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

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ABSTRACT

I empirically analyze how changes in access to housing collateral affect homeowner borrowing behavior. To isolate the role of collateral constraints from that of wealth effects, I exploit the fully anticipated expiration of resale price controls on owner-occupied housing in Montgomery County, Maryland. I estimate a marginal propensity to borrow out of housing collateral that ranges between $0.04 and $0.13 and is correlated with homeowners' initial leverage. Additional analysis of residential investment and ex-post loan performance indicates that some of the extracted funds generated new expenditures. These results suggest a potentially important role for collateral constraints in driving household expenditures.

LARGE FLUCTUATIONS IN HOUSEHOLD DEBT have become a common feature of business cycles worldwide (Jordà, Schularick, and Taylor (2014), Mian, Sufi, and Verner (2015)). Recent experience in the United States, which saw the ratio of household debt to GDP increase by over 40% from 2000 to 2008 before plummeting in the aftermath of the Great Recession, is no exception.[1] Understanding the microeconomic mechanisms driving these aggregate changes in

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[1] Between 2000 (Q1) and 2008 (Q1), the ratio of household debt to GDP increased from 67.7% to 97.6%. This ratio later bottomed out at 77.7% in 2015 (Q3). These figures were calculated using aggregate data on household debt from the Federal Reserve Flow of Funds (Z.1 Release, Series LA154104005.Q) and GDP data from National Income and Product Accounts of the United States (BEA Account Code: A191RC1).

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household debt is of first-order importance for guiding monetary, fiscal, and macroprudential policy.

A growing body of theoretical work has emphasized the importance of household borrowing constraints, linking changes in household debt, and economic activity over the business cycle to changes in households' access to credit.[2] Given that the vast majority of household debt is collateralized by real estate, much of this work has paid particular attention to the role of *collateral* constraints. However, direct causal evidence on the role that collateral constraints play for household borrowing is difficult to establish, and answers to key empirical questions have remained elusive. When provided with an additional dollar of collateralized borrowing capacity, how much do households choose to borrow, which households respond the most, and what do they spend the money on?

This paper sheds new light on these questions by studying how the borrowing behavior of individual homeowners responds to changes in their ability to access housing collateral. In a departure from prior work, which has focused primarily on how borrowing responds to changes in house prices, I study homeowner behavior in an empirical context in which the market value of housing is held constant.[3] This approach allows me to overcome two of the key challenges confronted by prior empirical research. First, disentangling the effect of collateral constraints from wealth effects in an environment of changing house prices can be difficult. While an increase in house prices may lead homeowners to take on additional debt, it is unclear whether this occurs because rising prices make homeowners feel richer or because rising prices relax previously binding collateral constraints. Second, even in the absence of wealth effects, drawing causal inferences from changes in house prices is challenging. Concerns over aggregate shocks to joint determinants of both house prices and homeowner borrowing, such as interest rates and expected future income, always loom large.[4] By holding the market value of housing constant and looking instead at a policy-induced change in homeowners' *access* to collateral, my approach is able to overcome both of these challenges.

[2] See, for example, Eggertsson and Krugman (2012), Guerrieri and Lorenzoni (2015), Justiniano, Primiceri, and Tambalotti (2015), Guerrieri and Iacoviello (2016), Korinek and Simsek (2016), and Philippon and Midrigan (2016).

[3] Recent papers studying the role of collateral constraints through the lens of changing house prices include Yamashita (2007), Disney and Gathergood (2011), Mian and Sufi (2011, 2014), Cooper (2013), and Bhutta and Keys (2014). Three important exceptions are Leth-Petersen (2010), Abdallah and Lastrapes (2012), and Agarwal and Qian (2014), who study explicit policy-induced changes in collateral constraints similar to the one studied in this paper. However, these studies rely on national- and state-level policy variation, which makes it difficult to separately identify aggregate trends from household-specific changes to collateral constraints.

[4] A frequently proposed solution to this problem is to instrument for local house prices using Saiz's (2010) estimates of cross-city variation in physical constraints to building (Mian and Sufi (2011, 2014), Aladangady (2013), Mian, Rao, and Sufi (2013)). However, as cautioned by Saiz (2010) and further emphasized by Davidoff (2013, 2014), physical supply constraints are highly correlated with a host of other demand factors that might be expected to directly affect both house prices and homeowner borrowing.

To isolate the effect of collateral constraints on household borrowing, I exploit a unique feature of local land use policy in Montgomery County, Maryland. Since 1974, housing developers in this county have been subject to an inclusionary zoning regulation known as the Moderately Priced Dwelling Unit (MPDU) program. This regulation requires developers to set aside at least 12.5% of all housing units in new developments to be made available at controlled prices to moderate-income households.[5] These housing units are subject to deed restrictions that cap their resale prices for a period of time ranging between 5 and 30 years. During this period, owners are not permitted to refinance or take on home equity debt for an amount that exceeds the controlled resale price. Once the price controls expire, however, owners are able to pledge the full market value of the home as collateral. Since the duration and stringency of the price controls are set by formula and known in advance at the time of purchase, their expiration has no effect on the owner's total expected lifetime wealth nor on the market value of their home at the time the price control is lifted. However, expiring price controls directly affect the owner's collateralized borrowing capacity through the relaxation of the borrowing restrictions. Therefore, differential changes in the propensity for MPDU homeowners to extract equity from their homes at the time the restriction is lifted contain explicit information regarding the effect of collateral constraints on homeowner borrowing. I use this information to provide new estimates of both the extensive margin effect of relaxing collateral constraints on home equity extraction and the marginal propensity to borrow against a $1 increase in collateralized borrowing capacity.

To conduct my analysis, I assemble a unique data set containing the precise geographic location and detailed structural characteristics of every housing unit in Montgomery County as well as the full history of transactions and loans secured against each property during the period 1997 to 2012. I combine this information with administrative records from the Montgomery County Department of Housing and Community Affairs (DHCA), which identify the restricted housing units and the dates for which the applicable price controls were in effect. This data set allows me to identify the effect of expiring price controls by comparing how the borrowing behavior of owners of controlled housing units changes following the expiration of the price control relative to that of owners of nearby and observationally identical never-controlled units. It also allows me to track the borrowing behavior of a given homeowner over time, permitting a within-ownership spell comparison of equity extraction before and after the expiration of the price control. The added degrees of freedom afforded by the fact that controlled units are dispersed relatively evenly throughout the county and expire at different points during the sample period further allow me to control flexibly for aggregate trends affecting borrowing behavior and for unobservable but fixed differences across localities within the county.

[5] For a four-person household, the maximum income limit is set at 70% of the median family income for the Washington, DC, metropolitan area. In 2014, that limit was $75,000, which is roughly 17% higher than the national median family income that year. In Section V.E. I document that the income distribution among MPDU buyers is quite similar to the distribution among all homebuyers in the United States during my sample period.

To estimate the implied increase in collateralized borrowing capacity generated by the expiring price control, I compare transaction prices for observationally identical controlled and noncontrolled homes in the same development before and after the expiration. Since the owners of controlled units are only able to borrow against the controlled value of the home while the price control is in effect, this difference in prices provides a rough measure of how much additional collateral is released at the time the borrowing restriction is lifted. The results suggest that the average MPDU owner experiences an increase in collateralized borrowing capacity of approximately $33,000 to $53,000 upon expiration. In response, I find compelling evidence that this increase in collateralized borrowing capacity leads homeowners to extract equity from their homes. In housing developments containing controlled units, owners of those units are roughly 4 percentage points more likely to extract equity from their homes in a given year after the expiration of the price control relative to owners of observationally identical noncontrolled units. This effect is large and is immediately present in the year the price control expires. It represents an increase of almost 100% relative to the preexpiration mean probability of equity extraction among owners of controlled units. As an alternative way to gauge the magnitude of these estimates, I use information on the size of individual loans to convert them into an estimate of the marginal propensity to borrow against an increase in housing collateral. On average, I find that a $1 increase in collateralized borrowing capacity leads homeowners to extract between $0.04 and $0.13 in additional home equity debt.

To provide additional evidence that collateral constraints are the dominant force leading owners of price-controlled units to extract equity from their homes following the expiration of the price control, I also investigate heterogeneity in the response across individuals. In particular, I show that homeowners with high initial leverage (as measured by their loan-to-value (LTV) ratio at the time of purchase) are far more likely to respond to expiring price controls by extracting equity than homeowners with low initial leverage. I find no statistically or economically significant effects for homeowners in the bottom portion of the initial leverage distribution ($LTV \leq 0.7$), whereas the effects for the most highly levered households ($LTV > 0.95$) are both statistically significant and roughly twice as large as the overall average effect. These results suggest that the increase in collateralized borrowing capacity induced by the expiring price controls only affects borrowing behavior among the subset of homeowners for whom collateral constraints were likely to have bound prior to expiration. Interestingly, I find no correlation between homeowner income and the magnitude of the borrowing response, which also provides some assurance that the effects I estimate are not being driven by the fact that owners of MPDU homes are differentially selected from the middle portion of the U.S. income distribution.

My empirical strategy identifies the effect of collateral constraints on equity extraction under the assumption that borrowing behavior would have evolved similarly for owners of both controlled and uncontrolled units in the absence of the expiring price control. To probe the validity of this assumption, I conduct a range of different robustness checks. Most importantly, I provide direct

graphical evidence showing that the trends in outcomes for controlled and uncontrolled units move together in the period prior to expiration and only begin to diverge once the price controls expire. To more formally assess the validity of the parallel trends assumption, I also conduct a series of placebo tests in which I randomly assign price control expiration dates to the controlled units and reestimate the main specifications. The results of this exercise suggest that the effects I find are unlikely to have been generated by spurious correlation alone. The estimated effects are also robust to the inclusion of both ownership spell fixed effects and subdivision-specific time trends, implying that any time-varying omitted factors driving the results must be present at both the level of the individual homeowner and the particular housing subdivision in which her home is located. Finally, to further address potential concerns regarding the comparability of controlled and never-controlled units, I also replicate the main analysis using a semiparametric propensity score matching estimator.

While my results provide clear evidence that homeowners respond to a relaxation of their housing collateral constraint by borrowing against their homes, the real effects of such borrowing depend on how the money is used. In particular, if homeowners simply reinvest the proceeds into more-liquid assets or use the funds to pay off other outstanding debt, then home equity-based borrowing induced by increases in collateralized borrowing capacity should not be expected to affect current consumption or investment expenditures. Although limitations of the data prevent me from being able to provide a full account of the uses of extracted funds, I provide two pieces of evidence suggesting that at least some fraction of the borrowed money was used to fund current expenditures. First, using administrative data on building and home improvement permits issued by the Montgomery County Department of Permitting Services, I find that the annual likelihood of applying for a home improvement permit increases differentially by roughly 0.6 to 1 percentage points among owners of price-controlled units following the expiration of the price control. This effect represents an increase of approximately 60% to 100% over the preexpiration mean and suggests that borrowers likely used some portion of the extracted equity to fund residential investment expenditures.[6] Second, the deeds data used to conduct the main analysis also contain information on home foreclosures. Using this information, I show that the three-year foreclosure rate associated with equity extractions secured against MPDU properties increases by roughly 1.5 to 2 percentage points relative to equity extractions secured against

[6] An alternative interpretation of this result is that the expiration of the price control increases the owner's incentives to invest in the home, as was documented by Autor, Palmer, and Pathak (2014) in the context of rent control, and may therefore explain both the increase in permitting activity and the increase in equity extraction even in the absence of any collateral effects. This is unlikely to be the case in my context because the formula used to determine the controlled price is adjusted upward dollar-for-dollar to reflect documented home improvements. As a result, the price control generates little disincentive for investment during the control period. Moreover, for owners who plan to stay in the home beyond the end of the control period, the expected return from home improvements is determined solely based on the market price. For these owners, the exact timing of the expiration therefore has no effect on investment incentives.

non-MPDU properties following the expiration of the price control. This result is consistent with previous findings regarding the increased risks associated with debt-financed home equity extraction and suggests that borrowers are unlikely to be reinvesting all of the proceeds into more-liquid assets, as their risk of foreclosure would presumably remain unchanged if that were the case (Mian and Sufi (2011), Bhutta and Keys (2014), Laufer (2014)).

My findings build on a large empirical literature studying various aspects of the relationship between housing and household consumption, savings, and borrowing behavior (recent examples include Hurst and Stafford (2004), Case, Quigley, and Shiller (2005, 2013), Campbell and Cocco (2007), Gan (2010), Leth-Petersen (2010), Carroll, Otsuka, and Slacalek (2011), Disney and Gathergood (2011), Mian and Sufi (2011, 2014), Abdallah and Lastrapes (2012), Carroll and Zhou (2012), Calormiris, Longhofer, and Miles (2013), Chen, Michaux, and Roussanov (2013), Cooper (2013), Mian, Rao, and Sufi (2013), Agarwal and Qian (2014), and Bhutta and Keys (2014)).[7] Many studies in this literature have attempted to infer the role that housing collateral constraints play for homeowner borrowing behavior by studying how borrowing responds to changes in house prices. To address the confounding role of wealth effects and concerns over omitted variables, most of these studies have tended to rely on indirect proxy measures of the stringency of the collateral constraint across different subpopulations of homeowners. For example, a common approach has been to explore how households with different credit histories and incomes or at different points in the life cycle respond to similar changes in house prices.[8] While the results from these analyses are generally suggestive of an important role for collateral constraints, the reliance on changes in house prices and the indirect nature of the proxy measures employed has left open the possibility that such estimates may be confounding differences in constraints with differences in preferences. I contribute to this literature by providing the first direct estimates of the collateral effect that leverage time-varying and household-specific changes in access to housing collateral while holding constant household wealth and the market value of housing.

This paper also relates to a much broader literature studying the role of collateral constraints in the macroeconomy. An important theoretical literature in macroeconomics emphasizes the role that collateral constraints can play in amplifying business cycle fluctuations (see, for example, Bernanke and Gertler (1989), Kiyotaki and Moore (1997), and Iacoviello (2005)). Given that real estate is such a large source of collateral for many households and businesses,

[7] Bostic, Gabriel, and Painter (2009) provide a useful review of the earlier literature in this area.

