outcomes observed in the pre- and post-periods, respectively, and n_{1t} and $n_{1t'}$ are the number of observations for MPDU properties in those two sets that are also in the region of common support. Implementing this estimator requires determining the region of common support and estimating the two expectations $\hat{E}(Y_{0ti}|P(X_i), D_i = 0)$ and $\hat{E}\left(Y_{0t'j}|P(X_j), D_j = 0\right)$, which serve as the counterfactual outcomes for MPDU properties in the two periods.

Both the determination of the region of common support and the estimation of the counterfactual outcomes depend on the propensity score, which I estimate using a simple probit model. Specifically, I take the estimated propensity score for property i to be the fitted values from a probit regression of the MPDU dummy on a set of property characteristics which includes the interior square footage of the home, the year it was built, the number of bathrooms and stories in the home and an indicator for whether the property is a condo or townhome. In order to get an accurate prediction of the propensity score, these covariates are entered into the model in a highly flexible fashion. I include cubic splines in both the square footage and year built as well as the linear interaction of both variables with a fully interacted set of dummies for the number of bathrooms and the number of stories. All of these terms are then further interacted with the condo dummy.

Having estimated the propensity score for each property, I then define the region of common support as the set of all propensity scores that are larger than the maximum of the first percentile in the distribution of propensity scores in both sets of properties and smaller than the minimum of the 99th percentile of propensity scores in both distributions. Figure C.2 plots the distribution of propensity scores for properties that fall within the region of common support separately for MPDUs and non-MPDUs. Dropping properties outside the region of common support leaves a total of 1,836 MPDU properties and 8,443 non-MPDU properties. Table C.5 provides an assessment of how well the estimated propensity score does in balancing covariates across these properties. The table shows the means of the covariates used to estimate the propensity score separately for MPDUs and non-MPDUs within terciles of the combined propensity score distribution. For each set of means, I also report the t-statistic from the test of the null hypothesis of no difference in means. While the propensity score does not do a perfect job of balancing the covariates, as is evidenced by the statistically significant differences for several of the variables, in nearly all cases, the differences in means are not economically meaningful and are far less stark than the differences in the full sample shown in columns 5 and 7 of Table I.

I use a local linear regression estimator to construct the matched counterfactual outcomes for each observed MPDU outcome. Implementing this estimator is relatively straightforward, and the full details can be found in Todd (1999). I focus the discussion here on how I construct the counterfactual outcome for observed MPDU outcomes in the post-period, Y_{1ti}. The process for constructing counterfactual outcomes for the pre-period outcomes, Y_{0ti}, is completely analogous. To construct the matched outcome for a particular MPDU property, i, observed in the post-period, I first match the post-period outcome for that property to all observed post-period outcomes among non-MPDU properties. I then calculate the difference in propensity scores between the MPDU property and each of the matched non-MPDU properties. For a particular non-MPDU property, j, denote this difference as $P(X_i) - P(X_j)$. I then run a weighted least squares regression of the outcomes for the matched non-MPDU properties on a constant and a linear term in this difference. I weight each observation according to the difference in propensity scores using a quartic kernel function and an bandwidth of 0.1. This means that outcomes among non-MPDUs whose propensity scores are more than 0.1 away from the propensity score for MPDU i receive no weight in the regression while those with identical propensity scores receive a weight that is close to 1. The estimated counterfactual outcome, $\hat{E}(Y_{0ti}|P(X_i), D_i = 0)$, is given by the constant from this this regression. This process is then repeated for all observed MPDU outcomes in both periods in order to obtain all of the matched outcomes.

With the matched outcomes in hand, I can then directly calculate the propensity score matching difference-in-differences estimate given by equation (C.4). To calculate the standard error for this estimate, I bootstrap the entire process using a stratified resampling procedure that randomly samples properties with replacement in a way that ensures that the number of properties sampled from each subdivision stays the same. Table C.6 reports the matching estimates for each of the three main outcomes—log transaction prices, the annual probability of extracting equity, and the total amount of equity extracted per year. The estimated effect for all three outcomes is positive and precisely estimated. The equity extraction estimates are almost identical to the main OLS difference-in-differences estimates reported in Table III and Table IV. Similarly, the price effect is slightly larger but qualitatively similar to the main estimates reported in Table II. Together, these results suggest that the main estimates are not being greatly affected by the fact that OLS relies on extrapolation in regions of the covariate space with poor overlap.

