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

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Abstract

I empirically analyze how changes to the collateral value of real estate assets affect homeowner borrowing behavior. While previous research has documented a positive relationship between house prices and home-equity based borrowing, a key empirical challenge has been to disentangle the role of collateral constraints from that of wealth effects in generating this relationship. To isolate the role of collateral constraints, I exploit the expiration of resale price controls on owner-occupied housing units created through an inclusionary zoning regulation in Montgomery County, Maryland. Because the duration and stringency of the price controls are set by formula and known in advance, their expiration has no effect on expected lifetime wealth but directly shocks collateralized borrowing capacity. Using data on all transactions and loans secured against every property in the county from 1997–2012, I estimate that the marginal propensity to borrow out of increases in housing collateral induced by expiring price controls is between \$0.04 and \$0.13. The magnitude of this effect is correlated with a homeowner's initial leverage and indicates that a significant fraction of the additional borrowing arising from house price increases is due to relaxing collateral constraints. Additional analysis of residential investment and ex-post loan performance further suggests that borrowers used at least some portion of the extracted funds to finance current consumption and investment expenditures. These results highlight the importance of collateral constraints for homeowner borrowing and suggest a potentially important role for house price growth in driving aggregate consumption.

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1 Introduction

By some accounts, over half of the mortgage debt accumulated in the U.S. during the run-up to the Great Recession can be directly attributed to the effect of rapidly rising house prices on the demand for home equity debt among existing homeowners (Mian and Sufi, 2011). This can be seen clearly in Figure 1, which plots aggregate trends in house prices and home equity debt relative to income over the period 1990–2014.[1] The pattern is stark. At the same time that house prices were rising, existing homeowners were taking out an increasingly large amount of debt against their homes—debt that they quickly began to off-load as prices collapsed.

Why do homeowners respond to rising house prices in this way? In standard models, an increase in house prices may lead homeowners to take on additional debt due to both a direct effect on household wealth—rising prices make homeowners feel richer—and an indirect effect on collateralized borrowing capacity—rising prices relax previously binding borrowing constraints tied to the value of the home. The goal of this paper is to isolate the empirical relevance of the latter channel by studying how the borrowing behavior of individual homeowners responds to changes in the collateral value of their homes.

Isolating the independent effect of collateral constraints from that of wealth effects is important from the perspective of macroeconomic policy because the two mechanisms have markedly different implications for the way in which house price changes spill over into aggregate economic activity. By propagating the effects of small shocks throughout the economy, increases in homeowner borrowing driven by the relaxation of binding collateral constraints have the potential to generate large swings in aggregate consumption (Bernanke and Gertler, 1989; Kiyotaki and Moore, 1997; Iacoviello, 2005). In contrast, increases in homeowner borrowing driven by wealth effects are likely to have a limited impact since they will typically be offset by decreases among renters, for whom higher house prices represent a *negative* wealth shock (Sinai and Souleles, 2005; Campbell and Cocco, 2007; Buiter, 2008). Therefore, knowing whether and to what extent collateral constraints drive individual homeowner borrowing behavior is central to our understanding of how house price changes affect the real economy and to the debate over how monetary policy should respond to such changes in prices.

Existing empirical research, however, has struggled to provide direct estimates of the effect of collateral values on homeowner borrowing. Two key challenges have hindered progress. First, it is difficult to identify situations in which changes to the collateral value of a house occur independently from changes to the owner's housing wealth. As a result, most analyses have fo-

[1] Greenspan and Kennedy (2005, 2008) present similar time-series evidence using a broader measure of home equity withdrawal that includes the proceeds from cash-out refinances and home sales. The measure used in this figure includes only equity extraction occurring through home mortgages secured by junior liens and home equity lines of credit.

cused primarily on the *overall* effect of house prices on borrowing while attempting to infer the role of collateral constraints through the use of indirect proxies—proxies that in many cases conflate credit demand with credit supply (Gross and Souleles, 2002; Agarwal et al., 2007).[2] Second, as in most empirical analysis, omitted variables and simultaneity biases loom large. Aggregate shocks to joint determinants of house prices and homeowner borrowing, such as interest rates and expected future income, make it exceedingly difficult to draw causal inferences from naturally occurring changes in house prices, even when one is only interested in the overall relationship between prices and borrowing.[3]

In this paper, I make use of an alternative approach to contribute new empirical estimates of the causal effect of housing collateral on home equity-based borrowing. To isolate the effect of collateral values from generalized wealth effects, I exploit a unique feature of local land use policy in Montgomery County, Maryland that drives a wedge between the value of a house as collateral and its value as a component of homeowner wealth. Since 1974, housing developers in Montgomery County have been subject to an inclusionary zoning regulation known as the Moderately Priced Dwelling Unit (MPDU) program. This policy requires developers to set aside at least 12.5 percent of all housing units in new developments to be made available at controlled prices to moderate-income households.[4] These housing units are subject to deed restrictions that cap their resale prices for a period of time ranging between 5 to 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

[2]Recent papers using this approach to study various determinants of equity extraction include Hurst and Stafford (2004); Yamashita (2007); Disney and Gathergood (2011); Mian and Sufi (2011, 2014); Cooper (2013) and Bhutta and Keys (2014). There are also a host of studies using this approach to study consumption responses to house price changes, many of which are reviewed in Bostic et al. (2009). 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, as discussed in more detail below, these studies use national and state-level policy variation, making it difficult to separately identify aggregate trends from household-specific changes to collateral constraints.

[3]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 et al., 2013). However, as cautioned by Saiz (2010) and further emphasized by Davidoff (2011, 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. Moreover, as Davidoff (2014) demonstrates, physical building constraints were not correlated with changes in the size of the housing stock during the 2000s, suggesting that the correlation between house prices and building constraints was not necessarily operating through the constraints themselves during that period.

[4]For a four-person household, the maximum income limit is set at 70 percent of the median family income for the Washington, D.C. metropolitan area as published annually by the U.S. Department of Housing and Urban Development. As of 2014, that limit is $75,000, which is roughly 17 percent higher than the national median family income.

on the owner's total expected lifetime wealth. However, expiring price controls directly affect the collateral value of the home through the relaxation of the borrowing restrictions. Leveraging this fact, I show within the context of a stylized model of home equity-based borrowing that 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 values on homeowner borrowing. I then use that 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 collateral value.

To conduct my analysis, I assemble a unique dataset 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 1997–2012. I combine this information with administrative records from the Montgomery County Department of Housing and Community Affairs, which identify the restricted housing units and the dates for which the applicable price controls were in effect. This dataset allows me to identify the effect of expiring price controls by comparing how the borrowing behavior and prices paid by 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.

I find compelling evidence that increases in collateral values lead homeowners to extract equity from their homes. In housing developments containing controlled units, transaction prices for the controlled units increase by roughly 40–65 percent relative to observationally identical non-controlled units in the same development following the expiration of the price controls. In response to these price gains, owners of controlled units are roughly four 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 non-controlled units. This effect is large, representing an almost 100 percent increase over the pre-expiration mean probability of equity extraction among owners of controlled units. Both the price effect and the increase in equity extraction among owners of controlled units are immediately present in the year the price control expires and almost perfectly offset the gap that exists between owners of controlled and non-controlled units during the imposition of the price control.