[8] This approach is motivated by similar strategies that have been used to study the role of liquidity constraints in the vast empirical literature estimating consumption and borrowing responses to various forms of income receipt (see, for example, Zeldes (1989), Jappelli (1990), Shapiro and Slemrod (1995), Jappelli, Pischke, and Souleles (1998), Parker (1999), Souleles (1999), Browning and Collado (2001), Johnson, Parker, and Souleles (2006), Agarwal, Liu, and Souleles (2007), Stephens (2008), Aaronson, Agarwal, and French (2012), Parker et al. (2013), Baker (2014), Zhang (2014), and many others as reviewed by Browning and Lusardi (1996), Browning and Crossley (2001), Jappelli and Pistaferri (2010), and Zinman (2015)).

a particular point of focus in the empirical literature studying the microeconomic foundations underlying this "financial accelerator" mechanism has been to examine how households and businesses respond to changes in their ability to borrow against real estate assets.[9] This paper contributes new empirical evidence on the household side by documenting a strong positive relationship between access to housing collateral and home equity-based borrowing.

The remainder of this paper is organized as follows. Section I provides institutional background on the Montgomery County MPDU program. Section II discusses how MPDU price control expirations can be used to identify collateral effects in the context of a stylized model of home equity extraction. The data sources and method used to measure equity extraction are discussed in Section III. Section IV outlines the empirical research design. Section V presents estimates of the effect of expiring price controls on borrowing behavior, and Section VI provides evidence on the uses of extracted funds. Section VII concludes.

I. The Moderately Priced Dwelling Unit Program

Established in 1974, the MPDU program in Montgomery County, Maryland, is one of the oldest and most well-known inclusionary zoning policies in the United States. Inclusionary zoning policies are local land use regulations that either require or incentivize housing developers to set aside a fraction of their new developments to be sold or rented to low- and moderate-income households at below-market prices. Historically, these policies have been particularly popular in high-cost suburban areas. However, in response to rising house prices and concerns over increasing spatial segregation on the basis of income, inclusionary zoning policies have grown in popularity over the last 15 to 20 years and now exist in roughly 500 municipalities across 27 states, including several large urban centers such as New York, San Francisco, Washington, DC, and Chicago (Hickey, Sturtevant, and Thaden (2014)).

A. Developer Requirements

The MPDU program requires that any developer wishing to build a residential development within the county containing more than 20 housing units

[9] For example, several recent papers have provided empirical evidence on the firm side by documenting a sizable effect of real estate prices on corporate investment, capital structure, and credit terms (Benmelech, Garmaise, and Moskowitz (2005), Gan (2007), Chaney, Sraer, and Thesmar (2012), Cvijanović (2014)). A related set of empirical papers has studied the relationship between house prices and entrepreneurship to test whether access to collateralized debt through home mortgages is an important determinate of small business formation and employment (Hurst and Lusardi (2004), Schmalz, Sraer, and Thesmar (2013), Adelino, Schoar, and Severino (2015), Jensen, Leth-Petersen, and Nanda (2014)). Similarly, on the household side, Caplin, Freeman, and Tracy (1997) and Lustig and Van Nieuwerburgh (2010) provide empirical evidence for a link between housing collateral constraints and the consumption responses to regional income shocks in the United States, while Almeida, Campello, and Liu (2006) present cross-country evidence, suggesting that both house prices and the demand for mortgage debt are more sensitive to income shocks in countries with more generous collateral constraints.

Figure 1. Spatial distribution of MPDU properties in two example subdivisions. This figure shows the location of several MPDU properties in two representative subdivisions. MPDU properties are marked with an orange circle. All unmarked homes are market-rate units. The two shaded areas identify the subdivision boundaries. (Color figure can be viewed at wileyonlinelibrary.com)

must set aside a minimum of 12.5% of those units to be sold or rented to income-eligible households at controlled prices.[10] Except in rare cases, the affordable units must be provided on-site and are subject to minimum quality standards and planning guidelines that encourage the developer to scatter MPDUs among market-rate units in the same development. Figure 1 provides an example of the spatial distribution of MPDUs in two representative subdivisions.[11] In general, MPDU units tend to be distributed throughout the subdivision though design standards can lead to some clustering in large subdivisions since MPDUs are typically smaller than many of the market-rate units and are therefore often placed alongside each other. While MPDUs are permitted to be smaller in terms of interior square-footage and the construction standards provide some allowances for lower quality interior finishes (e.g., countertops and bathroom fixtures), both the planning guidelines and the private incentives of developers encourage the exterior design of MPDUs to reflect that of the nearby market-rate units. This can be seen in Internet Appendix Figure IA.1, which provides pictures of several example MPDUs and nearby market-rate units.[12] Since its inception, the program has resulted in the creation of roughly 14,000 housing units that were price-controlled at some point in their history.[13] As of the 2010 Census, these units represented roughly 3.7% of the total

[10] If the developer agrees to provide more than the required 12.5% of affordable units, they are also granted density bonuses that allow for the construction of more market-rate units than would otherwise be permitted under the preexisting zoning code. In practice, developers rarely take this option.

[11] The data used to determine the location of the price-controlled units are described in detail in Section III.

[12] The Internet Appendix is available in the online version of this article on the *Journal of Finance* website.

[13] This figure is based on aggregate counts published by the Montgomery County DHCA at http://www.montgomerycountymd.gov/DHCA/housing/singlefamily/mpdu/produced.html. The

stock of housing units in the county.[14] Approximately 70% of the MPDUs were originally offered for sale as owner-occupied units, while the remainder were marketed as rentals. In this paper, I restrict attention to the owner-occupied portion of the program.

B. Income Limits and Eligibility

Eligibility to purchase an MPDU is restricted to first-time homebuyers who qualify for a mortgage and whose annual gross household income falls within specified ranges published annually by the Montgomery County DHCA. In the frequent case in which more than one eligible buyer is interested in purchasing an MPDU, the right to purchase is allocated by lottery.[15] Minimum income limits are set at the same level for all households and are meant to reflect the minimum income required to qualify for a typical mortgage on an MPDU home. Maximum income limits are pegged to the median family income for the Washington, DC, metropolitan area as published by the U.S. Department of Housing and Urban Development (HUD). For a four-person household, the maximum income limit is set at 70% of the area median income for a household of the same size. That limit is then scaled by an adjustment factor to determine the income limits for households of other sizes. The income limits for 2014 are shown in Internet Appendix Table IA.I. In general, the income limits are quite high, reflecting both the relative affluence of the DC metropolitan area and the fact that the MPDU program is meant to specifically target *moderate*-income households. For example, the maximum income limit of $75,000 for a four-person household is roughly 17% higher than the 2014 national median income for a household of the same size. As will be discussed in Section V.E below, this leads purchasers of MPDU homes to look relatively similar to the typical homebuyer in the United States, at least in terms of household income.

C. Price Controls and Borrowing Restrictions

The initial purchase price for an MPDU is set by the DHCA using a schedule that is meant to reflect construction costs associated with housing units of various types and sizes. Owners of MPDUs are permitted to resell their homes. However, if the sale occurs before the end of the "control period" (a span of time ranging between 5 and 30 years depending on the initial purchase

aggregate counts disagree slightly with the number of units for which I was able to obtain microdata. This is likely due to changes in administrative record keeping that led some of the older units to drop out of the DHCA database from which my data are derived.

[14] This figure is less than the mandated 12.5% due primarily to the durability of the housing stock. According to the 2012 American Community Survey, the median housing unit in Montgomery County was built in 1977, implying that roughly half of the housing units in the county were built before the law went into effect. The remaining gap is likely made up by housing units in smaller subdivisions to which the law does not apply.

[15] The lottery gives preference to those living or working in the county and to those who have been on the waiting list for multiple drawings.

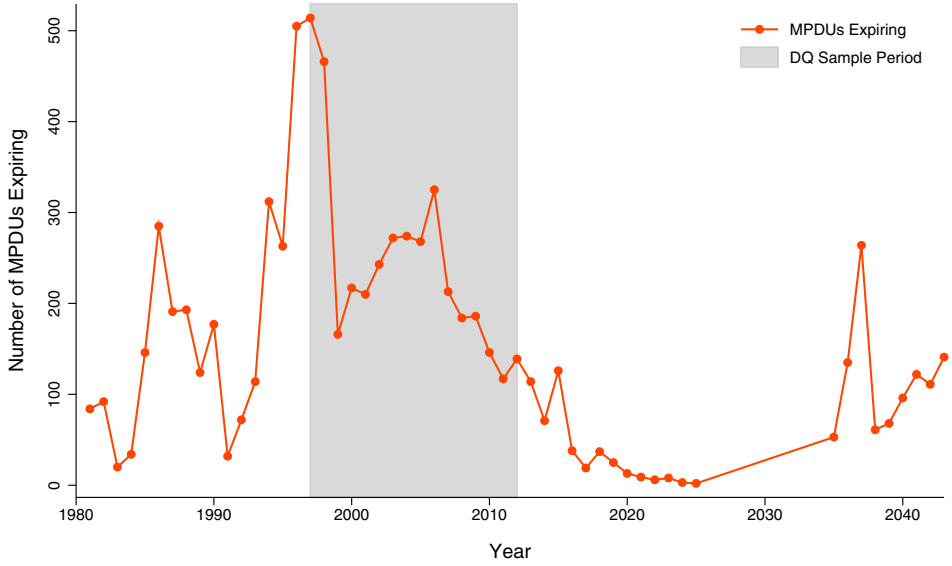


Figure 2. Number of expiring price controls by year. This figure plots the trend in price control expiration dates at a yearly frequency for all owner-occupied MPDU properties in the county. Each dot plots the number of properties whose price controls expired or will expire in the indicated year. The shaded gray area marks the period of time for which information on housing transactions and home equity-based borrowing is available from DataQuick. (Color figure can be viewed at wileyonlinelibrary.com)

date), then the resale price is capped at the original price plus an allowance for inflation. Dollar-for-dollar adjustments are also made to account for any documented home improvements. The resale restrictions are enforced through deed covenants that are tied to the land and are released upon the first sale after the end of the control period (see Internet Appendix Figure IA.2 for an example deed covenant). Owners who sell before the end of the control period must sell their home either directly to the DHCA or to another eligible household on the waiting list. The first owner to sell the home after the end of the control period is permitted to sell to any buyer at the market price but is required to split any capital gains over the controlled price equally with the DHCA.

Prior to 2002, the control period was set as a fixed period of time from the date of the initial sale by the developer. Beginning in March 2002, the program was changed so that the control period now resets if the unit is sold at any time prior to expiration. The length of the control period was also extended from 10 to 30 years in April 2005.[16] These changes to the law are reflected in Figure 2, which plots the number of MPDU properties whose price controls expired or will expire in each year since the inception of the program. The shaded gray area marks the period of time during which I am able to observe

[16] Internet Appendix Table IA.II shows the history of rules governing the length of the control period.

transactions and loans.[17] The 1980s construction boom shows up as an increase in the number of expiring price controls in the 1990s, while the boom associated with the most recent cycle will not show up until approximately 2035. Of the price-controlled homes in my final analysis sample, roughly 92% had a 10-year initial price control.

Importantly, the owner's ability to borrow against the home is also restricted during the control period. In particular, MPDU owners are prohibited from refinancing their mortgages or taking on home equity debt for an amount that exceeds the controlled resale price. Thus, while the market value of the home may be substantially higher than the controlled price, the owner is prohibited from pledging that equity as collateral until the expiration of the price control. This requirement is enforced by both the DHCA and by lenders themselves, who typically run title searches as part of the underwriting process. After the price controls have expired, MPDU owners are no longer restricted from borrowing more than the controlled price and, due to the shared profit agreement, are typically able to pledge up to half of the difference between the controlled price and the full market value as additional collateral.[18] As discussed in detail in Section V, I estimate that the average discount for an MPDU home during the control period is between $66,000 and $106,000, implying an increase in collateralized borrowing capacity of roughly $33,000 to $53,000. This increase in borrowing capacity can be used to provide direct estimates of the effect of housing collateral constraints on home equity-based borrowing.

II. Conceptual Framework

To illustrate how expiring price controls can be used to identify the effect of housing collateral constraints on homeowner borrowing, this section presents a stylized model of a homeowner's equity extraction decision. I begin by considering a baseline model in which there are no price controls. The purpose of this baseline case is to highlight the empirical difficulties confronted by prior research attempting to study the effect of collateral constraints through the lens of changing house prices. I then present a modified version of the model to show how the borrowing restrictions associated with MPDU price controls can be used to address these difficulties in an environment where the market value of housing is being held constant. The basic structure of this model draws heavily on Bhutta and Keys (2014), who use the same framework to study the effect of interest rates on equity extraction. To keep the model simple and focus the discussion on the features that are most relevant for the empirical analysis, I abstract from several issues that might be present in a more fully specified life-cycle model but would otherwise not permit an analytical solution.[19] Most

[17] The transaction and loan data as well as the data used to determine the number of expiring price controls in each year are discussed in detail in Section III.

[18] This is because lenders are aware of the owner's obligation to the county and are thus reluctant to extend credit for an amount beyond what the owner would receive in the event of sale.

[19] For examples of fully specified life-cycle models that incorporate the home equity extraction decision, see Hurst and Stafford (2004) or Chen, Michaux, and Roussanov (2013).

importantly, I assume that house prices and income are known with certainty, that households enter the world endowed with a house and a mortgage, and that they only live for two periods during which they may use their home as a source of collateral and as a source of wealth to fund consumption but from which they receive no direct utility.

A. *Baseline Case: No Price Controls*

Consider a household that lives for two periods, $t \in \{0, 1\}$, and is endowed with a house of value H and outstanding mortgage debt M_0 in the first period. The household has log preferences defined over nonhousing consumption in each period, $u_t(c_t) = \log(c_t)$, receives per-period income, y_t, and may extract equity by borrowing against the home in the first period, b_0, at the going mortgage interest rate, r, and up to an exogenous collateral constraint $\lambda H - M_0$, $\lambda \in [0, 1]$. The household chooses consumption in each period to maximize total lifetime utility

$$\max_{c_0, c_1} U(c_0, c_1) = \log(c_0) + \beta \log(c_1), \tag{1}$$

subject to constraints that are given in the baseline case by

$$c_0 = y_0 + b_0, \tag{2}$$

$$c_1 = y_1 - (1 + r)(M_0 + b_0) + \omega H, \tag{3}$$

$$0 \leq b_0 \leq \lambda H - M_0, \tag{4}$$

where $\beta \in [0, 1]$ is the discount factor and $\omega \in [0, 1]$ captures, in a reduced form way, the household's desire to consume out-of-housing wealth.