C.3 Alternative Specifications

The main results presented in Section V identify the effect of the expiring price control by comparing changes in outcomes for MPDU properties relative to non-MPDU properties before and after the first MPDU expiration date within a given property's subdivision. In this section, I explore alternative ways of estimating the effect of the expiring price control that leverage the exact timing of an individual property's price control expiration and which do not rely on using the outcomes of non-MPDU properties as a counterfactual.

Specifically, Table C.7 presents coefficient estimates from various versions of the following regression for each of the three main outcomes—log transaction prices, the annual probability of extracting equity, and the total amount of equity extracted per year:

$$y_{ist} = \eta_i + \delta_t + \alpha_{st} + X_{it}'\gamma + \beta \cdot Treat_{it} + \epsilon_{ist}. \tag{C.5}$$

In this specification, the "treatment" dummy, $Treat_{it}$, is defined at the level of the individual property rather than the subdivision. For MPDU properties it takes the value one in years following the expiration of that property's price control and zero otherwise. For non-MPDU properties the treatment dummy does not vary over time and is set equal to one in all years. Since the specification also includes property fixed effects, η_i, this means that the coefficient of interest, β, will be identified using only within-property variation for MPDU properties. To control for aggregate time-series trends the specification also includes calendar year fixed effects, δ_t. Moreover, since there is some within-subdivision variation in the timing of when MPDU price controls expire, in some specifications I am also able to control for within subdivision trends by including a full set of subdivision-by-year fixed effects, α_{st}. Finally, some specifications also control for time varying property characteristics, X_{it}, by including a full set of property age fixed effects.

The first three columns of Table C.7 present the coefficient estimates on the treatment dummy from regressions that were estimated in the full analysis sample used to generate the main results in Section V. Results are presented separately for the effect of the treatment on (log) transaction price (Panel A.), the annual probability of extracting equity (Panel B.), and the total amount of equity extracted per year (Panel C.). Each column contains a different set of controls, moving from a parsimonious specification in column 1 that contains only property and year fixed effects to a more fully saturated specification in column 3, which contains property, year, age, and subdivision-by-year fixed effects. Comparing the results from the most flexible specification in column 3 to their closest analogs from column 4 of Tables II, III, and IV, it is clear that this specification yields nearly identical conclusions as the main results. The coefficients in Table C.7 are only slightly smaller and are statistically indistinguishable from their counterparts reported in the main analysis.[12] The second row in the bottom portion of Panel C. also reports the implied marginal propensity to borrow (MPB) given these estimates. In all three specifications the MPB is well within the \$0.04–\$0.13 range reported in the main text.

Columns 4–6 report similar results estimated using a sample that contains only MPDU properties. While dropping non-MPDU properties leads to a loss of statistical power, the qualitative conclusion from these results remain the same. The coefficient estimates in these regressions are relatively smaller than their analogs from column 4 of Tables II, III, and IV. However, the implied MPBs in all three specifications are not materially different from the range reported in the main results and are in fact *larger* than that range in two of the three specifications.

[12] While economically unimportant, the fact that the coefficients are marginally smaller is somewhat surprising given the potential for downward bias in the main estimates that results from assigning all MPDU properties in a subdivision the same expiration date. However, given that most MPDU properties in a subdivision expire around the same time, it is not clear how large a role this bias should play relative to other factors that may cause these estimates to differ.