Using information on the size of individual loans, I convert these figures into an estimate of the marginal propensity to borrow against an increase in housing collateral. On average, I find that a \$1 increase in collateral values leads homeowners to extract between \$0.04–\$0.13 in additional home equity debt. To put this into context, estimates from the literature of the *overall* effect of house prices on homeowner borrowing, which combine both collateral and wealth effects, range between \$0.06–\$0.25.[5] Thus, my estimates imply that collateral constraints can explain a sizable fraction of the effect of house price increases on homeowner borrowing, even in the absence of any changes in perceived wealth.

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 collateral values 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.

My empirical strategy identifies the effect of collateral values 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 re-estimate 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 semi-parametric

[5] See Haurin and Rosenthal (2006); Disney and Gathergood (2011); Mian and Sufi (2011, 2014), and Bhutta and Keys (2014).

propensity score matching estimator.

While my results provide clear evidence that homeowners respond to increases in housing collateral 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 rising collateral values 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, two pieces of evidence from the housing market suggest 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 0.6–1 percentage points among owners of price-controlled units following the expiration of the price control. This effect represents an increase of approximately 60–100 percent over the pre-expiration 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 contains 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 1.5–2 percentage points relative to equity extractions secured against non-MPDU properties following the expiration of the price control. This result is consistent with previous findings regarding the increased risks associated with house price-induced 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).

Related Literature

My findings build on a large empirical literature studying the effect of house prices on household consumption, savings, and borrowing behavior. The most directly related strand of this litera-

[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 et al. (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 in my context for three reasons. First, the formula used to determine the controlled resale price is adjusted upward dollar-for-dollar to reflect documented home improvements and therefore generates 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 expected return from home improvements is determined based on the market price. For these owners, the exact timing of the expiration therefore has no effect on investment incentives. Finally, if the only factor driving the increase in equity extraction were changes to the demand for debt-financed home improvements, then one would not necessarily expect the equity extraction effect to be concentrated only among the set of borrowers with high initial leverage.

ture has focused explicitly on the effect of house prices on home equity-based borrowing and has found consistent evidence that homeowners respond to increases in house prices by extracting equity from their homes (see, for example, Dynan and Kohn, 2007; Yamashita, 2007; Cooper, 2010, 2013; Disney and Gathergood, 2011; Mian and Sufi, 2011, 2014; Bhutta and Keys, 2014).[7] A different but closely related branch of this literature has also found consistently positive consumption responses to changes in house prices (see, for example, Skinner, 1989, 1996; Engelhardt, 1996; Lehnert, 2004; Benjamin et al., 2004; Case et al., 2005, 2013; Haurin and Rosenthal, 2006; Campbell and Cocco, 2007; Bostic et al., 2009; Gan, 2010; Carroll et al., 2011; Carroll and Zhou, 2012; Browning et al., 2013; Calormiris et al., 2013; Mian et al., 2013). Many studies have attempted to infer the role that collateral constraints play in generating these relationships through the use of indirect proxies for constraints. 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 indirect nature of the proxy measures employed has left open the possibility that such estimates may be confounding differences in collateral constraints with differences in preferences.

In response to this limitation, an alternative approach has been to explore how borrowing and spending behavior responds to explicit policy-induced relaxation or tightening of collateral constraints.[9] Three recent papers taking this approach and which are closely related to this paper bear mentioning. Leth-Petersen (2010) and Agarwal and Qian (2014) provide evidence that homeowners' spending and borrowing behavior responds to national changes in the ability to use home equity debt for consumption purposes in Denmark and to the amount of time homeowners must live in their residences before being able to access home equity through cash-out refinances in Singapore, respectively. However, since both of these studies examine national policy changes that do not vary at the individual level, the authors again must rely on indirect proxy measures to determine which households would be most affected by changes in the ability to borrow against home equity. Moreover, the policy changes studied in these papers occur at the same time for

[7]A related literature studies non-house price determinants of the demand for home equity debt, including income shocks (Hurst and Stafford, 2004) and various other sources of macroeconomic uncertainty (Chen et al., 2013). There is also an extensive literature studying the financial incentive to refinance an existing mortgage without taking on additional home equity debt in response to falling interest rates from an option theoretic point of view (see, for example, Agarwal et al., 2013; Keys et al., 2014, and references therein.)

[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 et al., 1998; Parker, 1999; Souleles, 1999; Browning and Collado, 2001; Johnson et al., 2006; Agarwal et al., 2007; Stephens, 2008; Aaronson et al., 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 (2014)).

[9]This approach is motivated by the work of Gross and Souleles (2002) and Alessie et al. (2005), both of which study the effects of supply side changes in access to credit in the consumer credit card market.

all households, making it difficult to separate the effect of changing collateral constraints from other aggregate trends affecting borrowing behavior. In a similar analysis, Abdallah and Lastrapes (2012) study how retail spending at the county level in Texas responded to a statewide relaxation of a ban on the ability to use home mortgage debt for non-housing consumption. While this policy provides a clear control group, namely counties outside of Texas, the fact the policy change occurred at the same time for all households makes it difficult to control for aggregate trends, which may have driven part of the observed response. The current paper improves upon these studies in two dimensions. First, because the variation in collateral values used in this paper is specific to the individual housing unit and because the identity and location of price-controlled units are clearly demarcated in the data, I am able to avoid having to rely on proxy measures to identify the "treated" and "control" households. Instead, my approach directly compares borrowing outcomes for observationally identical housing units in the same housing development, some of which experience a relaxation of collateral constraints and some of which do not. Second, because the price control expiration dates that I study occur at various points throughout the business cycle, I am able to directly control for potentially confounding aggregate trends, something which is not possible when studying a single policy change that occurs at the same time for all households.

Finally, this paper also relates to a much broader literature studying the role of collateral in the macroeconomy. An important theoretical literature in macroeconomics emphasizes the role that collateral constraints can play in amplifying business cycle fluctuations through the effect of changes in asset prices on the borrowing capacity of both firms and households (see, for example, Bernanke and Gertler, 1989; Kiyotaki and Moore, 1997; Iacoviello, 2005). Given that real estate is such a large source of collateral for many households and businesses, 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 the collateral value of their real estate assets.[10] This paper contributes new empirical evidence on the household side by documenting a strong positive relationship between housing collateral values and home equity-based borrowing.

[10]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 et al., 2005; Gan, 2007; Chaney et al., 2012; Cvijanoviċ, 2014), although Deng et al. (2013) find no evidence of a real estate collateral channel on firm investment in China. 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 et al., 2013; Adelino et al., 2014; Jensen et al., 2014). Similarly, on the household side, Caplin et al. (1997) and Lustig and Van Nieuwerburgh (2010) provide empirical evidence for a link between falling house prices, collateral constraints, and the consumption responses to regional income shocks in the U.S., while Almeida et al. (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.

The remainder of this paper is organized as follows: Section 2 provides institutional background on the Montgomery County Moderately Priced Dwelling Unit Program. Section 3 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 4. Section 5 outlines the empirical research design. Section 6 presents the main estimates of the effect of expiring price controls on collateral values and borrowing behavior and discusses heterogeneity in the borrowing response across borrowers with differing initial leverage. Evidence on the uses of extracted funds is presented in Section 7, and Section 8 concludes.