Differences in the parameter ω across households can arise from various sources. For example, in a life-cycle model with finitely lived households, ω will vary according to age. Younger households who plan to continue living in the same house for a longer period of time likely have lower values relative to older households who may choose to downsize in the near future (Campbell and Cocco (2007)). Similarly, ω may vary within age group due to differences in bequest motives, which lead households with larger bequest motives to consume less of their housing wealth before death. Or, as in Sinai and Souleles (2005), ω may covary with expected tenure length and with the correlation in house prices across markets to which a household is likely to move in the future. Modeling the housing wealth effect in this way, while somewhat ad hoc, greatly simplifies the discussion and is meant to capture these sources of heterogeneity without needing to specify a particular mechanism through which the wealth effect arises. The important point is that, for some households who plan to consume part of their housing wealth before death, increases in house prices will lead to a desire to smooth consumption across periods.

Substituting the per-period budget constraints (2) and (3) into the objective function (1) and solving for the optimal level of equity extraction yields the solution

$$b_0^* = \begin{cases} b^* \equiv \dfrac{y_1 + \omega H - (1+r)(M_0 + \beta y_0)}{(1+r)(1+\beta)} & b^* < \lambda H - M_0, \quad (5a) \\ \lambda H - M_0 & b^* \geq \lambda H - M_0, \quad (5b) \end{cases}$$

where b^* denotes the optimal level of borrowing in the absence of the collateral constraint. This expression highlights the empirical difficulties confronting past research that has attempted to study the effect of collateral constraints by observing how borrowing responds to changes in house prices. To see this, consider the effect of an exogenous increase in house prices on equity extraction. For unconstrained borrowers, this effect is given by the partial derivative of (5a) with respect to H and is a pure wealth effect, whereas for constrained borrowers, it is equal to the partial derivative of (5b) and operates entirely through the collateral constraint. The empirically observable change in borrowing is therefore given by:

$$\frac{\partial b_0^*}{\partial H} = \begin{cases} \dfrac{\omega}{(1+r)(1+\beta)} & b^* < \lambda H - M_0 \quad (6a) \\ \lambda & b^* \geq \lambda H - M_0, \quad (6b) \end{cases}$$

where (6a) is the wealth effect for unconstrained borrowers and (6b) is the collateral effect for constrained borrowers. Without prior knowledge of b^*, it is impossible to know which of these two conditions applies for a given household and therefore impossible to know how much of the observed average change in borrowing in response to a change in house prices is due to the relaxation of the collateral constraint. The empirical approach in this paper overcomes this issue by holding the market value of housing constant and looking instead at how homeowner behavior responds to the relaxation of the borrowing restriction associated with an expiring MPDU price control.

B. Identifying Collateral Effects Using MPDU Expiration Dates

It is straightforward to incorporate the MPDU price control into the above framework. To see this, note that, during the control period, an MPDU owner is prohibited from borrowing against the full market value of the property and therefore faces a more stringent collateral constraint, so that equation (4) becomes

$$0 \leq b_0 \leq \lambda(H - \eta) - M_0, \quad (7)$$

where $\eta \geq 0$ denotes the MPDU price discount. For an MPDU owner who plans to stay in the home beyond the end of the control period, the eventual resale value of the home in the second period, H, remains unchanged and the optimal level of borrowing can be found by replacing equation (4) with equation (7) and

resolving the borrower's problem:[20]

$$b_0^* = \begin{cases} b^* \equiv \dfrac{y_1 + \omega H - (1+r)(M_0 + \beta y_0)}{(1+r)(1+\beta)} & b^* < \lambda(H-\eta) - M_0 \quad (8a) \\ \lambda(H-\eta) - M_0 & b^* \geq \lambda(H-\eta) - M_0. \quad (8b) \end{cases}$$

In this framework, an expiring price control is equivalent to lowering the value of η to zero in the first period while leaving the eventual resale price of the home in the second period, H, unchanged. To see how this affects borrowing, note that the effect of a decrease in η on b_0^* is given by

$$-\frac{\partial b_0^*}{\partial \eta} = \begin{cases} 0 & b^* < \lambda(H-\eta) - M_0 \quad (9a) \\ \lambda & b^* \geq \lambda(H-\eta) - M_0. \quad (9b) \end{cases}$$

This expression makes immediately clear that borrowing should only respond to an expiring price control through the behavior of households who were collateral constrained prior to expiration. Comparing (9a) with (6a), we can see that there is no longer any role for wealth effects. Thus, any observed changes in borrowing behavior at the time the price control is lifted can be entirely attributed to the effect of relaxing collateral constraints. This is the key insight underlying the empirical analysis. In the following sections, I provide empirical estimates of the magnitude of this response by studying how the borrowing behavior of MPDU owners changes around the time the price control expires relative to that of owners of observationally identical market-rate units in the same housing development for whom there is no corresponding change in borrowing capacity.

C. Expected Tenure and Price Salience

The discussion thus far has assumed that MPDU owners plan to stay in their homes beyond the end of the price control period. If, however, they are uncertain as to whether they will need to sell their home before the price control expires, then there may still be a role for wealth effects: as the expiration date draws closer, the likelihood of needing to sell before expiration decreases, generating an increase in expected wealth that could induce increases in borrowing. In practice, however, this possibility is unlikely to be a first-order concern for my empirical analysis. Uncertainty over the need to sell should resolve itself smoothly as the date of the expiration draws closer. Within a very narrow band of time around the expiration date, it is reasonable to assume that MPDU owners have complete certainty over whether they would need to sell their home.

[20] Here, H should be thought of as the owner's expected proceeds from selling the home, net of the profit-sharing agreement with the county. While the price control affects the amount of profit sharing, the key point is that, for owners who plan to stay in the home beyond the end of the control period, that effect is fully anticipated so that the actual timing of the price control expiration has no effect on the expected proceeds from selling the home.

Thus, there should be no discontinuous jump in expectations at the time of expiration. Given that the borrowing response I document occurs discontinuously in *exactly* the year the price control expires, it is likely not being driven by this channel.

A second, related concern is that MPDU owners' beliefs about the market value of their homes may change discontinuously at the time of expiration. This could happen if the salience of the expiring price control itself leads owners to update otherwise stale priors regarding the price of housing in their neighborhoods.[21] While I cannot rule out the possibility of this type of behavioral effect, in order for it to generate an increase in borrowing, such an effect would have to cause MPDU owners to systematically revise *upward* their beliefs about the value of their homes. In the Internet Appendix, I explore this possibility by comparing the magnitude of the borrowing effect during "boom" and "bust" periods, which are likely to be associated with opposing revisions in house price expectations. The results are slightly larger during boom periods; however, the qualitative magnitudes of the effects are similar, suggesting a limited role for this type of salience effect.[22]

III. Data and Measurement

To conduct the empirical analysis, I merge data at the property, transaction, and loan level using information from tax assessments, deeds records, and administrative data from the MPDU program. This section provides a brief overview of the data sources, variable construction, and sample selection procedures. Further details are available in the Internet Appendix.

A. Data Sources

A.1. Property-Level Data

The basic structure of my data set is organized around the 2011 Montgomery County property tax assessment file, which provides a single snapshot of all taxable properties in the county as of 2011. This file was purchased from DataQuick, a private vendor that collects and standardizes publicly available tax assessment and deeds records from municipalities across the United States. The tax assessment file includes detailed information on the physical characteristics (e.g., square footage, number of bathrooms, number of stories, and year built), use type (e.g., residential, commercial, single-family, and condo), and

[21] This could also occur if the expiring price control provides the owner with new information about the value of the house. For example, the owner's property tax assessment will change to reflect the estimated market value of the home after expiration. However, tax assessments in Maryland are only updated every three years, so the expiration of the price control would have to line up with the tax assessment year for this to lead to new information being provided in the year of the expiration itself.

[22] See Table IA.X for these results.

street address for every property in the county. From this file, I drop all nonresidential and multifamily properties as well as any properties with missing characteristics.[23] This leaves a "universe" of 286,484 single-family residential properties from which I select my analysis sample. Each of these properties is geocoded and assigned a subdivision ID based on whether the geographic coordinates for the property fall within the boundaries of a particular subdivision, as delineated by the Maryland State Department of Assessments and Taxation (SDAT).[24]

To identify MPDU homes, I match the property assessment file with a list of MPDUs retrieved from a publicly available online search portal hosted by the DHCA.[25] These data include the street addresses for all MPDU properties in the DHCA administrative database as well as the price control expiration dates for those properties. MPDU properties are matched to the assessment file using a combination of exact physical location (geographic coordinates) and street address as described in the Internet Appendix. Of the roughly 8,300 MPDUs in the DHCA database, I am able to match approximately 90% to a property in the assessment file.[26] Figure 3 maps the location of these properties as well as census tract-level population density for Montgomery County in 2010. In general, MPDU properties are evenly distributed across the nonrural regions of the county. One exception is the southern region of the county immediately bordering Washington, DC, where MPDUs are underrepresented. This region contains the cities of Bethesda and Silver Spring and was developed much earlier than the rest of the county.[27] As a result, much of the housing stock in that area was not subject to the MPDU regulations at the time of development.[28]

[23] The Montgomery County assessment data are of unusually high quality. Only 3,702 out of 290,186 single-family residential properties are dropped due to having missing characteristics. In 3,258 of these cases, it is the year built that is missing while other characteristics, such as square footage and number of bathrooms, are coded as zero, suggesting that many of these properties were vacant land at the time of assessment.

[24] The subdivision boundary file was created using a parcel-level boundary file provided by the Montgomery County Planning Department. In addition to the geographic boundaries, this file also contains the SDAT subdivision ID for each parcel. The subdivision boundaries were constructed by dissolving the individual parcel boundaries into larger polygons based on whether they shared the same subdivision ID.

[25] The search portal can be accessed at http://www6.montgomerycountymd.gov/apps/DHCA/pdm_online/pdmfull.asp. The data were retrieved by exhaustively searching through and returning all possible MPDU addresses beginning with an alpha-numeric character.

[26] The match rate is lower than 100% largely due to poor-quality record keeping in the DHCA database for some of the older MPDU properties. For example, when matching on street address, I require an exact match on the street number. Some of the older MPDU properties are missing street numbers and are therefore not included in the set of matches.

[27] This can be seen in Internet Appendix Figure IA.3, which replicates Figure 3 after replacing population density with property age.

[28] Another reason for the underrepresentation of MPDUs in this region is that a larger fraction of the housing stock in the most densely populated areas (i.e., central cities) is composed of rental properties, which are not included in the MPDU data that I use.

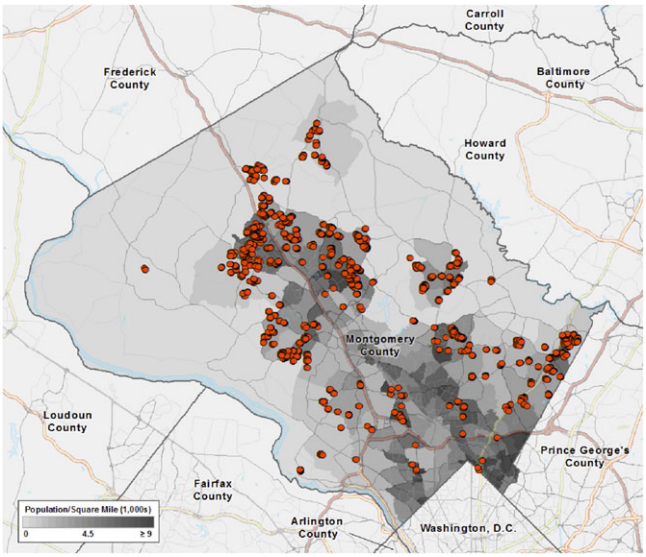


Figure 3. Geographic distribution of MPDU properties within Montgomery County, Maryland. This figure shows the location of all MPDU properties that were successfully matched to a property in the DataQuick assessment file ($N = 7{,}404$). MPDU properties are marked with an orange circle. Census tracts within Montgomery County are shaded according to their population density as reported in the 2010 American Community Survey. (Color figure can be viewed at wileyonlinelibrary.com)

A.2. Transaction and Loan-Level Data

To analyze how expiring price controls affect collateral values and homeowner borrowing, I merge the property-level file with two additional data sets from DataQuick. Both data sets are sourced from local deeds records and can be linked to properties in the assessment file using a unique property ID. The first data set contains information on all housing transactions occurring in the county during the period 1997 to 2012. For each transaction, this data set records the purchase price, buyer, seller, and lender names, as well as loan amounts on up to three loans used to finance the purchase. The second data set contains information on all nonpurchase loans secured against a property during the same period. This data set records the initial loan amount and borrower and lender name for every refinance, junior lien, and home equity line of credit (HELOC) secured against a property. Together, these two data sets provide a highly granular and near complete picture of all mortgage borrowing and housing purchases occurring in the county during this period. Each data set is cleaned as described in the Internet Appendix to ensure that the transactions represent true ownership-changing arm's length transactions and that the loan information is accurate and consistent.

B. Measuring Equity Extraction

Since the nonpurchase loans data set contains a combination of loan types but does not distinguish between them, several steps must be taken to construct an accurate measure of equity extraction. In particular, it is important to distinguish between three different types of nonpurchase 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 nonpurchase originations, which directly extract equity for the amount of the new loan. 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. Debt histories are constructed by amortizing prior loan balances using the average interest rate at the time the loan was originated.[29] Given this history, when a new loan is observed, I can determine whether that loan represents a purchase loan, cash-out refinance, new nonpurchase origination, or regular refinance by comparing the size of the new loan to the estimated outstanding balance on the relevant existing loan (see the Internet Appendix for details on this procedure). When a new refinance or purchase loan is observed, the old loan is replaced and the new loan serves as the basis for calculating remaining debt going forward.