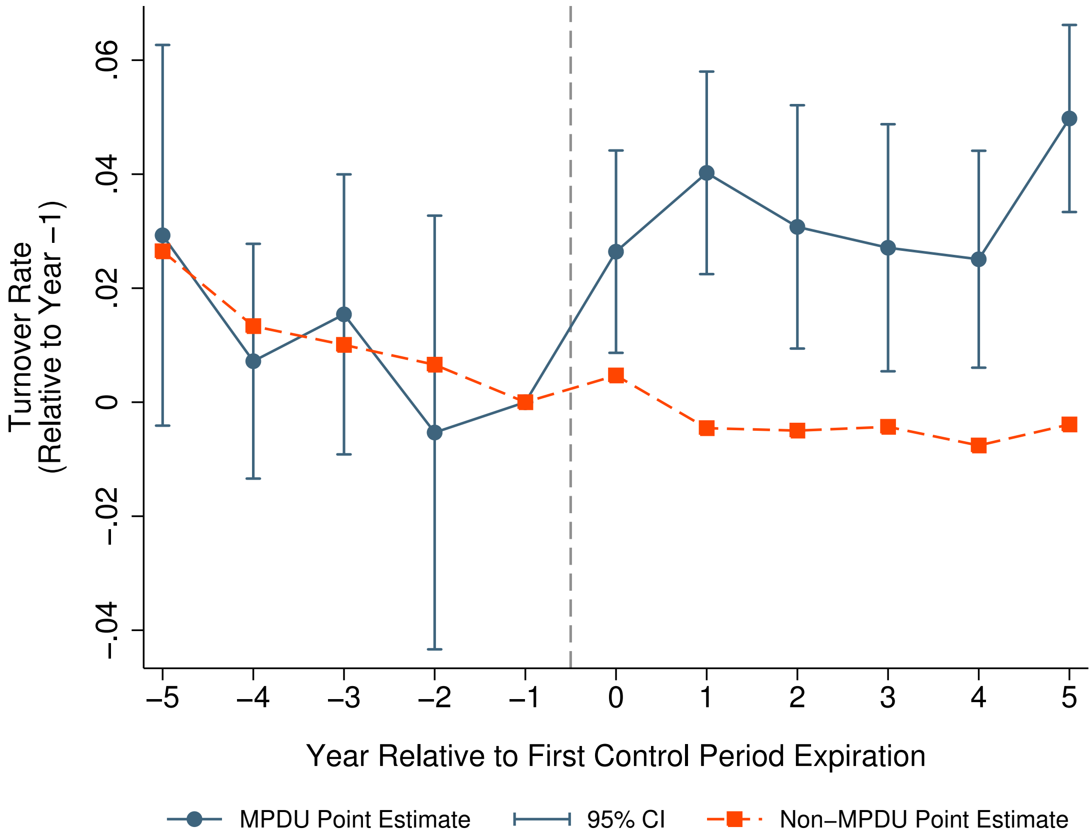


FIGURE C.1

Dynamic Effects of Expiring Price Controls on Turnover at MPDU Properties

NOTE.—This figure reports estimates of the effect of expiring price controls on the annual turnover rate at MPDU properties derived from a flexible difference-in-differences regression that allows the effect to vary by year relative to the expiration of the price control. Estimates were constructed by regressing an indicator for whether a given property sold in a particular year on an indicator for whether that property is an MPDU and the interaction of the MPDU indicator with a series of dummy variables indicating whether the year of observation falls in a given relative year as measured from the year the first MPDU in the relevant subdivision expired. Relative year zero denotes the year the first price control in the subdivision expired. Relative year −1 is the omitted category so that all estimates should be interpreted as relative to the year prior to expiration. Results are shown for five years preceding and following the expiration of the price control, with all years outside that window grouped into the effects for relative years −5 and 5. The series in orange squares plots the coefficient estimates on the relative year main effects, which represent the trend in turnover rates among non-MPDU properties. The series in blue circles plots the estimate and 95% confidence interval for the sum of the relative year main effects and the interaction of those effects with the MPDU indicator, representing the trend among MPDU properties. The 95% confidence intervals are based on standard errors which were clustered at the subdivision level. The regression also included year fixed effects, subdivision fixed effects and their interaction with a linear time trend and a set of property characteristics. The property characteristics include a quadratic in the interior square footage of the home, dummies for the number of bathrooms, stories, and property age, as well as an indicator for whether the property is a condo or townhome and the interaction of that indicator with the year fixed effects and all of the other property characteristics.