2 The Moderately Priced Dwelling Unit Program

Established in 1974, the Moderately Priced Dwelling Unit (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, D.C. and Chicago (Hickey et al., 2014).

2.1 Developer Requirements

The MPDU program requires that any developer wishing to build a residential development within the county containing more than 20 housing units must set aside a minimum of 12.5 percent of those units to be sold or rented to income-eligible households at controlled prices.[11] 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 2 provides an example of the spatial distribution of MPDUs in two representative subdivisions.[12] In general, MPDU units tend to be distributed throughout the subdivision though design standards can lead to some clustering in large

[11] If the developer agrees to provide more than the required 12.5 percent 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 pre-existing zoning code. In practice, developers rarely take this option.

[12] The data used to determine the location of the price-controlled units is described in detail in Section 4.

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. counter-tops 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 Appendix Figure A.1, which provides pictures of several example MPDUs and nearby market-rate units. 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 percent of the total stock of housing units in the county.[14] Approximately 70 percent 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.

2.2 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 Department of Housing and Community Affairs (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, D.C. 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 percent 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 Appendix Table A.1. In general, the income limits are quite high, reflecting both the relative affluence of the D.C. metropolitan area and the fact that the MPDU program is meant to specifically target *moderate*-income households. For example, the

[13]This figure is based on aggregate counts published by the Montgomery County Department of Housing and Community Affairs (DHCA) at `http://www.montgomerycountymd.gov/DHCA/housing/singlefamily/mpdu/produced.html`. The 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 percent 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.

maximum income limit of $75,000 for a four-person household is roughly 17 percent higher than the 2014 national median income for a household of the same size. As will be discussed in the data section below, this leads purchasers of MPDU homes to look relatively similar to the typical homebuyer in the U.S., at least in terms of household income.

2.3 Price Controls and Borrowing Restrictions

The initial purchase price for an MPDU is set by the DHCA according to a schedule that is meant to reflect construction costs associated with housing units of various types and sizes. Adjustments are made on a square footage basis for unit sizes deviating from those specified in the schedule and various "soft cost" adjustments are made in order to take into account developer financing costs, overhead, and other miscellaneous fixed costs of construction.

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 date), then the resale price is capped at the original price plus an allowance for inflation and a dollar-for-dollar adjustment that takes into account any documented major 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 Appendix Figure A.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 income-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.

Appendix Table A.2 shows the history of rules governing the length of the control period. 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 years to 30 years in April 2005. These changes to the law are reflected in Figure 3, 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 grey area marks the period of time during which I am able to observe transactions and loans.[16] The 1980s construction boom shows up as an increase in the number expiring price controls in the 1990s while the boom associated with the most recent cycle will not show up until approximately 2035.

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

[16]The transaction and loan data as well as the data used to determine the number of expiring price controls in each year is discussed in detail in Section 4

or taking on home equity debt for an amount that exceeds the controlled resale price. Thus, while the appraised 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, which would reveal any deed restrictions placed on the property. 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.[17] As discussed in detail in Section 6, I estimate that the average discount for an MPDU home during the control period is between \$66,000–\$106,000, implying an increase in collateralized borrowing capacity of roughly \$33,000–\$53,000. Expiring price controls thus generally lead to a large increase in the collateral value of an MPDU owner's home, which, as the next section discusses, can be used to provide estimates of the effect of changes in housing collateral on home equity-based borrowing.

3 Conceptual Framework

To illustrate how expiring price controls can be used to identify the effect of housing collateral 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 in order to highlight the difficulties associated with disentangling collateral effects from wealth effects using natural variation in house prices. I then show how the borrowing restrictions associated with MPDU price controls can be used to address these difficulties. 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 distinguishing collateral from wealth effects, 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.[18] Most 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.

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

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

3.1 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 non-housing 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 which 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 who wish to leave more to the next generation to consume less of their housing wealth before death. Or, as in Sinai and Souleles (2005), ω may vary by expected tenure length and by 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 associated with using natural variation in house prices to disentangle collateral effects from wealth effects. 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 wealth effects or collateral constraints.

The typical approach to solving this problem has been to infer the role of collateral constraints by examining how the magnitude of the borrowing response varies across different populations for whom one might expect either (6a) or (6b) to be the more relevant condition. For example, we might expect that not only are younger households more likely to have values of ω close to zero, but they are also more likely to have larger values of b^* as a result of steeply sloped life-cycle wage profiles that generate a gap between current and future income ($y_1 > y_0$). Thus, if younger homeowners are observed to increase borrowing more than older homeowners in response to similar increases in house prices, this could be taken as evidence for the importance of collateral constraints. However, as noted by Jappelli (1990), younger households might also be expected to be *less* constrained than older households if the rate of time preference is low relative to the real interest rate and consumption profiles are increasing in age. This ambiguity highlights the difficulty of using indirect proxy measures such as age to infer the role of collateral constraints and may explain why studies that do so have found mixed evidence (Campbell and Cocco, 2007;

Mian and Sufi, 2011; Bhutta and Keys, 2014).[19]

Another common approach is to examine heterogeneity in responsiveness across borrowers with different levels of prior debt utilization (M_0) or loan-to-value ratios ($\frac{M_0}{H}$), which shift the right-hand side of the inequality determining whether (6a) or (6b) applies. In this case, a finding that borrowers with higher loan-to-value ratios or prior debt utilization rates are more responsive to changes in prices is often taken as evidence in favor of collateral constraints (Disney and Gathergood, 2011; Mian and Sufi, 2011; Mian et al., 2013).[20] Similarly, several authors have investigated heterogeneity based on differences in income, liquid assets, or credit scores, with high-income, more-liquid, and high-credit score households expected to be less affected by collateral constraints (Yamashita, 2007; Mian and Sufi, 2011, 2014; Cooper, 2013; Bhutta and Keys, 2014). While these proxies, and loan-to-value ratios in particular, are more direct measures of collateral constraints than a homeowner's age, such proxies are nonetheless limited by their reliance on relatively strong *a priori* assumptions that are required in order to identify the set of potentially constrained households—assumptions that in many cases conflate credit demand with credit supply (Gross and Souleles, 2002; Agarwal et al., 2007). For instance, it is unclear whether homeowners with higher initial LTVs or fewer liquid assets borrow more in response to house price increases because they were unable to borrow prior to the change in prices (collateral constraints) or simply because they have stronger consumption smoothing motives that led them to carry more debt in the first place (wealth effects). More generally, as these examples illustrate, indirect proxy measures are inherently limited in their ability to distinguish differences in collateral constraints from other potential sources of heterogeneity, thus highlighting the need for estimates that are based on a more direct approach.

3.2 Identifying Collateral Effects Using MPDU Expiration Dates

The borrowing restrictions associated with MPDU price controls drive a wedge between the value of a home as collateral and its value as a component of homeowner wealth that allows for a direct test of the role of collateral constraints. 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

$$0 \leq b_0 \leq \lambda(H-\eta) - M_0, \tag{7}$$

[19] Appealing to similar reasoning, Cooper (2013) finds larger consumption responses among households who experience higher realized future income growth and argues that this is evidence in favor of the role of collateral constraints.