After categorizing loans in this way, I construct an annual panel that records for each property whether the current owner extracted equity in a particular year, and if so, how much equity was extracted. I define total equity extraction in a given year as the sum of nonpurchase originations and cash withdrawn through cash-out refinances during that year. Similarly, an owner is defined as having extracted equity in a given year if total equity extracted is greater than zero. For properties built prior to 1997, the panel covers the full sample period from 1997 to 2012; for properties built afterwards, the construction year is used as the first year of observation. Each observation is also uniquely associated with a particular "ownership spell" for that property. Ownership spells are defined to include all years between ownership-changing transactions, where the first ownership spell starts in either 1997 or the year that the property was built. In the Internet Appendix, I provide details validating the accuracy of this equity extraction measure against two measures provided at the aggregate level based on data from Equifax credit reports and the Freddie Mac Quarterly Cash-Out Refinance report. In both cases, my measure of equity extraction is shown to be highly correlated with national aggregates.

[29] 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). Since the DataQuick data do not distinguish between HELOCs and closed-end liens, 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. See the Internet Appendix for the details of this procedure.

C. Sample Restrictions and Descriptive Statistics

Starting with the full sample of 286,484 properties, I impose several restrictions to arrive at my primary analysis sample. I first drop any property that cannot be matched to a housing subdivision. This eliminates 31,603 properties located primarily in rural and outlying areas of the county where SDAT does not assign subdivision IDs. I further drop all properties located in subdivisions containing no MPDUs. This restriction eliminates 167,117 properties, many of which are located in densely populated areas consisting mostly of rental housing or in older subdivisions to which the regulation did not apply. Among subdivisions containing MPDUs, I further require that at least one MPDU expires during the DataQuick sample period. This eliminates 35,236 properties located in either older subdivisions containing only MPDUs that had already expired as of 1997 or in more recently developed subdivisions containing only MPDUs that had yet to expire as of 2012. Finally, I require that all MPDUs within a subdivision have nonmissing expiration dates and that at least 95% of the MPDUs can be matched to their corresponding DataQuick property ID in a way that requires that the property unit number agree. The latter requirement is imposed because MPDUs are often classified as townhomes or condominiums, which, in addition to a standard street address, also have a unit number. The final sample contains 31,244 properties located in 69 subdivisions throughout the county.

Table I presents descriptive statistics for both the full sample of properties and the restricted sample used in the analysis. For the analysis sample, summary statistics are presented both pooling across all properties as well as separately for non-MPDUs and MPDUs. In Panel A, the unit of observation is the individual property; in Panel B, it is the transaction; and in Panel C, it is the property-year. All dollar amounts here and throughout the paper are converted to real 2012 dollars using the Consumer Price Index for All Urban Consumers (CPI-U). To limit the influence of extreme outliers, transaction prices are winsorized at the 0.5^{th} and 99.5^{th} percentiles in the full sample.

The differences between the full sample (columns (1) and (2)) and the analysis sample (columns (3) and (4)) are largely what would be expected given the nature of the sample restrictions imposed. Properties in the analysis sample are newer and larger than properties in the full sample, reflecting the fact that the MPDU regulations only apply to subdivisions constructed after 1974. Despite being newer, these properties transact at slightly lower prices than the average house in the county, again likely reflecting the fact that many of the properties in the oldest and most expensive region of the county immediately bordering Washington, DC, are located in subdivisions that do not contain MPDUs and are thus not included in the analysis sample. With regard to borrowing behavior, the average owner in the analysis sample is slightly more likely to extract equity in a given year relative to the average owner in the county but, conditional on extracting, typically borrows less.

Within the analysis sample, the differences between market-rate units (columns (5) and (6)) and MPDUs (columns (7) and (8)) are also largely in line

Table I

Summary Statistics for Properties, Transactions, and Annual Measures of Equity Extraction

This table presents descriptive statistics for both the full sample of single-family residential properties with nonmissing housing characteristics contained in the DataQuick assessment file (columns (1) and (2)) and the restricted sample used in the analysis (columns (3) to (8)). All table entries represent sample means or, in parentheses, standard deviations. For the analysis sample, summary statistics are presented pooling across all properties as well as separately for non-MPDUs and MPDUs. In Panel A, the unit of analysis is the individual property; in Panel B, it is the transaction; and in Panel C, it is the property-year. All dollar amounts are converted to real 2012 dollars using the Consumer Price Index for All Urban Consumers (CPI-U). Transaction prices are winsorized at the 0.5^{th} and 99.5^{th} percentiles in the full sample.

	Full Sample		Analysis Sample					
	All Properties		All Properties		Non-MPDUs		MPDUs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Property Characteristics								
Square Footage (1,000s)	1.90	(4.50)	1.98	(1.01)	2.07	(1.02)	1.18	(0.22)
Number of Bathrooms	2.62	(1.17)	3.00	(1.05)	3.08	(1.04)	2.26	(0.77)
Number of Stories	1.65	(0.53)	1.86	(0.42)	1.85	(0.41)	1.95	(0.53)
Age (Years)	37.80	(20.28)	24.73	(9.86)	25.10	(10.13)	21.18	(5.66)
Number of Properties	286,484		31,244		28,278		2,966	
Panel B. Transaction/Buyer Characteristics								
Transaction Price ($1,000s)	444.09	(330.85)	418.09	(290.70)	432.81	(294.80)	237.37	(140.34)
Loan Amount ($1,000s)	351.18	(673.69)	330.10	(200.84)	339.53	(202.00)	210.58	(137.71)
Loan-to-Value Ratio	0.85	(0.57)	0.86	(0.61)	0.85	(0.58)	0.94	(0.90)
Fraction FHA Insured	0.14	–	0.16	–	0.15	–	0.27	–
Number of Properties	156,206		19,152		17,719		1,433	
Number of Transactions	233,879		30,209		27,934		2,275	
Panel C. Equity Extraction Measures								
Amount Extracted ($1,000s)	7.63	(40.97)	8.45	(40.75)	8.85	(42.08)	4.63	(24.37)
Amount Extracted | > 0 ($1,000s)	101.08	(113.07)	92.48	(101.96)	94.14	(103.91)	69.93	(66.31)
Probability of Extracting Equity	0.08	–	0.09	–	0.09	–	0.07	–
Number of Properties	286,484		31,244		28,278		2,966	
Number of Ownership Spells	493,386		57,333		52,390		4,943	
Number of Property-Years	4,383,768		483,805		437,977		45,828	

with what would be expected given the nature of the MPDU program. MPDUs are smaller and substantially cheaper than non-MPDUs. This price difference is due to a combination of differences in housing characteristics and the price control itself, which mechanically lowers prices for part of the sample period. Owners of MPDUs are also less likely to extract equity in a given year and, conditional on extracting, typically borrow less relative to owners of market-rate units. Some of the difference in equity extraction behavior is due to intrinsic differences in the preferences and characteristics of owners of different types of housing, while some of it is driven by the existence of the MPDU borrowing restrictions. By comparing how these overall average differences change around the time the price controls expire and controlling flexibly for aggregate trends and observable characteristics, my empirical strategy isolates the portion of the difference in prices and borrowing behavior that is driven by the MPDU program itself.

IV. Empirical Framework

A. Identification Strategy

I estimate the effect of expiring price controls on collateralized capacity and borrowing behavior using a difference-in-differences research design that compares outcomes among market-rate units (the control group) and MPDUs (the treatment group) in the same housing development before and after the expiration of the price control. The key identifying assumption is that, in the absence of the expiring price control, the borrowing behavior and prices paid by owners of MPDUs and owners of market-rate units in the same subdivision would have evolved in parallel.

In the Section V below, I present direct evidence to support the validity of the parallel trends assumption by showing that outcomes for MPDUs and non-MPDUs move together during the period prior to the expiration of the price control and that their trends only begin to diverge afterward. This fact is both reassuring and perhaps unsurprising. While fixed differences in the characteristics of MPDU properties and their owners from those of market-rate units may lead to constant differences in prices and borrowing behavior, there is no particular reason to expect that the *evolution* of prices and borrowing over time should vary greatly across these two groups. Properties within the same subdivision are exposed to the same changes in local amenities, school quality, and crime, and frequently belong to a common homeowner's association. As a result, changes in the willingness to pay of the marginal neighborhood entrant, and thus market prices, should evolve similarly for all properties in the development. Along the same lines, homeowners within a given subdivision all face the same changes in aggregate determinants of equity extraction, such as interest rates and credit standards, and should therefore be expected to display similar changes in borrowing behavior. While MPDU owners may be more likely to be subject to income shocks that could induce them to extract equity (Hurst and Stafford (2004)), the timing of such shocks would have to be highly correlated

both with the expiration of the price control and across MPDU owners within a subdivision to generate systematic differences in trends.

B. Estimation

My baseline econometric model is a simple difference-in-differences regression fit at the individual property level. Specifically, I estimate regressions of the following form:

$$y_{ist} = \alpha_s + \delta_t + X'_{it}\gamma + \beta_1 \cdot MPDU_i + \beta_2 \cdot MPDU_i \times Post_{st} + \epsilon_{ist}, \quad (10)$$

where y_{ist} is an outcome for property i in subdivision s in year t, α_s are subdivision fixed effects, δ_t are year fixed effects, X_{it} is a vector of possibly time-varying property characteristics, and ϵ_{ist} is an error term assumed to be conditionally uncorrelated with unobserved determinants of y_{ist}. The dummy variable $MPDU_i$ is a treatment indicator that takes the value 1 if property i is an MPDU, while the $Post_{st}$ indicator takes the value 1 if year t falls on or after the year the first price control in subdivision s expires. In all specifications, the $Post_{st}$ main effect is also included among the set of covariates, X_{it}. I define the treatment date in this way to take into account the fact that market-rate properties have no explicit expiration date, as well as the fact that controlled properties within a subdivision may expire at different times as a result of construction lags and differences in initial purchase dates. Using the first expiration date is conservative and should only serve to attenuate the estimates since a small number of properties will be counted among the "treated" group before their controls actually expire.[30] The coefficient of interest is β_2, which measures the differential change in the outcome for MPDUs relative to non-MPDUs following the expiration of the price control, holding constant individual housing characteristics and aggregate differences in outcomes both across subdivisions and over time. To account for serial correlation and subdivision-specific random shocks, I cluster the standard errors at the subdivision level in all specifications.

One potential concern with this specification is that, in addition to providing an increase in collateralized borrowing capacity, the expiration of the price control may also create an incentive for MPDU owners to sell their homes. As a result, differences in prices and borrowing behavior following the expiration of the price control may be driven by changes in the composition of MPDU properties that transact or changes in the characteristics of owners of MPDU homes.[31] I address this concern directly by estimating specifications

[30] Over half of all MPDUs in my sample expire within two years of the first MPDU in their subdivision and roughly 75% expire within five years. These differences are most likely due to normal construction lags. Differences larger than five years likely come from one of two sources: (1) phased property development in larger subdivisions, which may be built out over longer periods of time, and (2) MPDU owners reselling during the control period in the latter portion of the sample, when program rules dictated that price controls reset if the property is sold during the control period.

[31] In the Internet Appendix, I present results showing that expiring price controls do, in fact, lead to an increase in housing turnover for previously controlled units of roughly 3 to 5 percentage

that also include property and ownership spell fixed effects. Including property fixed effects in the price regressions controls for changes to the set of houses that transact and identifies the effect of expiring price controls by comparing *within*-property changes in prices between MPDUs and market-rate units following the expiration of the price control. Similarly, including ownership-spell fixed effects in the equity extraction regressions controls for differences in the characteristics of owners and identifies the effect of expiring price controls by comparing *within*-owner changes in borrowing behavior between owners of MPDUs and market-rate units. In all cases, results from these specifications are not meaningfully different from those that do not include property or owner fixed effects.

As a more flexible alternative to (10), I also estimate specifications that allow the effect of the price control to differ by year relative to the first control period expiration. Specifically, let $\tau(s)$ denote the year the first MPDU in subdivision s expires. To capture the full time path of the effect of expiring price controls, I estimate specifications of the form

$$y_{ist} = \alpha_s + \delta_t + X_{it}'\gamma + \beta_1 \cdot MPDU_i + \sum_{\rho=-5}^{5} \Big[\eta_\rho \cdot \mathbb{1}_{t-\tau(s)=\rho} + \beta_{2,\rho} \cdot MPDU_i \times \mathbb{1}_{t-\tau(s)=\rho}\Big] + \epsilon_{ist}, \tag{11}$$

where $\mathbb{1}_{t-\tau(s)=\rho}$ is a relative year dummy taking the value 1 if the current year falls ρ years after the expiration of the price control and 0 otherwise. All other variables are as previously defined. The coefficients η_ρ and $\beta_{2,\rho}$ measure the baseline trend in the outcome for non-MPDUs and the differential trend for MPDUs, respectively, around the time the price control expires. I show results for up to five years preceding and following the expiration of the price control, grouping all years outside that window into the effects for relative years -5 and 5. Relative year -1 is always the omitted category so that the coefficients should be interpreted relative to the year prior to the first price control expiration within the subdivision. These coefficients are informative about both the timing of the effect of price control expirations and the validity of the parallel trends assumption. If MPDUs and non-MPDUs have common pretrends, then the $\beta_{2,\rho}$ coefficients should be equal to zero for any $\rho < 0$. This specification is also useful for ruling out the potentially confounding role of uncertainty over the need to sell before the end of the control period. Since this uncertainty would presumably resolve itself smoothly over time as the expiration date approaches, we would not expect the effect to manifest itself as a level shift in the $\beta_{2,\rho}$ coefficients in exactly the year the price control expires if this were the main factor driving the results.

points per year. However, the Internet Appendix also reports results from several robustness checks showing that this differential turnover is likely to have a negligible effect on the estimates. This is not surprising given that turnover has a relatively small impact on the composition of the sample. 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.