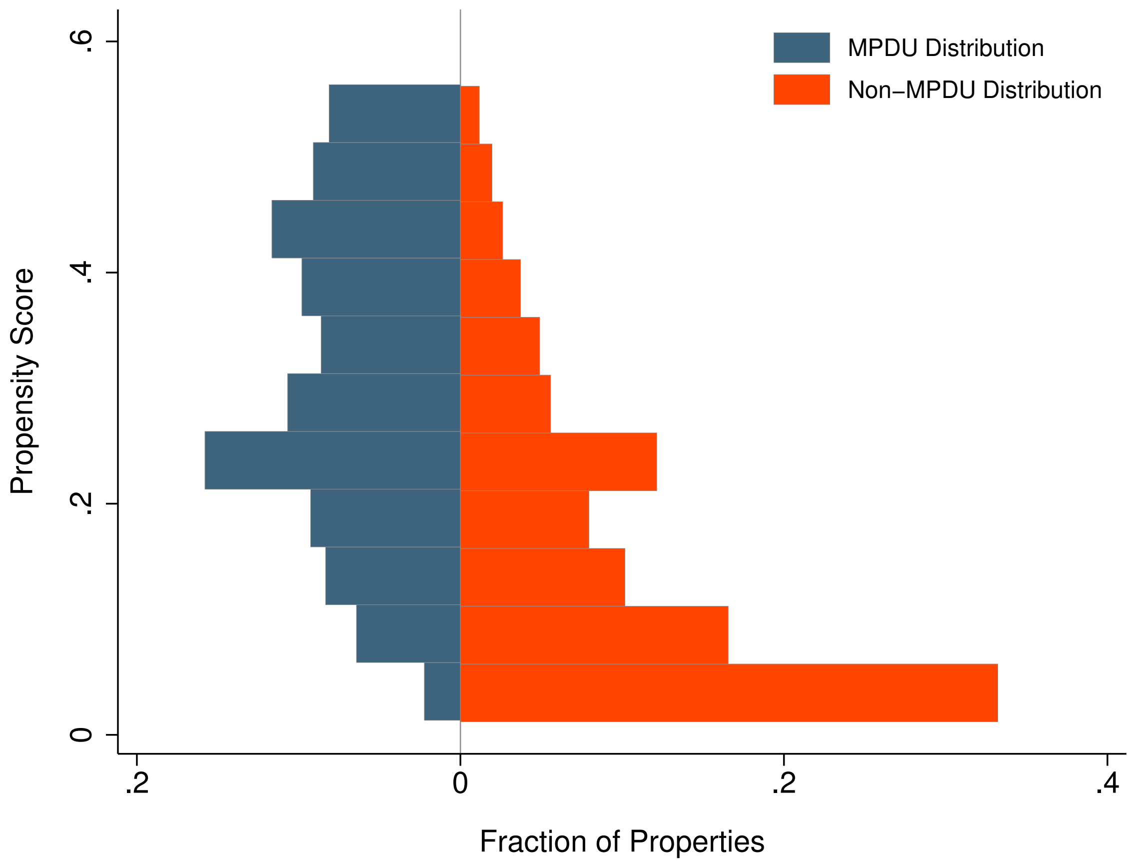


FIGURE C.2
Propsensity Score Overlap

NOTE.—This figure shows the overlap in the distribution of estimated propensity scores for MPDU and non-MPDU properties in the region of common support. The propensity score was estimated using a simple probit regression of the MPDU dummy on a set of property characteristics that included the interior square footage of the home, the year it was built, the number of bathrooms, the number of stories and an indicator for whether the property is a condo or townhouse. These covariates were entered in a flexible fashion that included cubic splines in square footage and year built as well as the linear interaction of both of those variables with a fully interacted set of dummies for the number of bathrooms and the number of stories. All of these terms were then further interacted with the condo dummy. The region of common support is defined as the set of all propensity scores that are larger than the maximum of the first percentile in the distribution of propensity scores in both sets of properties and smaller than the minimum of the 99th percentile of propensity scores in both distributions.