[20] In related work, Hurst and Stafford (2004) also use loan-to-value ratios as a proxy for collateral constraints in studying how equity extraction responds to changes in interest rates.

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 utility maximization problem:[21]

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

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:

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

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 collateral values.

4 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 Appendix B.

[21]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.

4.1 Data Sources

Property-Level Data

The basic structure of my dataset 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 U.S. The tax assessment file includes detailed information on the physical characteristics (e.g. square footage, number of bathrooms, number of stories, year built), use type (e.g. residential, commercial, single-family, condo), and street address for every property in the county. From this file, I drop all non-residential and multi-family properties as well as any properties with missing characteristics.[22] 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).[23]

To identify MPDU homes, I match the property assessment file with a list of MPDUs scraped from a publicly available online search portal hosted by the DHCA.[24] This data provides me with 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 were matched to the assessment file using a combination of exact physical location (geographic coordinates) and street address as described in Appendix B.1. Of the roughly 8,300 MPDUs in the DHCA database, I am able to match approximately 90 percent to a property in the assessment file.[25] Figure 4 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 non-rural regions of the county. One exception is the southern region of the county immediately bordering Wash-

[22] The assessment data in Montgomery County is 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.

[23] 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.

[24] The search portal can be accessed at `http://www6.montgomerycountymd.gov/apps/DHCA/pdm_online/pdmfull.asp` and was scraped using a script that exhaustively searched through and returned all possible MPDU addresses beginning with an alpha-numeric character.

[25] The match rate is lower than 100 percent 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.

ington, D.C., 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.[26] As a result, much of the housing stock in that area was not subject to the MPDU regulations at the time of development.[27]

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 datasets from DataQuick. Both datasets are sourced from local deeds records and can be linked to properties in the assessment file using a unique property ID. The first dataset contains information on all housing transactions occurring in the county during the period 1997–2012. For each transaction, this dataset 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 dataset contains information on all non-purchase loans secured against a property during the same period. This dataset 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 datasets provide me with a highly granular and near complete picture of all mortgage borrowing and housing purchases occurring in the county during this period. Each dataset is cleaned as described in Appendix B.2 in order to ensure that the transactions represent true ownership-changing arm's length transactions and that the loan information is accurate and consistent.

4.2 Measuring Equity Extraction

Since the non-purchase loans dataset contains a combination of loan types but does not distinguish between them, several steps must be taken in order to construct an accurate measure of equity extraction. In particular, it is important to distinguish between three different types of non-purchase loans: (1) regular refinances, which replace an existing loan without extracting any equity; (2) cash-out refinances, which replace an existing loan with a *larger* loan, thereby extracting equity for the amount of the difference; and (3) new non-purchase originations, which directly extract equity for the amount of the new loan. In order to make this distinction, I construct a "debt history" for every property that records an estimate of the current amount of outstanding debt secured against the property at any point in time on up to two potential loans.

[26] This can be seen in Appendix Figure B.1, which replicates Figure 4 replacing population density with property age.

[27] 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.

Debt histories are constructed by amortizing prior loan balances using the average interest rate at the time the loan was originated.[28] Given this history, when a new loan is observed, I am then able to determine whether that loan represents a purchase loan, cash-out refinance, new non-purchase origination, or regular refinance by comparing the size of the new loan to the estimated outstanding balance on the relevant existing loan (see Appendix B.3 for the details of 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.

Having categorized loans in this way, I then 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 non-purchase 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 1997–2012; 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 Appendix B.3, 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.

4.3 Sample Restrictions and Descriptive Statistics

Starting with the full sample of 286,484 properties, I impose several restrictions in order to arrive at my primary analysis sample. I first drop any property that could not 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 were 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

[28] 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 Appendix B.3 for the details of this procedure.

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 non-missing expiration dates and that at least 95 percent of the MPDUs were matched to their corresponding DataQuick property ID in a way that required the property unit number to agree. The latter requirement is imposed because MPDUs are typically 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 1 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 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 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 0.5th and 99.5th percentiles in the full sample.

The differences between the full sample (columns 1–2) and the analysis sample (columns 3–4) 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, D.C. 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 5–6) and MPDUs (columns 7–8) are also largely in line 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 even holding characteristics constant. Similarly, reflecting their lower incomes, owners of MPDUs are more likely to purchase their homes with FHA-insured loans at higher initial loan-to-value ratios relative to owners of market-rate units. 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.

Economic and Demographic Representativeness

Given the income limits and eligibility requirements associated with the MPDU program, it is also interesting to compare how the economic and demographic characteristics of the homeowners in my analysis sample (particularly those who purchase price-controlled homes) compare to a more nationally representative sample. To that end, I match a subset of the transactions data to loan application data made publicly available through the Home Mortgage Disclosure Act (HMDA). The HMDA data provide loan-level information on borrower income and race for nearly all home mortgage applications filed in the United States and serve as a useful gauge of the national representativeness of my sample along these dimensions.

The details of the match and several figures comparing the national distributions of income and race with the distributions for my analysis sample and the subset of transactions involving an MPDU home are provided in Appendix B.5. As shown in Figure B.5, the incomes of the households who purchase the price-controlled units are actually quite similar to the income of the typical homebuyer in the U.S. during my sample period. The income distribution among buyers of price-controlled homes is roughly centered around the median of the national distribution and spans a large portion of the interquartile range of that distribution. The racial breakdowns, shown in Figure B.6, are less similar. Relative to the national average, Montgomery County has an unusually high share of Asian households and slightly higher shares of Black and Hispanic households. The high Asian share likely reflects the industrial composition of the county, which is a major hub for the biotech industry, while the higher Black and Hispanic shares are most likely a result of the fact that the national sample contains many non-minority households located in the Midwest and other less diverse regions of the country. To the extent that the propensity to extract equity following a loosening of collateral constraints differs substantially by race, such differences may affect the external validity of my results. However, given the similarity in the income distributions, these differences would have to persist even conditional on income in order for the estimates derived from my sample to differ drastically from a more nationally representative sample.

5 Empirical Framework

5.1 Identification Strategy

I estimate the effect of expiring price controls on collateral values 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 results section 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 afterwards. 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 in order to generate systematic differences in trends.

5.2 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:

$$y_{ist} = \alpha_s + \delta_t + X_{it}'\gamma + \beta_1 \cdot MPDU_i + \beta_2 \cdot MPDU_i \times Post_{st} + \epsilon_{ist}, \tag{10}$$

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 one if property i is an MPDU, while the $Post_{st}$ indicator takes the value one if year t falls on or after the year the first price control in subdivision s expires. I define the treatment date in this way to take into account both the fact that market-rate properties have no explicit expiration date and that controlled properties within the same 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.[29] 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 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 debt 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 and not necessarily the change in collateral values.[30] I address this concern directly by estimating specifications 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.

[29] Over half of all MPDUs in my sample expire within two years of the first MPDU in their subdivision and roughly 75 percent 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.

[30] In Appendix C.1, I present results showing that expiring price controls do in fact lead to an increase in housing turnover for previously controlled units of roughly three to five percentage points per year. While a three to five percentage point increase is not nearly large enough to lead to a total turnover of the MPDU housing stock during my sample period, it nonetheless justifies the use of specifications that include property or owner fixed effects due to the concern that part of the effect could be driven by differences in the transacted housing stock or changes to ownership over time.