V. Price Controls, Collateralized Borrowing Capacity, and Borrowing Behavior

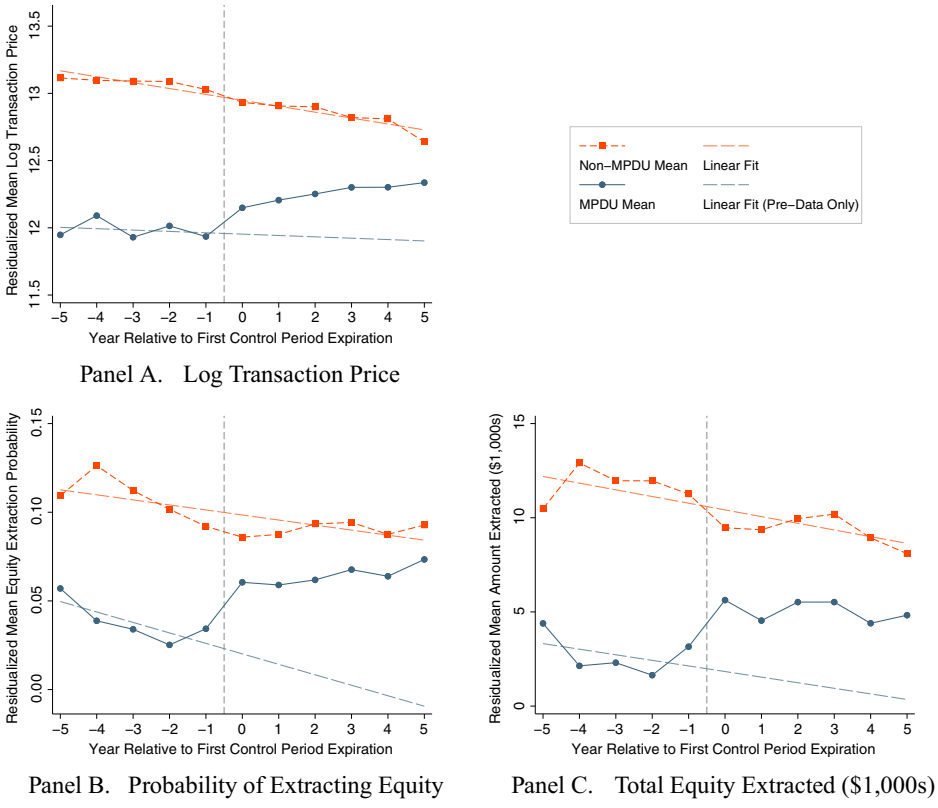
This section presents the main estimates of the effect of expiring price controls on the transaction prices (i.e., pledgeable collateral value) of previously controlled MPDUs and borrowing behavior among the owners of those properties. As an initial assessment of the validity of the parallel trends assumption, I begin by presenting simple graphical results for each of three main outcomes: (1) log transaction prices, (2) the annual probability of extracting equity, and (3) total equity extracted per year. To quantify the causal effects of interest, I then present a series of formal difference-in-differences estimates for each of the three outcomes. These estimates are subsequently combined to yield estimates of the marginal propensity to borrow out of increases in housing collateral. To provide additional evidence on the role that collateral constraints play in governing the borrowing response to expiring price controls, I also examine heterogeneity in the response across the distribution of initial leverage. Finally, I also explore heterogeneity in the borrowing response according to household income. Unless otherwise specified, all results pertain to the set of transactions and property-years contained in the analysis sample described in Section III.

A. Graphical Evidence

As a point of departure for the empirical analysis, Figure 4 plots calendar year-adjusted means for each of the three main outcomes. Means are plotted separately for MPDUs (blue circles) and non-MPDUs (orange squares) as a function of years relative to the first control period expiration within the relevant subdivision. In each panel, relative year 0 represents the year the first MPDU within the subdivision expired. Means are shown for up to five years preceding and following the expiration of the price control, grouping all years outside that window into the means for relative years -5 and 5. The plotted means should be interpreted as the mean outcome among MPDUs and non-MPDUs in a given relative year, adjusted for aggregate county-wide trends affecting all properties.[32] For visual reference, the dashed lines plot a linear trend for each outcome, derived from the fitted values of a regression of the binned means on a linear term in relative year. For MPDUs, only the preperiod means are used to construct the fitted values.

Consistent with the aggregate descriptive statistics presented in Table I, in any given relative year, MPDU properties transact at lower prices relative to non-MPDUs and their owners are less likely to extract equity from their homes.

[32] The means are adjusted for calendar year in order to remove the effect of the housing cycle, which would otherwise swamp the variation in the figure. Adjusted means were created by regressing the indicated outcome on a full set of calendar year fixed effects and averaging the residuals from that regression separately for MPDUs and non-MPDUs within relative year bins. To clarify the interpretation of the y-axis, the grand mean of each outcome was then added back into both series.



Panel A. Log Transaction Price

Panel B. Probability of Extracting Equity

Panel C. Total Equity Extracted ($1,000s)

Figure 4. Assessing the parallel trends assumption. This figure plots calendar year-adjusted means for each of the three primary outcomes—log transaction prices, the annual probability of extracting equity, and total equity extracted per year—separately for MPDUs (blue circles) and non-MPDUs (orange squares) as a function of years relative to the first control period expiration within the relevant subdivision. Relative year zero represents the year the first MPDU within the subdivision expired. Means are shown for up to five years preceding and following the expiration of the price control, grouping all years outside that window into the means for relative years −5 and 5. Adjusted means are created by regressing the indicated outcome on a full set of calendar year fixed effects and averaging the residuals from that regression separately for MPDUs and non-MPDUs within relative year bins. To clarify the interpretation of the *y*-axis, the grand mean of the outcome is then added back to both series. The dashed lines plot the fit from a regression of the binned means on a linear term in a relative year. For MPDUs, only the preperiod means are used to construct the fitted values. (Color figure can be viewed at wileyonlinelibrary.com)

However, for all three outcomes, the MPDU means diverge significantly from their preperiod trend starting in the year the first MPDU price control expires. There is no corresponding shift in the outcomes for non-MPDUs. As a result, a large portion of the gap in outcomes that exists during the imposition of the price control disappears once the price control expires. After five years, roughly half of the raw gap in prices and total equity extraction and over three quarters of the gap in the annual probability of extracting equity are eliminated.

Table II
The Effect of Expiring Price Controls on the Transaction Prices of MPDU Properties

This table reports difference-in-differences estimates of the effect of expiring MPDU price controls on transaction prices for MPDU properties. Each column reports a separate regression estimated at the transaction level where the dependent variable is the log of the transaction price. 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 percentage increase in prices associated with the coefficient estimate on the *MPDU* × *Post* indicator reported in the second row of the table. The implied dollar increase is calculated by applying that percentage increase to the mean price (in levels) among MPDU properties in the period prior to the expiration of the price control. Standard errors are reported in parentheses and are clustered at the subdivision level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)
MPDU	–0.455***	–0.616***	–0.620***	
	(0.070)	(0.067)	(0.067)	
MPDU × Post	0.455***	0.499***	0.505***	0.349**
	(0.077)	(0.084)	(0.083)	(0.139)
Property Characteristics	X	X	X	
Year and Age FEs	X	X	X	X
Subdivision FEs		X	X	
Subdivision Trend			X	X
Property FEs				X
Implied %Δ	57%	64%	65%	40%
Implied $Δ (1,000s)	$93	$104	$106	$66
R-squared	0.76	0.81	0.81	0.94
Number of Observations	30,209	30,209	30,209	30,209

The remaining gaps reflect fixed differences in the characteristics of MPDU properties and their owners that would presumably exist even in the absence of the price control (many of which are controlled for in the analysis below). Importantly, the non-MPDU trend for each outcome is almost exactly parallel to the preperiod MPDU trend, providing strong support for the validity of the parallel trends assumption underlying the difference-in-differences estimates that follow.

B. The Effect of Expiring Price Controls on Collateralized Borrowing Capacity

To more precisely quantify the effect of expiring price controls on the collateralized borrowing capacity of MPDU owners, Table II presents estimates from

the pooled difference-in-differences specification given by equation (10) using log transaction prices as the outcome. The first column reports estimates from a baseline specification that includes only the *MPDU* main effect, the interaction of that effect with the *Post* indicator, fixed effects for both the year of observation and the age of the property in that year, the *Post* main effect, and a series of time-invariant property characteristics. 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. Since MPDU homes are frequently classified as condos and townhomes, I also fully interact the condo indicator with the year fixed effects, age fixed effects, and other time-invariant characteristics. This allows the aggregate trends, age profiles, and hedonic value of fixed property characteristics to freely vary with property type. The coefficient estimate on the *MPDU* main effect implies that, during the imposition of the price control, MPDU properties sell at a discount of roughly 45 log points relative to observationally identical non-MPDU properties. This price gap is then completely eliminated following the expiration of the price control, as evidenced by the identical but opposite signed coefficient on the *MPDU* × *Post* indicator. The bottom panel also reports the implied percentage change and absolute dollar change associated with the estimated 45 log point increase.[33] Following the expiration of the price control, transaction prices at previously controlled MPDUs increase by roughly 57%. Applying that figure to the mean of the preperiod transaction price (in levels) among MPDUs implies an increase of roughly $93,000.

The remaining columns of the table add a series of control variables that increasingly restrict the nature of the comparison that is being used to identify the effect of the price control. In the second column, I add a set of fixed effects for each of the 69 subdivisions. This specification removes the influence of average differences in price levels across subdivisions and identifies the effect of the price control by comparing prices for observationally identical MPDUs and non-MPDUs within the same subdivision before and after the expiration of the price control. The third column not only allows for average differences in price *levels* but also allows the aggregate *trend* in prices to vary across subdivisions by interacting the subdivision fixed effects with a linear time trend. Finally, to address concerns related to differential turnover at MPDU properties after the price control expires, column (4) includes a full set of property fixed effects. In this specification, the time-invariant property characteristics and *MPDU* main effect drop out, and the effect of the price control is identified by comparing within-property changes in prices for properties that are and are not MPDUs.

The estimated effects are all highly significant and relatively stable across specifications, implying that expiring price controls lead to an increase in prices at previously controlled MPDUs that ranges from 35 to 50 log points (40% to 65%). Converting these estimates into dollars implies that expiring price

[33] Following Kennedy (1981), I calculate the implied percentage increase associated with the coefficient estimate $\hat{\beta}_2$ and its standard error $\hat{\sigma}_{\beta_2}$ as $\%\Delta = 100 \times [\exp(\hat{\beta}_2 - \frac{1}{2}\hat{\sigma}^2_{\beta_2}) - 1]$.

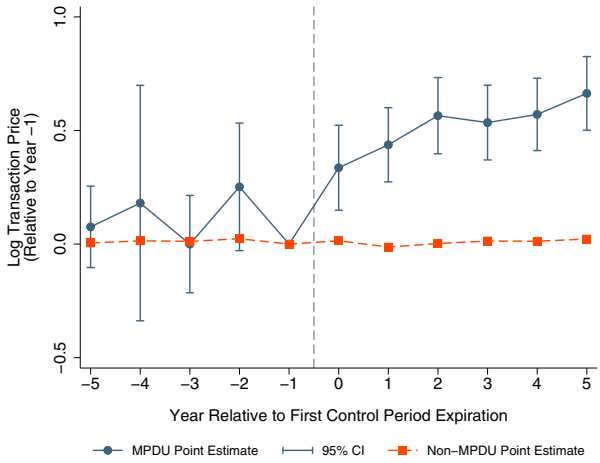


Figure 5. Dynamic effects of expiring price controls on transaction prices of MPDU properties. This figure reports estimates of the effect of expiring price controls on the transaction prices of MPDU properties derived from a flexible difference-in-differences regression that allows the effect to vary by year relative to the expiration of the price control. Estimates are constructed by regressing the log of the transaction price on an indicator for whether the associated 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 log prices 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 that are clustered at the subdivision level. The regression also includes 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. (Color figure can be viewed at wileyonlinelibrary.com)

controls lead transaction prices to increase by roughly $66,000 to $106,000. Due to the shared profit agreement, half of that increase belongs to the county, while the owner retains the other half as equity. Assuming that banks are willing to lend against the full increase in equity, these estimates imply that the average MPDU owner experiences an increase in collateralized borrowing capacity of approximately $33,000 to $53,000 upon the expiration of the price control.

To give a sense of the dynamics of the price effect, Figure 5 plots estimates from the more flexible difference-in-differences specification given by equation (11). These estimates are obtained from a regression that includes all of the same controls as the specification in column (3) of Table II, but that allows the

effect of the price control to vary separately for MPDUs and non-MPDUs by year relative to the first control period expiration. The series in orange squares plots the coefficient estimates on the 10 relative year main effects, while the series in blue circles plots the sum of the relative year main effects and their interaction with the *MPDU* "treatment" dummy along with the 95% confidence interval for that sum. In both cases, relative year −1 is the omitted category, so that the two series can be interpreted as the trends for non-MPDUs (orange squares) and MPDUs (blue circles) relative to their respective values in the year prior to when the first MPDU in the subdivision went off of price control. Prices for MPDUs diverge sharply from their preperiod trend starting precisely in the year that the first price control expires. In contrast, the trend among market-rate units is completely flat. The price effect grows over time for MPDU properties, reflecting the fact that some MPDUs had yet to actually expire as of the date the first MPDU within their subdivision expired. Importantly, the trends are statistically indistinguishable in the period prior to the expiration of the price control and only begin to diverge in the year of expiration, thus lending further support to the validity of the parallel trends assumption required for identification in the difference-in-differences research design.

C. Borrowing Responses

The evidence presented in the previous section suggests that expiring price controls lead to a large increase in collateralized borrowing capacity for the owners of previously controlled MPDU homes. In this section, I explore how the borrowing behavior of MPDU homeowners responds to this relaxation of their collateral constraint.

C.1. Extensive Margin Borrowing Responses

I begin by considering the homeowners' extensive margin choice of whether or not to extract equity from their home. To do so, I turn to the annual property-level panel and estimate versions of the pooled difference-in-differences regression given by equation (10) using as the outcome an indicator for whether the property's owner extracted equity in a particular year. Table III presents the results from these regressions. In the first three columns, the control variables are the same as those used for estimating the price effect in Table II and are introduced in the same order across the columns. To account for the potential for differential turnover and changes in ownership at MPDU properties following the expiration of the price control, the fourth column includes fixed effects for each of the 57,333 unique ownership spells observed in the panel. These four specifications are all estimated using simple linear probability models. To explore the sensitivity of the results to alternative estimators, columns (5) and (6) report the marginal effects from probit and logit models estimated using the same controls contained in column (3).

Across specifications, the estimates are extremely precise and highly stable. The baseline specification in column (1), which contains only the *MPDU* main

Table III

The Effect of Expiring Price Controls on the Annual Probability of Extracting Equity among Owners of MPDU Properties

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. 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) to (4) report coefficient estimates from linear probability models, while columns (5) and (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.