TABLE C.1

THE EFFECT OF EXPIRING PRICE CONTROLS ON TURNOVER AT MPDU PROPERTIES

	OLS				Probit	Logit
	(1)	(2)	(3)	(4)	(5)	(6)
MPDU	-0.072***	-0.064***	-0.063***		-0.057***	-0.055***
	(0.007)	(0.007)	(0.008)		(0.007)	(0.008)
MPDU $\times$ Post	0.050***	0.049***	0.049***	0.032***	0.034***	0.030***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)
Property Characteristics	X	X	X		X	X
Year and Age FEs	X	X	X	X	X	X
Subdivision FEs		X	X		X	X
Subdivision Trend			X	X	X	X
Property FEs				X		
Pre-Expiration MPDU Mean	0.048	0.048	0.048	0.048	0.048	0.048
Number of Observations	483,805	483,805	483,805	483,805	483,805	483,805

NOTE.—This table reports difference-in-differences estimates of the effect of expiring MPDU price controls on the annual turnover rate at MPDU properties. Each column reports a separate regression estimated at the property-year level where the dependent variable is an indicator for whether the property sold in a particular year. Coefficients are reported for the "treatment" dummy, denoting whether the property is an MPDU, and the interaction of that dummy with an indicator for whether the year of observation falls on or after the year the first price control within the relevant subdivision expired. All specifications include fixed effects for both the year of observation and the age of the property in that year as well as the Post main effect. The property characteristics include a quadratic in the interior square footage of the home, dummies for the number of bathrooms and the number of stories, and an indicator for whether the property is a condo or townhome as well as the interaction of that indicator with the year fixed effects and all of the other property characteristics including property age. Subdivision trends are estimated by interacting the subdivision fixed effects with a linear time trend. Columns 1–4 report coefficient estimates from linear probability models, while columns 5–6 report marginal effects from probit and logit specifications. The mean of the dependent variable among MPDU properties in the period prior to the first price control expiration is reported in the second to last row. Standard errors are reported in parentheses and are clustered at the subdivision level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

TABLE C.2

COMPARISON OF MPDU OWNERSHIP SPELLS THAT END IN SALE FOLLOWING THE EXPIRATION OF THE PRICE CONTROL WITH THOSE THAT DO NOT

	Property Characteristics				Transaction/Owner Characteristics		
	(1) Square Footage	(2) Number of Bathrooms	(3) Number of Stories	(4) Year Built	(5) Price ($1000's)	(6) Initial LTV	(7) Income ($1000's)
Constant	1221.28***	2.28***	1.94***	1995.92***	164.35***	0.84***	74.93***
	(5.87)	(0.02)	(0.02)	(0.13)	(14.12)	(0.03)	(5.27)
Sold Post Expiration	16.69	-0.04	0.04	-2.36***	-23.96	-0.04	6.42
	(14.38)	(0.06)	(0.04)	(0.32)	(47.41)	(0.09)	(17.58)
Number of Observations	1,271	1,271	1,271	1,271	293	293	200

NOTE.—This table reports estimates of the difference in the characteristics of the properties and ownership spells associated with MPDU owners who sell their homes following the expiration of the price control and those that remain in the home until the end of the sample. The level of observation is the ownership spell and each column reports the estimates from a regression of the indicated property or transaction/owner characteristic on a constant and a dummy for whether the ownership spell ended with a transaction that occurred after the price control expiration but before the end of the sample. In columns 1–4, the sample is all ownership spells of MPDU properties that began prior to the expiration of the price control and either lasted until the end of the sample or ended with a transaction sometime following the expiration. In columns 5–6, the sample is the subset of ownership spells contained in columns 1–4 that also began with a transaction that occurred during my sample period. In column 7, the sample is the subset of ownership spells in column 6 for which the initial transaction was able to be matched to a HMDA loan application.