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

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

where $\mathbb{1}_{t-\tau(s)=\rho}$ is a relative year dummy taking the value one if the current year falls ρ years after the expiration of the price control and zero 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 pre-trends, then the $\beta_{2,\rho}$ coefficients should be equal to zero for any $\rho < 0$.

6 Price Controls, Collateral Values, and Borrowing Behavior

This section presents the main estimates of the effect of expiring price controls on the transaction prices (i.e. collateral values) 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. In order 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. Finally, to provide additional evidence for the role of collateral constraints in governing the borrowing response to expiring price controls, I also examine heterogeneity in the response across the distribution of initial leverage. Unless otherwise specified, all results pertain to the set of transactions and property-years contained in the analysis sample described in

Section 4.

6.1 Graphical Evidence

As a point of departure for the empirical analysis, Figure 5 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 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. 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.[31] 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 pre-period means were used to construct the fitted values.

Consistent with the aggregate descriptive statistics presented in Table 1, 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. However, for all three outcomes, the MPDU means diverge significantly from their pre-period 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 5 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. 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 pre-period MPDU trend, providing strong support for the validity of the parallel trends assumption underlying the difference-in-differences estimates that follow.

[31] 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 in to both series.

6.2 The Effect of Expiring Price Controls on Collateral Values

To more precisely quantify the effect of expiring price controls on the collateral value of MPDU properties, Table 2 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, 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 built 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.[32] Following the expiration of the price control, transaction prices at previously controlled MPDUs increase by roughly 57 percent. Applying that figure to the mean of the pre-period 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 which 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 four 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

[32] 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 $\%\Delta = 100 \times \left[\exp\left(\hat{\beta}_2 - \frac{1}{2}\hat{\sigma}^2_{\beta_2}\right) - 1\right]$.

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. Converting these estimates into dollars implies that expiring price controls lead transaction prices to increase by roughly \$66,000–\$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–\$53,000 upon the expiration of the price control.

To give a sense of the dynamics of the price effect, Figure 6 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 2, but which 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 ten 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 percent 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 pre-period 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.

6.3 The Borrowing Response to Increases in Housing Collateral

The evidence presented in the previous section suggests that expiring price controls lead to large increases in the resale value of previously controlled MPDU homes. Assuming MPDU owners have the ability to borrow against up to half of that increase implies a similarly large increase in collateralized borrowing capacity. In this section, I explore how the borrowing behavior of MPDU homeowners responds to the additional collateral released by expiring price controls.

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 3 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 2 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 effect, the MPDU$\times$Post interaction, 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$\times$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 percent increase over the pre-period 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 70–80 percent 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 take place 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 3 suggest that in response to the increase in collateral values 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 percent over their pre-period average propensity to borrow.

Figure 7A 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 6 and was 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 included all the same controls as the specification in column 3 of Table 3. 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 percent confidence intervals for MPDU owners. Starting in the year the first MPDU price control expires, the MPDU trend diverges sharply from from its pre-period 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 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. The fact that the extraction probabilities remain elevated relative to their pre-period level among MPDUs beyond the first year further suggests that the removal of the price control provides owners of MPDUs with access to the same natural increases in collateral made available to owners of non-MPDU properties through normal house price appreciation.

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 increases in collateral both on the extensive margin and by increasing the amount they borrow conditional on extracting equity. This section presents estimates of the combined effect of these two margins of adjustment on the annual amount of equity extracted among owners of MPDU homes.

Table 4 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 set equal to the sum of all non-purchase 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 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 implied by the estimated tobit coefficients for the expected value of the censored outcome.

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 1–4 imply that expiring price controls lead to an increase in the average amount of equity extracted per year of roughly \$2,300–\$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 pre-period 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 75–100 percent of the pre-period gap in equity extraction between owners of MPDUs and non-MPDUs. The dynamics of the effect are also shown in Figure 7B and mirror the results for the extensive margin reported in the same figure. The average amount of equity extracted jumps sharply among MPDU owners in the year the price control expires and remains high relative to its pre-period level for the remainder of the sample period. There is also no evidence of differential trends for MPDUs and non-MPDUs during the pre-period. 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.

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 collateral values implied by the transaction price regressions reported in Table 2 and the results on total equity extraction just discussed in Table 4. Specifically, the price results in Table 2 imply that expiring price controls lead to an increase in transaction prices at previously controlled MPDUs that ranges between \$66,000–\$106,000. Assuming that MPDU owners are able to borrow against up to half of the price increase, this implies an increase in pledgeable collateral of roughly \$33,000–\$53,000. The results in Table 4 imply that following the expiration of the price control, borrowers increase the average amount of equity they extract from their homes by roughly \$2,300–\$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–\$0.13.

As a point of reference, these figures can be compared to recent estimates from the literature on the *overall* effect of house prices changes on homeowner borrowing, which combine both collateral and wealth effects. For example, 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–\$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 marginal propensity to borrow of roughly \$0.07. Two important outliers are the estimates provided by Mian and Sufi (2011) and Mian and Sufi (2014), who focus exclusively on the most recent housing cycle and estimate marginal propensities to borrow of \$0.25 and \$0.19, respectively. While differences in methodology and estimation samples make it difficult to make a direct comparison between the estimates provided in this paper and those just discussed, the \$0.04–\$0.13 range provided above suggests that a significant portion of the effect of house prices on home equity-based borrowing is driven by collateral values rather than wealth effects.

6.4 Heterogeneity in the Borrowing Response 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 by examining 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 loan-to-value ratio (LTV) 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:

$$y_{ijst} = \gamma_j + \delta_t + \alpha_s \cdot t + X_{it}'\gamma + \sum_{k=1}^{4} \beta_k \cdot MPDU_i \times Post_{st} \times LTV_{jk} + \epsilon_{ijst}, \tag{12}$$

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, γ_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_{it} is a vector of time varying property 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_{it} includes only property age and its interaction with the condo dummy and 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 borrowers with different initial LTVs. Figure 8 plots these coefficient estimates along with their 95 percent 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.[33] 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.

6.5 Additional Robustness Checks

Placebo Tests

As a further test of the parallel trends assumption underlying the main difference-in-differences estimates provided in Section 6.3, 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 estimate for both 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

[33]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 five percent level for both outcomes. Similarly, one sided test for whether the effects are larger among those in the two highest groups relative to those in the two lowest groups also fails to reject the null at the ten percent level for the extensive margin response and at the five percent level for the total equity extraction measure.

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 pre-period or the post-period 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 3 for the extensive margin response and from column 3 of Table 4 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.

Matching Estimates

Another potential concern with the main difference-in-differences estimates provided in Section 6.2 and Section 6.3 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 Appendix C.2, I report the results from an alternative approach to estimating the effect of the expiring price control using a semi-parametric propensity score matching estimator (cf. Heckman et al., 1997, 1998). This approach, which is described in detail in the 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 Appendix Table C.3 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.

7 Evidence on the Uses of Extracted Funds

The results in the previous section provide strong evidence that homeowners respond to increases in the collateral value of their homes by extracting equity; however, the aggregate impact of this behavior depends 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 aggregate effects of rising collateral values would not be as large as if the money were used to fund consumption or investment expenditures. 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.