	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 × Post	0.041***	0.040***	0.041***	0.034***	0.044***	0.042***
	(0.008)	(0.008)	(0.008)	(0.008)	(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	483,805	483,805	483,805	483,805	483,805	483,805

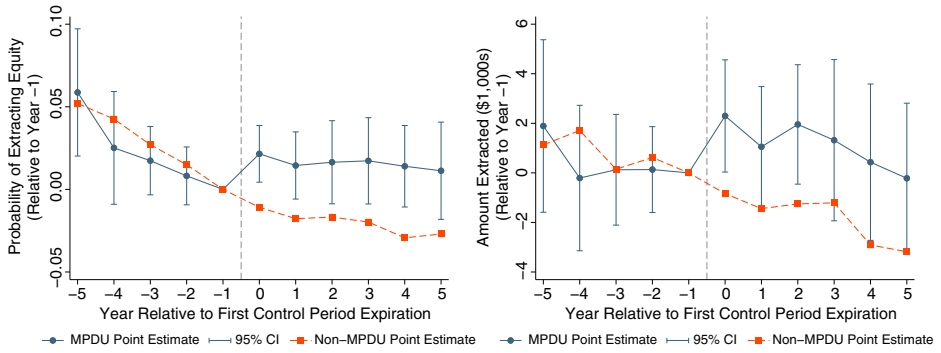
effect, the *MPDU* × *Post* interaction, the *Post* main effect, year fixed effects, and property characteristics, indicates that, during the imposition of the price control, owners of MPDU properties are, on average, substantially less likely to extract equity from their homes relative to owners of market-rate units, but that the propensity to borrow increases differentially for MPDU owners following the expiration of the price control. The coefficient estimate on the *MPDU* × *Post* interaction term implies that expiring price controls lead to a 4.1 percentage point increase in the probability of extracting equity among owners of previously controlled units. This effect is large and represents an approximate 100% increase over the preperiod mean of 3.9 percentage points among MPDU owners reported in the bottom panel of the table. Adding

subdivision fixed effects and their interaction with a linear time trend in columns (2) and (3) hardly changes the coefficient. Comparing the MPDU main effect with the interaction term implies that expiring price controls close between 70% and 80% of the gap in equity extraction probabilities that exists between owners of MPDUs and non-MPDUs during the period of price control. Including ownership spell fixed effects in column (4), which restricts the comparison to using only within-owner variation in equity extraction, reduces the coefficient on the interaction term only slightly to 3.4 percentage points. Finally, estimating the marginal effects via probit or logit specifications in columns (5) and (6) also does not meaningfully change the magnitude of either the MPDU main effect or its interaction with the *Post* indicator. Taken together, the results presented in Table III suggest that, in response to the increase in collateralized borrowing capacity induced by the expiring price control, MPDU owners increase their annual probability of home equity extraction by roughly 4 percentage points, which corresponds to an increase of approximately 100% over their preperiod average propensity to borrow.

Panel A of Figure 6 plots the dynamics of the effect of expiring price controls on the extensive margin probability of extracting equity. This figure is directly analogous to Figure 5 and is constructed from the coefficient estimates on the relative year main effects and their interaction with the *MPDU* "treatment" dummy as specified in equation (11). The regression from which these coefficient estimates are generated includes all the same controls as the specification in column (3) of Table III. The series in orange squares shows the trend in equity extraction among owners of non-MPDU properties, while the series in blue circles shows the trend and 95% confidence intervals for MPDU owners. Starting in the year the first MPDU price control expires, the MPDU trend diverges sharply from its preperiod trend, while the trend for non-MPDU owners remains smooth. Consistent with the parallel trends assumption, the equity extraction probabilities among owners of MPDUs and non-MPDUs move together in the period prior to the expiration of the price control and only diverge starting in the year of expiration. Furthermore, almost all of the increase in equity extraction among MPDU owners occurs in exactly the year the first price control expires. This suggests that MPDU owners are responding directly to the increase in access to collateral induced by the expiring price control rather than to changes in their expectation as to whether they would need to sell the home prior to the end of the control period.

C.2. Combined Extensive and Intensive Margin Borrowing Responses

While the results in the previous subsection provide evidence that the likelihood of extracting equity responds significantly to the expiration of the MPDU price controls, they say nothing with respect to how the *amount* of equity extracted responds. Borrowers may respond to the relaxation of the collateral constraint both on the extensive margin and by increasing the amount they borrow conditional on extracting equity. This section presents estimates of the



Panel A. Probability of Extracting Equity

Panel B. Total Equity Extracted ($1,000s)

Figure 6. Dynamic effects of expiring price controls on the borrowing behavior of MPDU homeowners. This figure reports estimates of the effect of expiring price controls on the borrowing behavior of MPDU homeowners 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 are constructed by regressing an indicator for whether the homeowner extracted equity in a particular year (Panel A) and the total amount of equity extracted per year (Panel B) on an indicator for whether the associated 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 annual equity extraction probabilities (Panel A) and average equity extracted per year (Panel B) 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 that are clustered at the subdivision level. The regressions also include 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. (Color figure can be viewed at wileyonlinelibrary.com)

combined effect of these two margins of adjustment on the annual amount of equity extracted among owners of MPDU homes.

Table IV presents results from estimating the pooled difference-in-differences regression using the total amount of equity extracted in each year as the dependent variable instead of an indicator for equity extraction as before. In years when homeowners do not extract equity, this variable is set equal to zero; in years when they do extract equity, it is set equal to the sum of all nonpurchase originations and cash withdrawn through cash-out refinances during that year. The first four columns of the table present results from OLS regressions that are directly analogous to those presented for the extensive margin response. To

Table IV

The Effect of Expiring Price Controls on Total Equity Extracted per Year (in $1,000s) among Owners of MPDU Properties

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. 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) to (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.

	OLS				Tobit
	(1)	(2)	(3)	(4)	(5)
MPDU	–2.572***	–3.514***	–3.607***		–5.939***
	(0.755)	(0.664)	(0.693)		(0.632)
MPDU × Post	2.319***	2.926***	3.037***	2.794***	4.407***
	(0.715)	(0.664)	(0.700)	(0.880)	(0.667)
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	483,805	483,805	483,805	483,805	483,805

take into account the fact that, in many years, homeowners do not extract any equity, the fifth column presents results based on a Tobit specification where the equity extraction variable is treated as being censored from below at zero. This specification explicitly adjusts for the fact that the decision to extract equity may be made separately from the choice of how much to extract by estimating separate equations for the "participation" and "amount" decisions. To make the interpretation of the estimates consistent across columns, in the fifth column, I report the marginal effects for the expected value of the censored outcome implied by the estimated Tobit coefficients.

As with the extensive margin, the estimated response of total equity extracted is positive, statistically significant, and relatively stable across all specifications. The coefficient estimates on the *MPDU* × *Post* indicator from

the OLS specifications in columns (1) to (4) imply that expiring price controls lead to an increase in the average amount of equity extracted per year of roughly $2,300 to $3,000. The Tobit marginal effects are bigger and imply an increase of approximately $4,400. These effects are large relative to the $2,600 preperiod mean amount of equity extracted among MPDU owners. Comparing the *MPDU* × *Post* coefficient with the *MPDU* main effect suggests that expiring price controls close between 75% and 100% of the preperiod gap in equity extraction between owners of MPDUs and non-MPDUs. The dynamics of the effect are also shown in Panel B of Figure 6 and mirror the results for the extensive margin reported in Panel A. The average amount of equity extracted jumps sharply among MPDU owners in the year the price control expires and remains high relative to its preperiod level for the remainder of the sample period. There is also no evidence of differential trends for MPDUs and non-MPDUs during the preperiod. While the estimates on the relative year effects for MPDU properties are less precise due to the additional variation introduced by including the intensive margin response, the conclusion remains the same: expiring price controls lead to a substantial increase in home equity-based borrowing among owners of previously controlled units.

C.3. The Marginal Propensity to Borrow Out of Increases in Housing Collateral

A rough gauge of the economic magnitude of the borrowing responses I estimate can be provided by combining the estimates of the increase in collateralized borrowing capacity implied by the transaction price regressions reported in Table II and the results on total equity extraction just discussed in Table IV. Specifically, the price results in Table II imply that expiring price controls lead to an increase in transaction prices at previously controlled MPDUs that ranges between $66,000 and $106,000. Assuming that MPDU owners are able to borrow against up to half of the price increase, this implies an increase in collateralized borrowing capacity of roughly $33,000 to $53,000. The results in Table IV imply that, following the expiration of the price control, borrowers increase the average amount of equity that they extract from their homes by roughly $2,300 to $4,400 per year. Applying those estimates to the year the price control expires implies a marginal propensity to borrow out of increases in housing collateral that ranges from $0.04 to $0.13. While the existing literature does not provide any directly comparable estimates of how homeowner borrowing responds to a pure relaxation of the housing collateral constraint, one potentially useful way to benchmark these estimates is to compare them to estimates of how much borrowing responds to changes in house prices, which range from roughly $0.06 to $0.25 per dollar.[34] A naive comparison of this sort would suggest that the estimates I find are quite large, though this comparison

[34] Haurin and Rosenthal (2006) estimate that a $1 increase in house prices leads to an increase of roughly $0.13 to $0.16 in total household debt. Disney and Gathergood (2011) estimate a smaller effect on total debt that ranges between $0.06 and $0.10 and is similar to the results of Bhutta and Keys (2014), who focus explicitly on home-equity debt and provide estimates that imply a

must be made with caution. House prices and housing wealth are intentionally held constant in my setting, making it difficult to draw a direct parallel to a situation in which house prices are changing. Moreover, the relaxation of the collateral constraint associated with the expiring price control is likely much larger than most households typically experience.

D. Heterogeneity in Borrowing Responses by Initial LTV

In this section, I provide further evidence that the increase in home equity extraction among owners of MPDUs following the expiration of the price control is driven by the relaxation of previously binding collateral constraints. To do so, I examine heterogeneity in the magnitude of the borrowing response across the distribution of initial leverage. If collateral constraints are driving the response, then we might expect those whose borrowing capacity was most limited prior to the expiration of the price control to respond more aggressively. While a borrower's initial leverage is endogenous and may be correlated with other unobservable factors determining equity extraction, it is also a relatively direct measure of collateralized borrowing capacity. Thus, evidence that borrowers with higher initial leverage respond more aggressively to the expiring price control would be consistent with a role for binding collateral constraints.

To test whether borrowers with higher initial leverage are more responsive, I restrict attention to the set of properties that are observed to transact at least once during the sample period and to the set of ownership spells that begin with a transaction. This restriction is imposed so that I can accurately measure the initial LTV ratio associated with each ownership spell. Ownership spells are then grouped into four categories based on their initial LTV: (1) less than or equal to 70%, (2) between 70% and 80%, (3) between 80% and 95%, and (4) greater than 95%. Using these groups, I estimate the following regression:

$$y_{ijst} = \eta_j + \delta_t + \alpha_s \cdot t + X'_{ijt}\gamma + \sum_{k=1}^{4} \beta_k \cdot MPDU_i \times Post_{st} \times LTV_{jk} + \epsilon_{ijst}, \quad (12)$$

where y_{ijst} is an equity extraction outcome measured at time t and associated with ownership spell j of property i located in subdivision s, η_j are ownership spell fixed effects, δ_t are year fixed effects, $\alpha_s \cdot t$ is a subdivision-specific linear time trend, and X_{ijt} is a vector of property and ownership-spell characteristics. The primary variables of interest are the interaction terms involving the LTV_{jk} variables, which are a set of dummy variables indicating which of the four LTV groups the ownership spell belongs to. Because the specification includes ownership spell fixed effects, all time-invariant characteristics associated with either the property or the ownership spell drop out so that the vector X_{ijt} includes only the property age fixed effects and their interaction with the condo

marginal propensity to borrow of roughly \$0.07. Mian and Sufi (2011, 2014), who focus exclusively on the most recent housing cycle, estimate much larger marginal propensities to borrow of \$0.25 and \$0.19, respectively.

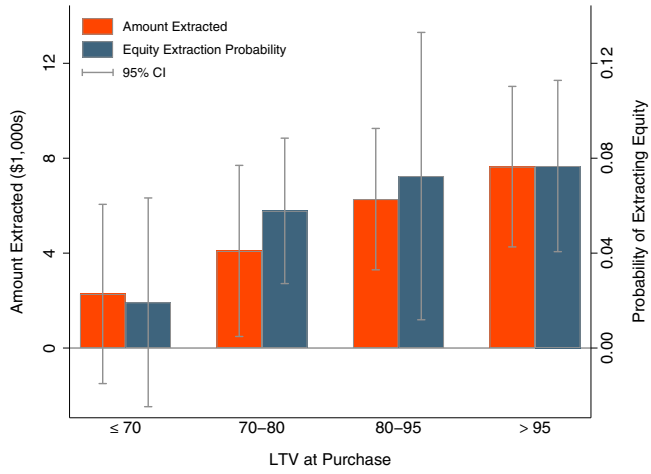


Figure 7. Heterogeneity across the distribution of initial leverage in the effect of expiring price controls on the borrowing behavior of MPDU homeowners. This figure reports estimates of the effect of expiring price controls on the borrowing behavior of MPDU homeowners derived from a difference-in-differences regression that allows the effect to vary according to the homeowner's initial LTV. Estimates for the extensive margin probability of extracting equity are shown in blue bars and measured along the right axis, while estimates for the total amount of equity extracted per year are shown in orange bars and measured along the left axis. The 95% confidence intervals for each estimate are also shown and are based on standard errors that are clustered at the subdivision level. The height of each bar corresponds to the coefficient estimate on the triple interaction term between an indicator for whether the property is an MPDU, an indicator for whether the year of observation falls on or after the year the first price control in the relevant subdivision expired, and an indicator for whether the initial LTV for the ownership spell fell within the range indicated on the x-axis. The regressions also include fixed effects for the ownership spell, the year of observation, and the age of the property in that year, as well as subdivision-specific linear time trends and the interaction between the age fixed effects and an indicator for whether the property is a condo or townhome. To measure initial leverage accurately, the sample is restricted to the set of properties that were observed to transact at least once and to the set of ownership spells that began with a transaction ($N = 211{,}249$). (Color figure can be viewed at wileyonlinelibrary.com)

dummy as well as the *Post* main effect. There are no main effects for the *MPDU* dummy or the LTV group indicators.