TABLE C.3
BOUNDING EXERCISE FOR THE EFFECT OF TURNOVER ON THE ESTIMATES OF THE EXTENSIVE MARGIN BORROWING RESPONSE TO THE EXPIRING PRICE CONTROL

	OLS				Probit	Logit
	(1)	(2)	(3)	(4)	(5)	(6)
MPDU	-0.057***	-0.050***	-0.050***		-0.053***	-0.054***
	(0.007)	(0.008)	(0.008)		(0.006)	(0.006)
MPDU $\times$ Post	0.039***	0.037***	0.038***	0.028***	0.041***	0.040***
	(0.008)	(0.008)	(0.008)	(0.009)	(0.007)	(0.007)
Property Characteristics	X	X	X		X	X
Year and Age FEs	X	X	X	X	X	X
Subdivision FEs		X	X		X	X
Subdivision Trend			X	X	X	X
Ownership Spell FEs				X		
Pre-Expiration MPDU Mean	0.039	0.039	0.039	0.039	0.039	0.039
Number of Observations	485,372	485,372	485,372	485,372	485,372	485,372

NOTE.—This table reports difference-in-differences estimates of the effect of expiring MPDU price controls on the annual probability of extracting equity among owners of MPDU properties assuming that MPDU owners who bought their homes prior to expiration and sold afterwards instead remained in the house all the way to the end of the sample and did not extract equity in any of the years following the sale. Each column reports a separate regression estimated at the property-year level where the dependent variable is an indicator for whether the property owner extracted equity from the home in a particular year. Coefficients are reported for the "treatment" dummy, denoting whether the property is an MPDU, and the interaction of that dummy with an indicator for whether the year of observation falls on or after the year the first price control within the relevant subdivision expired. All specifications include fixed effects for both the year of observation and the age of the property in that year as well as the Post main effect. The property characteristics include a quadratic in the interior square footage of the home, dummies for the number of bathrooms and the number of stories, and an indicator for whether the property is a condo or townhome as well as the interaction of that indicator with the year fixed effects and all of the other property characteristics including property age. Subdivision trends are estimated by interacting the subdivision fixed effects with a linear time trend. Columns 1–4 report coefficient estimates from linear probability models, while columns 5–6 report marginal effects from probit and logit specifications. The mean of the dependent variable among MPDU properties in the period prior to the first price control expiration is reported in the second to last row. Standard errors are reported in parentheses and are clustered at the subdivision level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

TABLE C.4

BOUNDING EXERCISE FOR THE EFFECT OF TURNOVER ON THE ESTIMATES OF THE TOTAL BORROWING RESPONSE (IN $1,000S) TO THE EXPIRING PRICE CONTROL

	OLS				Tobit
	(1)	(2)	(3)	(4)	(5)
MPDU	-2.557***	-3.491***	-3.582***		-5.920***
	(0.755)	(0.667)	(0.696)		(0.633)
MPDU × Post	2.149***	2.745***	2.854***	2.056**	4.153***
	(0.730)	(0.683)	(0.720)	(0.918)	(0.665)
Property Characteristics	X	X	X		X
Year and Age FEs	X	X	X	X	X
Subdivision FEs		X	X		X
Subdivision Trend			X	X	X
Ownership Spell FEs				X	
Pre-Expiration MPDU Mean	2.662	2.662	2.662	2.662	2.662
Number of Observations	485,372	485,372	485,372	485,372	485,372

NOTE.—This table reports difference-in-differences estimates of the effect of expiring MPDU price controls on the annual amount of equity extracted among owners of MPDU properties assuming that MPDU owners who bought their homes prior to expiration and sold afterwards instead remained in the house all the way to the end of the sample and did not extract equity in any of the years following the sale. Each column reports a separate regression estimated at the property-year level where the dependent variable is the amount of equity (in $1,000s) that the property owner extracted from the home in a particular year. Coefficients are reported for the "treatment" dummy, denoting whether the property is an MPDU, and the interaction of that dummy with an indicator for whether the year of observation falls on or after the year the first price control within the relevant subdivision expired. All specifications include fixed effects for both the year of observation and the age of the property in that year as well as the Post main effect. The property characteristics include a quadratic in the interior square footage of the home, dummies for the number of bathrooms and the number of stories, and an indicator for whether the property is a condo or townhome as well as the interaction of that indicator with the year fixed effects and all of the other property characteristics including property age. Subdivision trends are estimated by interacting the subdivision fixed effects with a linear time trend. Columns 1–4 report coefficient estimates from OLS regressions, while column 5 reports the marginal effects for the expected amount of equity extraction (censored and uncensored, treating censored as zero) from a tobit specification. The mean of the dependent variable among MPDU properties in the period prior to the first price control expiration is reported in the second to last row. Standard errors are reported in parentheses and are clustered at the subdivision level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