7.1 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 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 et al., 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. This data, which is described in further detail in Appendix B.6, contains 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 permits 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 2000–2012, unless the property was built during that time period, in which case the data beings 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 5 reports the coefficient estimates for the MPDU×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 for estimating 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 five percent 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 0.6–0.7 percentage points. These effects are large relative to the pre-period 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.

7.2 Evidence from Foreclosures

The second piece of evidence I provide regarding 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 that the act of extracting equity itself would 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 would 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 collateral values 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 does 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 non-purchase loans dataset that are coded as equity extractions and estimate versions of the following regression specified at the loan level

$$Foreclosure_{ijst} = \alpha_s + \delta_t + X'_{ijt}\gamma + \beta_1 \cdot MPDU_j + \beta_2 \cdot MPDU_j \times Post_{st} + \epsilon_{ijst}, \qquad (13)$$

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 1997–2009 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, 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 6 reports estimates of this coefficient from various versions of equation (13). All specifications include the standard set of property characteristics as well as the 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×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 1.5–2 percentage points relative to equity extractions secured against non-MPDU properties following the expiration of the price control. This effect represents between 70–90 percent 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 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.

8 Conclusion

For many households, houses are both the largest asset they own and the most readily available source of pledgeable collateral against which they can borrow. Changes in the value of housing thus have the potential to lead to significant changes in the desire and ability of individual homeowners to borrow. The macroeconomic consequences of house price-induced changes in individual borrowing behavior depend crucially on whether those changes are driven by wealth effects, which in aggregate may be offset by opposing changes among renters and others who are "short" housing, or by the relaxation of binding collateral constraints, which do not have the same offsetting effects. Empirical analyses of the effect of house price fluctuations on homeowner borrowing often struggle to distinguish between these two channels, as it is difficult to find instances in which changes to the value of a home do not represent both a direct increase in a household's net worth and an indirect expansion of their collateralized borrowing capacity.

This paper addresses these challenges and isolates the role of collateral values by exploiting a unique feature of an inclusionary zoning policy in Montgomery County, Maryland that imposes temporary price controls 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. 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 collateral value of the home. Using this fact, I show that changes in the collateral value of housing have important effects on homeowner borrowing behavior. Specifically, following the expiration of the price controls, the probability of home equity extraction increases differentially among owners of previously controlled units by roughly 4 percentage points relative to owners of observationally identical non-controlled 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–\$0.13. These estimates are roughly within the range of existing estimates from the literature of the *total* effect of house price increases on homeowner borrowing and suggest that collateral constraints are an important factor driving that relationship. The magnitude of the borrowing response is also monotonically increasing in the homeowner's initial leverage, providing further evidence that collateral constraints are the dominant force leading owners of controlled units to respond to the expiration of the price control by borrowing against their homes. Finally, evidence from home improvement permit applications and subsequent loan performance suggests that at least some portion of the extracted funds were used to finance current consumption or investment expenditures and not simply used as a means for portfolio diversification or paying down existing debt. These results corroborate existing evidence on the importance of collateral constraints based on indirect proxy measures and have implications both for understanding the microeconomic mechanisms driving the relationship between house prices and homeowner borrowing and the macroeconomic consequences of that relationship.

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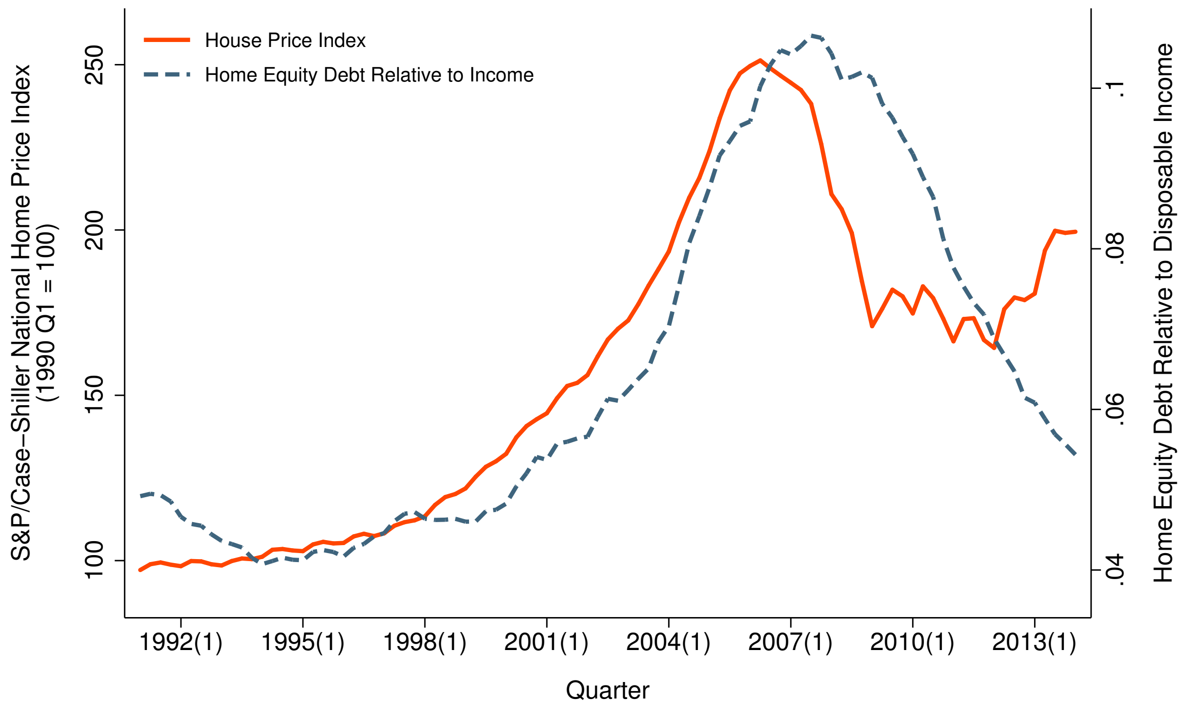


FIG. 1.—House Prices and Home Equity Debt Relative to Disposable Personal Income. This figure plots aggregate trends in U.S. house prices and home equity debt relative to disposable personal income at a quarterly frequency over the period 1990–2014. Aggregate data on home equity debt come from the Federal Reserve Flow of Funds (Z.1 Release, Series FL893065125.Q). Disposable personal income data come from the National Income and Product Accounts (BEA Account Code A067RC1). The home equity debt series is only available from the Flow of Funds beginning in the fourth quarter of 1990. The house price index is normalized to 100 in the first quarter of 1990, and the underlying data come from the S&P/Case-Shiller National Home Price Index.

FIG. 2.—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.

FIG. 3.—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 grey area marks the period of time for which information on housing transactions and home equity-based borrowing is availble from DataQuick.