The β_k coefficients measure how the effect of the expiring price control varies across the distribution of initial leverage by comparing within-owner changes in borrowing behavior following the expiration of the price control across owners with different initial LTVs. Figure 7 plots these coefficient estimates along with their 95% confidence intervals for both the extensive margin equity extraction indicator (shown in blue bars and measured along the right axis) and the total amount of equity extracted per year (shown in orange bars and measured along the left axis). In both cases, the estimated effects for the lowest LTV group are small and statistically indistinguishable from zero, while the effects for the higher LTV groups are all statistically significant and increase monotonically

in initial leverage.[35] That is, MPDU owners whose initial debt is high relative to the controlled price and whose collateralized borrowing capacity is therefore most limited during the imposition of the price control are precisely the set of borrowers who are most likely to respond to its elimination by extracting equity from their homes.

E. Heterogeneity in the Borrowing Response by Income

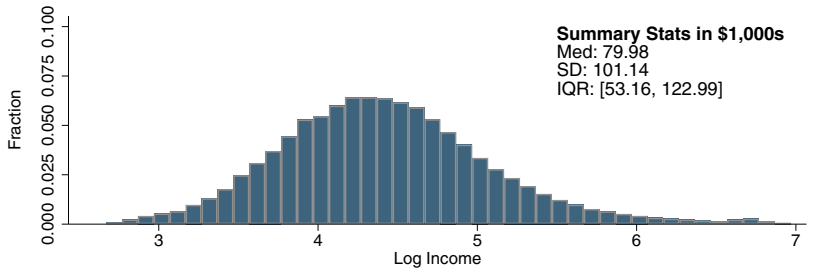
While the natural experiment afforded by the expiring MPDU price controls greatly enhances the internal validity of the estimated borrowing responses reported above, the eligibility requirements associated with the MPDU program raise several potential concerns regarding the external validity of those estimates. In particular, the income limit could potentially lead to either an over- or underestimate of the borrowing response if the population of interest is the typical U.S. homeowner. For example, if MPDU homeowners are drawn from the far left tail of the U.S. income distribution and lower income individuals tend to be more financially constrained, then the estimates may be too large. On the other hand, the estimates may also be too small given the presence of potentially binding payment-to-income (PTI) constraints that prevent lower income individuals from being able to act on relaxed collateral constraints to the same extent that higher income individuals can.

To address these concerns, I match a subset of the transactions data to loan application data made publicly available through the Home Mortgage Disclosure Act (HMDA).[36] The HMDA data provide loan-level information on borrower income for nearly all home mortgage applications filed in the United States. Using this matched sample, I am able to explore differences between the income distribution of MPDU homeowners and that of the typical U.S. homeowner as well as test for evidence of heterogeneity in borrowing responses by income within the sample of MPDU owners.

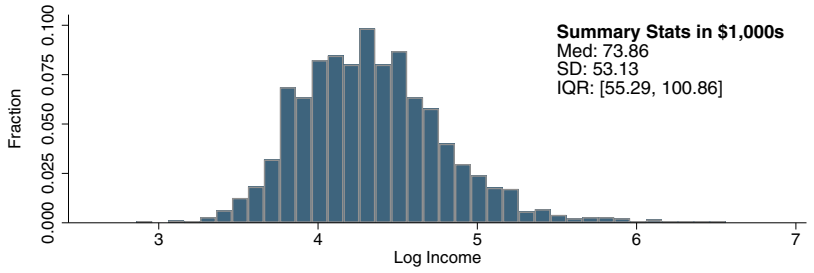
Panel A of Figure 8 plots the distribution of (log) homebuyer income for the set of all approved purchase-mortgage applications filed in the United States between 1997 and 2012. Panel B reports the distribution for the subsample of mortgage applications that match to an MPDU property transaction in the primary analysis sample. Each panel also reports the median, standard deviation, and interquartile range in levels ($1,000s) for the plotted log income distribution. As the figure shows, the incomes of the households who purchase the price-controlled units are, in fact, quite similar to the income of the typical

[35] While the standard errors are relatively wide, one-sided hypothesis tests for the difference between the highest LTV group and the lowest LTV group fail to reject the null hypothesis that the effects are larger among those in the higher group at the 5% level for both outcomes. Similarly, a one-sided test for whether the effects are larger among those in the two highest groups relative to those in the two lowest groups fails to reject the null at the 10% level for the extensive margin response and at the 5% level for the total equity extraction measure.

[36] The matching procedure, which is detailed in the Internet Appendix, uses information on the primary loan amount, lender name, Census tract, loan type (conventional, Federal Housing Administration (FHA), or Department of Veteran's Affairs (VA)), and origination year.



Panel A. National Sample



Panel B. MPDU Only Sample

Figure 8. Distribution of homebuyer income. This figure plots, for two separate samples, the distribution of (log) homebuyer income (in real 2012 $1,000s) as reported on loan applications contained in the HMDA data. Panel A shows the distribution for the set of all approved purchase-mortgage applications filed in the United States between 1997 and 2012 for which the borrower reported a nonmissing income (N = 64,267,113). Panel B restricts the sample to include only applications that match to a transaction in the analysis sample that involved an MPDU property (N = 1,749). Each panel also reports the median, standard deviation, and interquartile range in levels for the plotted log income distribution. A bin width of 10 log points is used in both panels. (Color figure can be viewed at wileyonlinelibrary.com)

homebuyer in the United States during my sample period. While the income distribution among buyers of price-controlled homes is more compressed, it is roughly centered around the median of the national distribution and spans a large portion of the interquartile range of that distribution. This suggests that the estimated borrowing responses reported above are not driven by a population of borrowers who are drawn from a particularly unrepresentative portion of the national income distribution.[37]

Nonetheless, the distributions are not identical and MPDU owners are, of course, not a truly representative sample. For this reason, it is also important to gauge whether there is any significant heterogeneity in the borrowing response across income levels. If there are meaningful differences even *within* the selected sample of MPDU owners, this may raise concerns that those

[37] While I am able to show that the income distributions are similar between MPDU owners and typical homebuyers, it may still be the case that the wealth distributions differ. Unfortunately, the HMDA data do not provide any information on an applicant's assets.

differences would be further magnified when comparing MPDU owners to a typical homeowner. Alternatively, given that the income distribution of MPDU owners spans a significant portion of the national distribution, a finding of no meaningful heterogeneity within this sample would provide additional confidence that the main results have external validity beyond the specific setting in which they are estimated.

To test whether the borrowing response differs for lower versus higher income MPDU owners, I restrict attention to the set of ownership spells that begin with a transaction that can be matched to an HMDA loan application. I then categorize ownership spells as high income if the buyer's income falls above the median income reported by all MPDU buyers and estimate versions of the following regression:

$$\begin{aligned} y_{ijst} = \eta_j + \delta_t + \alpha_s \cdot t + X'_{ijt}\gamma + \beta_1 \cdot MPDU_i \times Post_{st} \\ + \beta_2 \cdot MPDU_i \times Post_{st} \times HighIncome_j + \epsilon_{ijst}, \end{aligned} \tag{13}$$

where $HighIncome_j$ is an indicator for whether the income associated with ownership spell j falls above the median MPDU buyer income and all other variables are as defined in equation (12). The primary coefficient of interest is β_2, which provides an estimate of the extent to which the borrowing response differs for higher income versus lower income MPDU owners. As with the LTV results reported in Section V.D, the inclusion of ownership spell fixed effects means that all time-invariant characteristics associated with either the property or ownership spell drop out and that the vector X_{ijt} therefore includes only the property age fixed effects and their interaction with the condo dummy as well as the *Post* main effect. There are no *MPDU* or high-income main effects. The inclusion of ownership spell fixed effects also implies that β_2 is identified by comparing within-owner changes in borrowing behavior before and after the expiration of the price control across owners who are higher versus lower income.

Table V reports the results from estimating equation (13) using both an indicator for equity extraction (columns (1) to (3)) and the total amount of equity extracted in each year (columns (4) to (6)) as the dependent variable. Columns (1) and (4) include only the ownership spell fixed effects and calendar year fixed effects as controls. Columns (2) and (5) further include a set of property age fixed effects and their interaction with an indicator for whether the property is a condo or townhome. Columns (3) and (6) also include a full set of linear subdivision time trends. While dropping ownership spells that could not be matched to an HMDA loan application leads to a loss of statistical power in the extensive margin specifications, the point estimate on the *MPDU* × *Post* term, which represents the effect among lower income homeowners, is of roughly similar magnitude to the estimates reported from the full sample in Tables III and IV.[38] Moreover, the coefficient on the triple interaction term

[38] When the two regressions are estimated jointly in a seemingly unrelated regressions (SUR) framework, I fail to reject the null of equal coefficients at conventional levels of statistical signif-

Table V

Heterogeneity by Income in the Effect of Expiring Price Controls on the Borrowing Behavior of MPDU Homeowners

This table reports estimates of the effect of expiring price controls on the borrowing behavior of MPDU homeowners derived from a difference-in-differences regression that allows the effect to vary based on the homeowner's income as reported on their HMDA loan application. Estimates for the extensive margin probability of extracting equity are reported in columns (1) to (3), while estimates for the total amount of equity extracted per year are reported in columns (4) to (6). The first row reports the coefficient on the interaction between an indicator for whether a property is an MPDU and an indicator for whether the year of observation falls on or after the year the first price control in the relevant subdivision expired. The second row reports the coefficient on the triple interaction between an indicator for whether a property is an MPDU, an indicator for whether the year of observation falls on or after the year the first price control in the relevant subdivision expired, and an indicator for whether the homeowner's reported income falls above the median income reported among all MPDU buyers. All specifications include fixed effects for the ownership spell as well as the year of observation. The property age fixed effects are included both directly and interacted with an indicator for whether the property is a condo or townhome. Subdivision trends are estimated by interacting a full set of subdivision fixed effects with a linear time trend. The sample is restricted to the set of ownership spells that began with a transaction that could be matched to an HMDA loan application. Standard errors are reported in parentheses and are clustered at the subdivision level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

	Probability of Extracting Equity			Total Equity Extracted ($1,000s)		
	(1)	(2)	(3)	(4)	(5)	(6)
MPDU × Post	0.037	0.040	0.041	4.346***	4.495***	4.982***
	(0.028)	(0.028)	(0.029)	(1.295)	(1.320)	(1.348)
MPDU × Post × High-Income	0.005	0.003	−0.000	−0.279	−0.454	−0.979
	(0.042)	(0.042)	(0.043)	(1.865)	(1.837)	(1.954)
Ownership Spell FEs	X	X	X	X	X	X
Year FEs	X	X	X	X	X	X
Property Age FEs		X	X		X	X
Subdivision Trend			X			X
Number of Observations	178,045	178,045	178,045	178,045	178,045	178,045

between *MPDU* × *Post* and the high-income dummy is both statistically and economically insignificant in every specification. This implies that there is no meaningful heterogeneity in the borrowing response across the distribution of income within the sample of MPDU owners that drive the main results. Given that the distribution of income for MPDU owners spans a nontrivial portion of the national homebuyer income distribution, these results suggest that the main estimates likely have external validity beyond the MPDU program itself.

icance. For example, a test of the equality of the coefficients reported in column (4) of Table III and column (3), row 1 of Table V yields $p = 0.82$. Similarly, a test of the equality of the coefficients reported in column (4) of Table IV and column (6), row 1 of Table V is only marginally statistically significant at the 10% level ($p = 0.09$).

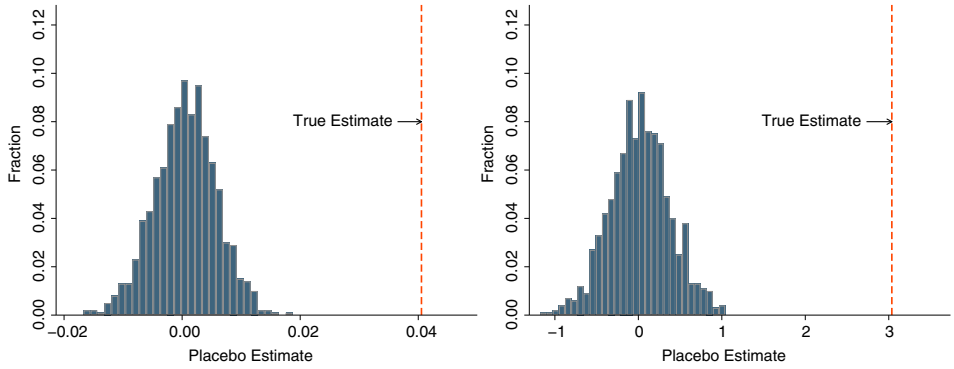
F. Additional Robustness Checks

F.1. Placebo Tests

As a further test of the parallel trends assumption underlying the main difference-in-differences estimates provided in Section V.C, I also conduct a series of placebo tests for the effect of the price control on borrowing behavior. Each placebo estimate is generated by randomly assigning a false first MPDU expiration date to each of the 69 subdivisions. Using those false dates, I then replicate the pooled difference-in-differences estimates for the extensive margin probability of equity extraction and the total amount of equity extracted per year using the specification that includes all of the property characteristics as well as the subdivision fixed effects and their interaction with a linear time trend. To prevent the placebo estimate from being influenced by any jump in the outcome at the true expiration date, I only use data from either the preperiod or the postperiod depending on whether the false date falls before or after the true first expiration date for the relevant subdivision. This exercise is repeated 1,000 times and the distribution of the resulting coefficients for each outcome is plotted in Figure 9. The true estimate is also shown in the figure using a vertically dashed line. The true estimates are taken from column 3 of Table III for the extensive margin response and from column (3) of Table IV for the total equity extraction response. As is clear from the figure, the true estimates are far larger than any of the placebo estimates, and the distribution of placebo estimates for both outcomes is centered around zero. This suggests that the results I find are unlikely to have been generated by pure chance and lends further validity to the identifying assumption of parallel trends.