TABLE C.5

COVARIATE BALANCE WITHIN TERCILES OF THE PROPENSITY SCORE DISTRIBUTION

	First P-Score Tercile			Second P-Score Tercile			Third P-Score Tercile		
	Non-MPDU	MPDU	*t-stat*	Non-MPDU	MPDU	*t-stat*	Non-MPDU	MPDU	*t-stat*
Fraction Condo	0.824	0.871	-1.164	0.774	0.843	-3.884***	0.916	0.924	-0.797
Square Footage (1000's)	1.333	1.249	1.977*	1.179	1.134	3.687***	1.159	1.141	2.715***
Number of Bathrooms	2.391	1.971	2.921***	2.124	2.187	-1.455	2.084	2.056	1.004
Number of Stories	1.690	1.557	1.691*	1.635	1.623	0.470	1.852	1.893	-2.369**
Age (Years)	24.207	23.186	1.315	24.033	23.832	0.802	23.562	23.400	1.065
Number of Observations			3,392			3,395			3,492

NOTE.—This table presents means of the covariates used to estimate the propensity score. Properties are grouped based on whether their propensity score falls in the bottom, middle, or top third of the combined distribution of propensity scores. Means are then calculated separately for MPDUs and non-MPDUs within these three terciles. For each set of means, the table also reports the t-statistic from a test of the null hypothesis of no difference in means between MPDUs and non-MPDUs. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

TABLE C.6

PROPENSITY SCORE MATCHING DIFFERENCE-IN-DIFFERENCES ESTIMATES

	Log Transaction Price	Probability of Extracting Equity	Amount Extracted ($1,000s)
	(1)	(2)	(3)
DID Matching Estimate	0.610***	0.035***	2.783***
	(0.060)	(0.013)	(0.686)
Number of Matched MPDUs	1,846	1,846	1,846
Number of Matched Non-MPDUs	8,443	8,443	8,443
Number of Bootstrap Replicates	100	100	100
Bandwidth	0.1	0.1	0.1

NOTE.—This table presents propensity score matching difference-in-differences estimates of the effect of expiring price controls on log transaction prices, the annual probability of equity extraction, and the total amount of equity extracted per year among MPDU properties and their owners. Estimates were constructed as described in Appendix C.2. Bootstrap standard errors are reported in parentheses. Significance levels 10%, 5%, and 1% are denoted by *, **, and *** and were determined based on the assumption that the bootstrap distribution is normally distributed.

TABLE C.7
ALTERNATIVE SPECIFICATIONS

	All Properties			MPDUs Only		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. Log Transaction Price					
Treatment Dummy	0.293***	0.298***	0.313***	0.218***	0.126*	0.132
	(0.043)	(0.044)	(0.046)	(0.041)	(0.067)	(0.122)
Implied %Δ	34%	35%	37%	24%	13%	13%
Implied $Δ (1,000s)	$53	$54	$57	$38	$21	$21
Number of Observations	30,209	30,209	30,209	2,275	2,275	2,275
	Panel B. Probability of Extracting Equity					
Treatment Dummy	0.018***	0.028***	0.028***	0.014***	0.012	0.021*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.009)	(0.012)
Number of Observations	483,805	483,805	483,805	45,828	45,828	45,828
	Panel C. Total Equity Extracted ($1,000s)					
Treatment Dummy	1.414**	2.576***	2.715***	1.023**	1.648*	1.678
	(0.584)	(0.609)	(0.627)	(0.417)	(0.858)	(1.165)
Number of Observations	483,805	483,805	483,805	45,828	45,828	45,828
Implied MPB	0.054	0.096	0.095	0.054	0.160	0.163
Property FEs	X	X	X	X	X	X
Year FEs	X	X	X	X	X	X
Age FEs		X	X		X	X
Subdivision × Year FEs			X			X