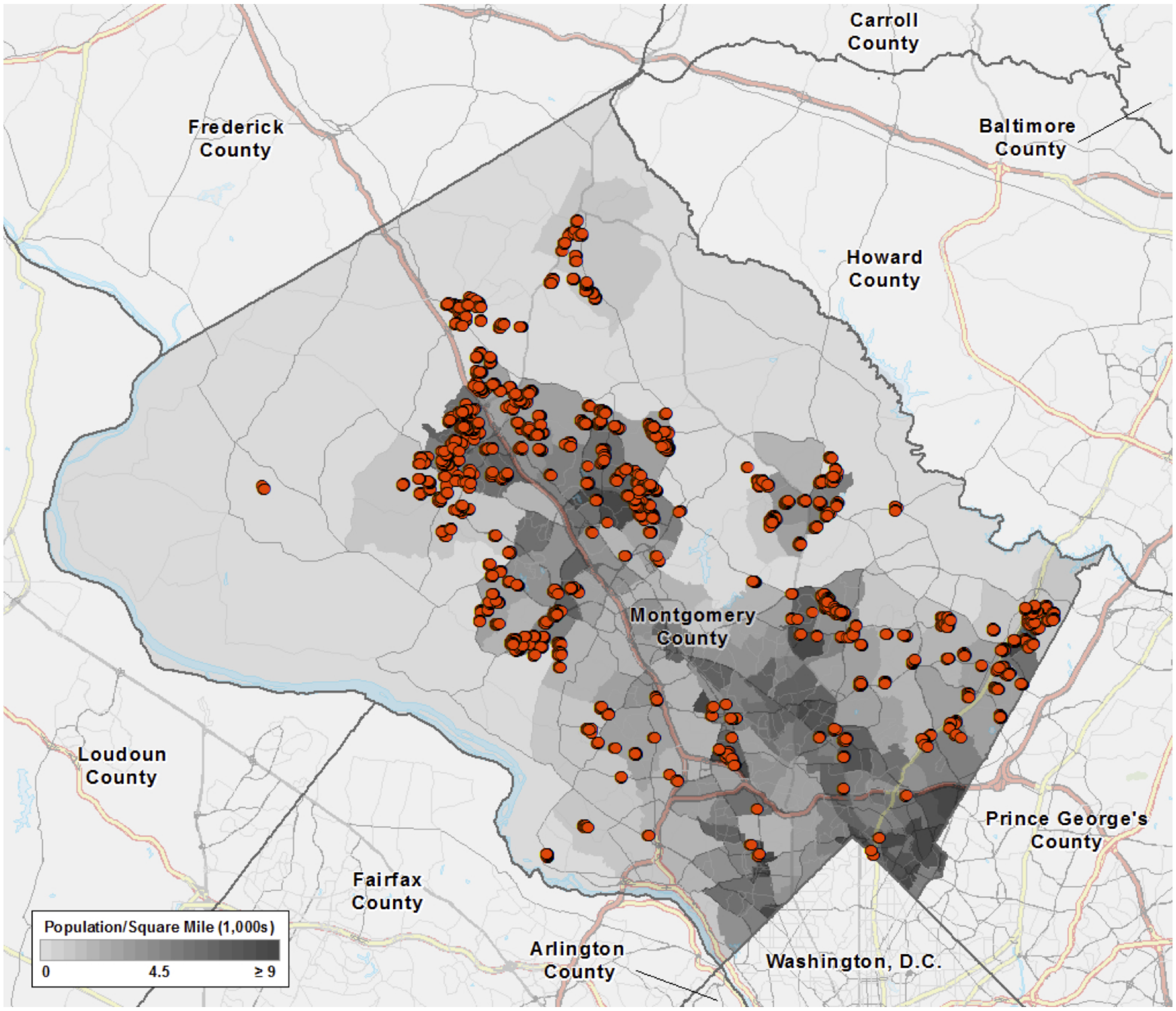
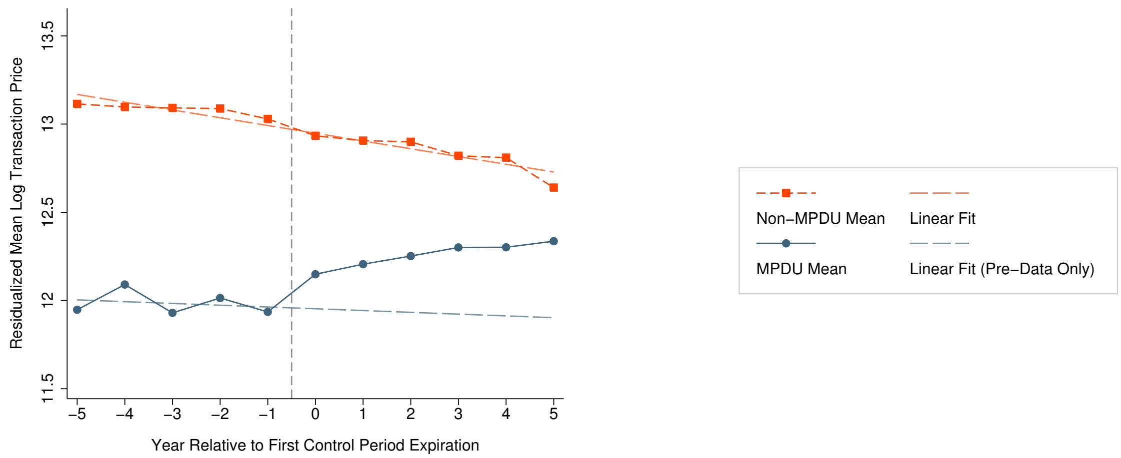
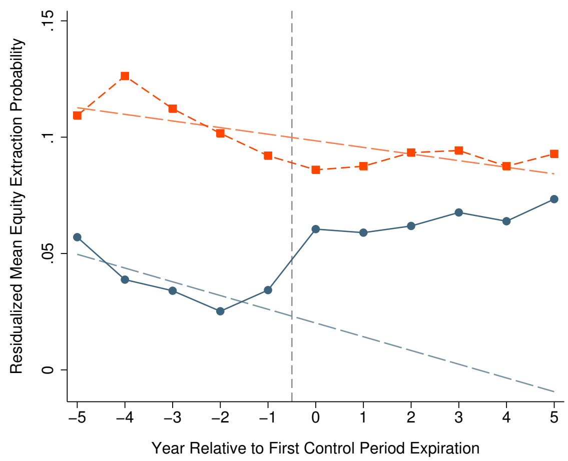


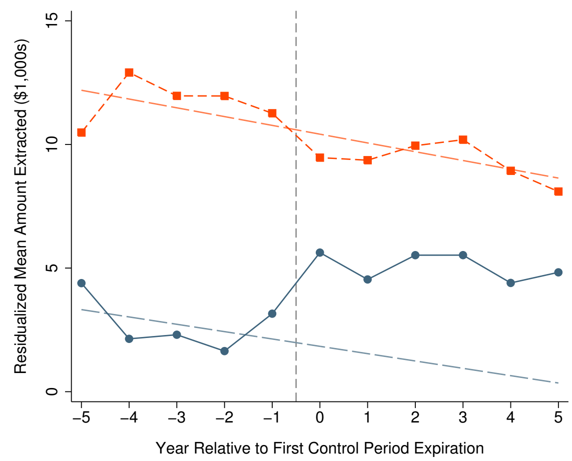
FIG. 4.—Geographic Distribution of MPDU Properties within Montgomery County, Maryland. This figure shows the location of all MPDU properties that were successfully matched to a property in the DataQuick assesment file (N=7,404). MPDU properties are marked with an orange circle. Census tracts within Montgomery County are shaded according to their population density as reported in the 2010 American Community Survey.