F.2. Matching Estimates

Another potential concern with the main difference-in-differences estimates provided in Sections V.B and V.C 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 the Internet Appendix, I report the results from an alternative approach to estimating the effect of the expiring price control using a semiparametric propensity score matching estimator (see, for example, Heckman, Ichimura, and Todd (1997), Heckman et al. (1998)). This approach, which is described in detail in the Internet Appendix, alleviates the concern over covariate imbalance by restricting attention to a set of properties with overlapping characteristics and constructing 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. The results from this approach are reported in Internet Appendix Table IA.VIII and yield estimates for the effect of the expiring price control on transaction prices, the annual probability of equity extraction, and the total amount of equity extracted per year that are all qualitatively similar to those reported above.



Panel A. Probability of Extracting Equity

Panel B. Total Equity Extracted ($1,000s)

Figure 9. Placebo tests of the effect of expiring price controls on the borrowing behavior of MPDU homeowners. This figure reports results from a series of placebo tests of the effect of expiring price controls on the probability of extracting equity (Panel A) and the total amount of equity extracted per year (Panel B) among owners of MPDU properties. Each panel plots the distribution of 1,000 placebo estimates for the indicated outcome. The vertically dashed lines show the true estimates, which are taken from column (3) of Table III for Panel A and column (3) of Table IV for Panel B. Each placebo estimate is created by randomly assigning a false first MPDU expiration date to each of the 69 subdivisions and generating a difference-in-differences estimate using that false date. To prevent the placebo estimate from being influenced by any jump in the outcome at the true expiration date, only data from either the preperiod or the postperiod are used depending on whether the false date fell before or after the true first expiration date within the relevant subdivision. In addition to the *MPDU* main effect and its interaction with the *Post* indicator, the regressions used to generate the placebo estimates include year fixed effects, the *Post* main effect, 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. (Color figure can be viewed at wileyonlinelibrary.com)

F.3. Alternative Specifications

As a final robustness check, I also explore alternative ways of estimating the effect of the expiring price control that exploit the exact timing of an individual property's price control expiration and that do not rely on using the outcomes of non-MPDU properties as a counterfactual. These results are all reported in the Internet Appendix. For MPDU properties, I define a treatment dummy that takes the value 1 in years when that property's price control has expired and 0 otherwise. Depending on the specification, non-MPDU properties are then either dropped from the sample entirely or considered to be always treated. I then run regressions of each of the three main outcomes on this treatment dummy while also controlling for property, age, and subdivision-by-year fixed effects. In these regressions, the effect of the expiring price control is therefore identified by comparing changes in prices and borrowing behavior among owners of MPDU properties before and after the expiration of their price control

relative to that of other MPDU properties in the same subdivision and year for which the price control has yet to expire. The results from this exercise are qualitatively similar to the main estimates reported above.

VI. Evidence on the Uses of Extracted Funds

The results in the previous section provide strong evidence that homeowners respond to increases in collateralized borrowing capacity by extracting additional equity from their homes. However, the real effects of this behavior depend on how the borrowed money is used. In particular, if homeowners simply use the extracted funds to pay off other existing debt or reinvest them into more liquid assets, then the effect of relaxing the collateral constraint on household consumption or investment expenditures would be significantly less than the effect on borrowing. While my data do not allow me to provide a full account of the uses of extracted funds, in this section, I present two pieces of evidence from the housing market that suggest that at least some portion of the borrowed money is used to fund consumption or home improvement expenditures.

A. Evidence from Home Improvement Permits

Focusing first on home improvement expenditures, I show that expiring price controls are associated with a disproportionate increase in the likelihood of applying for home improvement permits among owners of previously controlled units. Given the concomitant increase in equity extraction documented in the previous section, this suggests that at least some portion of the extracted money was used to fund new residential investments. Of course, this result also raises the concern that the observed increase in equity extraction may not be driven by access to new collateral but rather by the fact that expiring price controls could increase the owner's incentives to invest in debt-financed home improvements. While disincentives for residential investment have been shown to be important in the context of rent control (Autor, Palmer, and Pathak (2014)), I argue that they are unlikely to be the driving force behind the increase in equity extraction in this context for three reasons. First, the formula used to determine the controlled resale price takes into account any documented home improvements and adjusts the resale price upward dollar-for-dollar on a cost basis. Because of this, the MPDU price controls generate little disincentive for investment during the control period. Second, for owners who plan to stay in the home beyond the end of the control period, the expiration of the price control has no effect on the incentive to invest. These owners know at the time they make the investment that they will eventually receive half of its full market value, regardless of whether that investment is made before or after the price control expires. Finally, if the only factor driving the observed increase in equity extraction was a change in the demand for debt-financed home improvements, then such an effect should presumably manifest itself equally across all MPDU owners. However, as shown in the previous section, the increase in equity extraction is concentrated primarily among the set of homeowners with

high initial leverage, for whom collateral constraints are presumably more important. For these reasons, it seems likely that the direction of causality runs from equity extractions (induced by increased access to collateral) to home improvements and not the other way around.

I use data from the Montgomery County Department of Permitting Services to estimate the effect of expiring price controls on residential investment behavior. These data, which are described in further detail in the Internet Appendix, contain address-level information on all building and home improvement permit applications filed since 2000 for all parts of the county except for the cities of Gaithersburg and Rockville. I match the permit applications to the DataQuick property file using the same approach used to match the list of MPDU addresses. Having matched the data, I then construct an annual panel that records for each property located outside of Gaithersburg or Rockville whether a permit application was filed for that property in a particular year. The permit data includes applications for both new construction and improvements. To avoid confusing new construction with home improvements, I only include property-year observations that are at least two years after the year the property was built. The panel thus runs from 2000 to 2012, unless the property was built during that time period, in which case the data begins in the second year after the property was built.

Using this panel, I estimate versions of the pooled difference-in-differences regression given by equation (10) where the outcome is now an indicator for whether a home improvement permit application was filed for a property in a particular year. Table VI reports the coefficient estimates for the $MPDU \times Post$ interaction term, which measures the differential increase in the likelihood of applying for permitted residential investment among owners of MPDU properties following the expiration of the price control. Across the columns, the control variables and specifications are the same as those used to estimate the extensive margin equity extraction response and are introduced in the same order. While the smaller sample size leads to a modest loss of precision, all of the estimated effects are positive and significant at the 5% level. For the OLS specifications, the point estimates imply that expiring price controls lead to an increase in the probability of filing a home improvement permit application of roughly 1 percentage point, while the probit and logit specifications yield slightly lower estimates of roughly 0.6 to 0.7 percentage points. These effects are large relative to the preperiod mean of 1 percentage point among MPDU owners reported in the bottom panel and suggest that borrowers likely used some portion of the equity they extracted to fund new residential investment expenditures.

B. Evidence from Foreclosures

The second piece of evidence that I provide on the uses of extracted funds draws inferences based on the ex-post performance of the loan. If equity extraction is merely a means for portfolio diversification or paying off existing debt, then one would not necessarily expect the act of extracting equity itself

Table VI

The Effect of Expiring Price Controls on the Annual Probability of Permitted Residential Investment among Owners of MPDU Properties

This table reports difference-in-differences estimates of the effect of expiring MPDU price controls on the annual probability of permitted residential investment among owners of 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 owner filed an application for a home improvement permit in a particular year. The table reports the coefficient on the interaction term between the "treatment" dummy, denoting whether the property is an MPDU, and 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) to (4) report coefficient estimates from linear probability models, while columns (5) and (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. The sample excludes properties located in the cities of Gaithersburg and Rockville, where permit application data are not available, and property-year observations occurring prior to 2000, the first year that permit applications are observed. To avoid mistaking new construction for home improvements, the sample also excludes any property-year observations occurring less than two years after the property was built. Standard errors are reported in parentheses and are clustered at the subdivision level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

	OLS				Probit	Logit
	(1)	(2)	(3)	(4)	(5)	(6)
MPDU × Post	0.0089**	0.0086**	0.0092**	0.0136**	0.0071**	0.0063**
	(0.0034)	(0.0037)	(0.0037)	(0.0055)	(0.0031)	(0.0031)
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	0.0106	0.0106	0.0106	0.0106	0.0106	0.0106
Number of Observations	385,192	385,192	385,192	385,192	385,192	385,192

to expose borrowers to additional risk or raise their probability of mortgage default and subsequent foreclosure. On the other hand, if borrowers use some of the extracted funds to pay for current consumption and investment expenditures, then their total leverage should increase, potentially putting them at higher risk of default and foreclosure (Bhutta and Keys (2014)). In this section, I provide evidence that equity extractions induced by the increase in collateralized borrowing capacity at the time the price controls expire are more likely to end in foreclosure relative to equity extractions driven by other motives, which

suggests that they are also more likely to be used for the purposes of funding current expenditures.

While the DataQuick data do not allow me to track the time at which a particular loan becomes delinquent or enters foreclosure, it does contain an indicator for whether an ownership transfer occurred as a result of a foreclosure sale or bank repossession. Using this information, I am able to determine for every loan observed in the loan-level data whether that loan was followed by a subsequent foreclosure. To measure differences in foreclosure rates associated with individual instances of equity extraction, I restrict attention to loans in the nonpurchase loans data set that are coded as equity extractions and estimate versions of the following regression specified at the loan level:

$$Foreclosure_{ijst} = \alpha_s + \delta_t + X'_{ijt}\gamma + \beta_1 \cdot MPDU_j + \beta_2 \cdot MPDU_j \times Post_{st} + \epsilon_{ijst}, \quad (14)$$

where $Foreclosure_{ijst}$ is an indicator denoting whether equity extraction i associated with property j located in subdivision s and originated at time t was followed by a foreclosure within up to three years after its origination. Since the focus is on three-year foreclosure rates, I only include equity extractions that occur between 1997 and 2009 to ensure that I can observe up to three years of potential foreclosure information for every loan. In addition to the standard set of property characteristics (and the *Post* main effect), the vector X_{ijt} also includes dummy variables indicating whether the loan was FHA-insured or had an adjustable interest rate. The coefficient of interest is β_2, which measures the differential increase in three-year foreclosure rates associated with equity extractions secured against MPDU properties following the expiration of the price control relative to the change in foreclosure rates associated with equity extractions secured against non-MPDU properties. A positive value for β_2 suggests that equity extractions that occur in response to the increased collateral made available by the expiring price control are at higher risk of foreclosure relative to equity extractions motivated by other factors.

Table VII reports estimates of this coefficient from various versions of equation (14). All specifications include the standard set of property characteristics as well as dummies for FHA and adjustable rate mortgages and fixed effects for the year of origination and the age of the property in that year. The second column adds subdivision fixed effects, which are further interacted with a linear time trend in column (3). Columns (4) and (5) report probit and logit marginal effects using the same specification as in column (3). In all cases, the estimated effect on the $MPDU \times Post$ interaction term is positive and precisely estimated. The estimates imply that the three-year foreclosure rate associated with equity extractions secured against MPDU properties increased by roughly 1.5 to 2 percentage points relative to equity extractions secured against non-MPDU properties following the expiration of the price control. This effect represents between 70% and 90% of the overall average three-year foreclosure rate among equity extractions secured against MPDU properties. Equity extractions induced by expiring price controls are thus substantially more risky than those motivated by other reasons, which suggests

Table VII

The Effect of Expiring Price Controls on the Three-Year Foreclosure Rate among Equity Extractions Secured against MPDU Properties

This table reports difference-in-differences estimates of the effect of expiring MPDU price controls on the three-year foreclosure rate among equity extractions secured against MPDU properties. Each column reports a separate regression estimated at the loan level where the dependent variable is an indicator for whether the loan was followed by a foreclosure within three years of origination. The sample includes only nonpurchase loans coded as equity extractions that were originated during the period 1997 to 2009. The table reports the coefficient on the interaction term between the "treatment" dummy, denoting whether the property is an MPDU, and an indicator for whether the year of origination 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 origination and the age of the property in that year as well as the *Post* main effect and property and loan characteristics. 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. The loan characteristics include an indicator for whether the loan was FHA-insured and whether it had an adjustable interest rate. Subdivision trends are estimated by interacting the subdivision fixed effects with a linear time trend. Columns (1) to (3) report coefficient estimates from linear probability models, while columns (4) and (5) report marginal effects from probit and logit specifications. The mean of the dependent variable among MPDU properties 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.

	OLS			Probit	Logit
	(1)	(2)	(3)	(4)	(5)
MPDU × Post	0.017***	0.015***	0.015***	0.020***	0.020***
	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)
Property Characteristics	X	X	X	X	X
Loan Characteristics	X	X	X	X	X
Year and Age FEs	X	X	X	X	X
Subdivision FEs		X	X	X	X
Subdivision Trend			X	X	X
Dep. Var. MPDU Mean	0.022	0.022	0.022	0.022	0.022
Number of Observations	45,719	45,719	45,719	45,719	45,719

that they are also more likely to have been used for the purposes of funding current expenditures rather than simply paying off existing debt or portfolio diversification.

VII. Conclusion

This paper provides new evidence on the relationship between housing collateral constraints and household borrowing by exploiting a unique feature of an inclusionary zoning policy in Montgomery County, Maryland, which imposes temporary price controls and borrowing restrictions on owner-occupied housing units. Because the duration and stringency of these price controls are set by

formula and known in advance at the time of purchase, their expiration has no effect on the owner's expected lifetime wealth but directly affects their collateralized borrowing capacity. Changes in borrowing behavior among owners of controlled units at the time of expiration can thus be directly attributed to the effect of the price control on the owner's collateralized borrowing capacity. Using this fact, I show that changes in access to housing collateral have important effects on homeowner borrowing behavior. Specifically, following the expiration of the price controls, the probability of home equity extraction increases by roughly 4 percentage points for owners of previously controlled units relative to owners of observationally identical noncontrolled units in the same housing development. Comparing the increase in equity extraction to the increase in available collateral implied by the change in prices at the time of expiration yields an estimate of the marginal propensity to borrow out of increases in housing collateral of approximately \$0.04 to \$0.13.

These results constitute the first direct estimates of the housing collateral effect that leverage time-varying and household-specific changes in access to housing collateral. Given that houses are the largest source of pledgeable collateral for most households, accurately estimating the magnitude of this relationship is important for understanding the microeconomic mechanisms driving the aggregate fluctuations in household debt that are commonly associated with business cycles. While a full accounting of the macroeconomic importance of housing collateral constraints is outside the scope of this paper, the estimates provided here are an important step toward that goal.

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Supporting Information

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Appendix S1: Internet Appendix.