NOTE.—This table reports results from alternative specifications that leverage the exact timing of an individual property's price control expiration and which do not rely on using the outcomes of non-MPDU properties as a counterfactual for estimating the effect of the expiring price control. Results are reported separately for transaction prices (Panel A.), the annual probability of equity extraction (Panel B.), and the total amount of equity extracted per year (Panel C.). Columns 1–3 report estimates from regressions estimated in the entire sample where the "treatment" dummy is defined at the property level. In these regressions, the treatment dummy is always equal to one for non-MPDU properties. For MPDU properties it is equal to one in years when that property's price control has expired and zero otherwise. Columns 4–6 report analogous results from regressions estimated in a sample containing only MPDU properties. In these regressions, the treatment dummy is equal to one in years when the property's price control has expired and zero otherwise. The second row in the bottom portion of Panel C. reports the implied marginal propensity to borrow (MPB). The implied MPB is calculated by dividing the estimate in Panel C. by one-half of the estimate in Panel A. (i.e. the implied increase in collateralized borrowing capacity). Standard errors are reported in parentheses with significance levels 10%, 5%, and 1% denoted by *, **, and ***, respectively.

TABLE C.8
THE EFFECT OF EXPIRING PRICE CONTROLS ON
TRANSACTION PRICES AND EQUITY EXTRACTION IN BOOM AND BUST PERIODS

	Log Transaction Price		Total Equity Extracted ($1000's)	
	(1) 2002–2005	(2) 2006–2012	(3) 2002–2005	(4) 2006–2012
MPDU	-0.714***	-0.834***	-6.338***	-3.697***
	(0.052)	(0.031)	(1.250)	(1.110)
MPDU × Post	0.601***	0.740***	4.775***	3.510***
	(0.069)	(0.049)	(1.126)	(1.095)
Property Characteristics	X	X	X	X
Year and Age FEs	X	X	X	X
Subdivision FEs	X	X	X	X
Subdivision Trend	X	X	X	X
Implied MPB	–	–	0.072	0.040
Number of Observations	10,701	8,600	123,049	218,520

NOTE.—This table reports difference-in-differences estimates of the effect of expiring MPDU price controls on transaction prices for MPDU properties and the annual amount of equity extracted among owners of MPDU properties separately for boom and bust periods. Each column reports a separate regression where the dependent variable is either the log of the transaction price (columns 1 and 2) or the amount of equity (in $1,000s) that the property owner extracted from the home in a particular year (columns 3 and 4). In columns 1 and 2 the level of observation is the individual transaction and in columns 3 and 4 it is the property-year. Regressions are estimated separately during "boom" and "bust" periods by splitting the sample. Columns 1 and 3 include only transactions or property-years between 2002–2005 and columns 2 and 4 limit to transactions and property years between 2006–2012. Coefficients are reported for the "treatment" dummy, denoting whether the property is an MPDU, and the interaction of that dummy with an indicator for whether the year of observation falls on or after the year the first price control within the relevant subdivision expired. All specifications include fixed effects for both the year of observation and the age of the property in that year as well as the Post main effect. The property characteristics include a quadratic in the interior square footage of the home, dummies for the number of bathrooms and the number of stories, and an indicator for whether the property is a condo or townhome as well as the interaction of that indicator with the year fixed effects and all of the other property characteristics including property age. Subdivision trends are estimated by interacting the subdivision fixed effects with a linear time trend. The first row of the bottom panel reports the implied marginal propensity to borrow which is calculated by dividing the estimates in the second row of columns 3 and 4 by one half of the implied increase in transaction price associated with the corresponding estimates in columns 1 and 2. Standard errors are reported in parentheses and are clustered at the subdivision level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

Appendix References

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