Panel A. Log Transaction Price



Panel B. Probability of Extracting Equity



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

FIG. 5.—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 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 the outcome was then added back in to both series. The dashed lines plot the fit from a regression of the binned means on a linear term in relative year. For MPDUs, only the pre-period means were used to construct the fitted values.

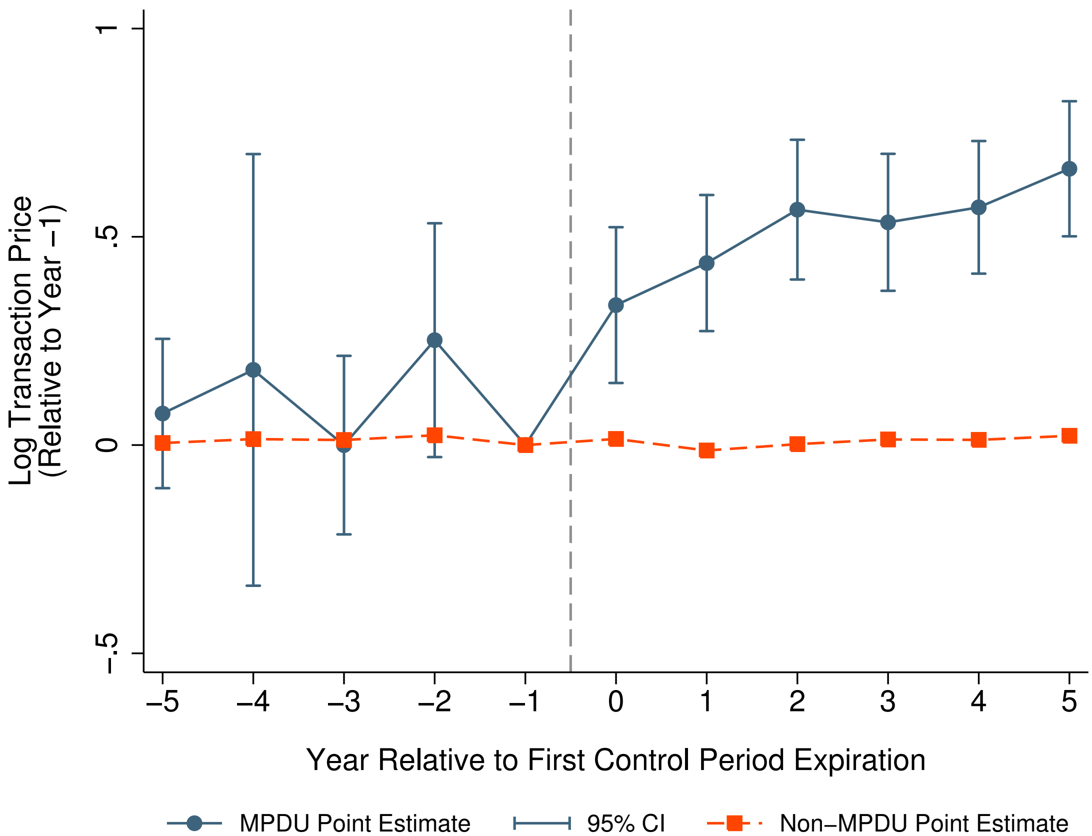
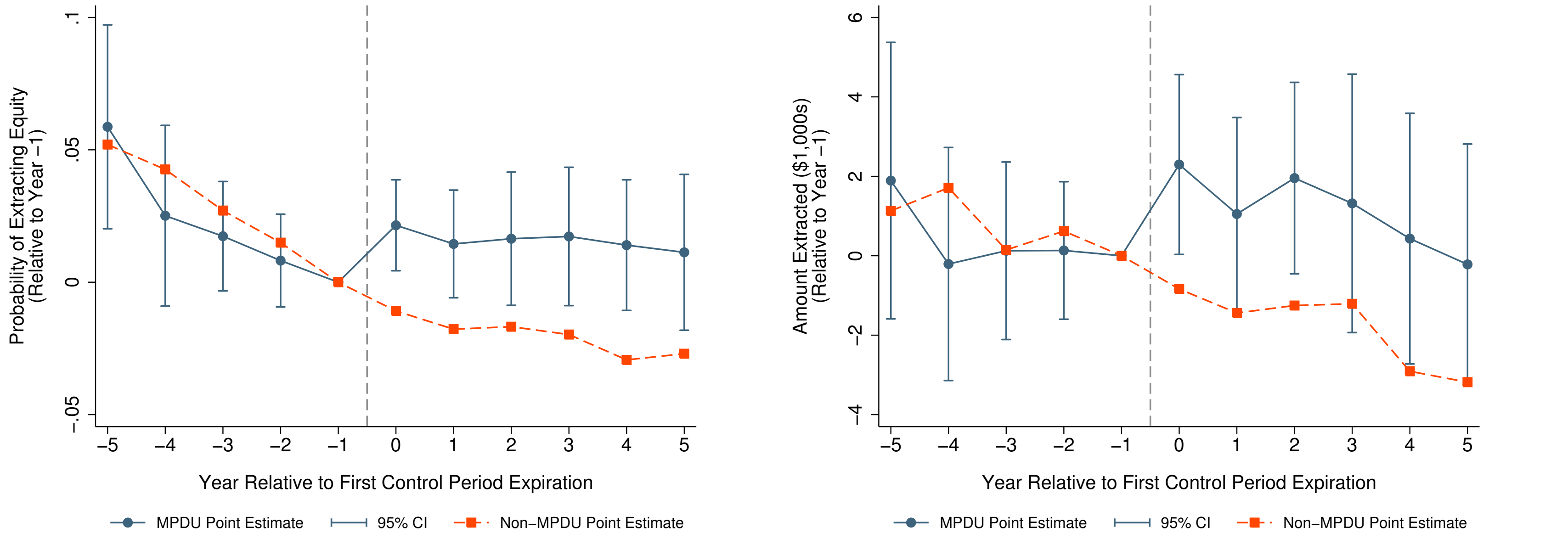


FIG. 6.—Dynamic Effects of Expiring Price Controls on Transaction Prices of MPDU Properties. This figure reports estimates of the effect of expiring price controls on the transaction prices of MPDU properties derived from a flexible difference-in-differences regression that allows the effect to vary by year relative to the expiration of the price control. Estimates were 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 percent 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 percent confidence intervals are based on standard errors which were clustered at the subdivision level. The regression also included year fixed effects, subdivision fixed effects and their interaction with a linear time trend and a set of property characteristics. The property characteristics include a quadratic in the interior square footage of the home, dummies for the number of bathrooms, stories, and property age, as well as an indicator for whether the property is a condo or townhome and the interaction of that indicator with the year fixed effects and all of the other property characteristics.



Panel A. Probability of Extracting Equity

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

FIG. 7.—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 were 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 percent 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 percent confidence intervals are based on standard errors that were clustered at the subdivision level. The regressions also included year fixed effects, subdivision fixed effects and their interaction with a linear time trend and a set of property characteristics. The property characteristics include a quadratic in the interior square footage of the home, dummies for the number of bathrooms, stories, and property age, as well as an indicator for whether the property is a condo or townhome and the interaction of that indicator with the year fixed effects and all of the other property characteristics.

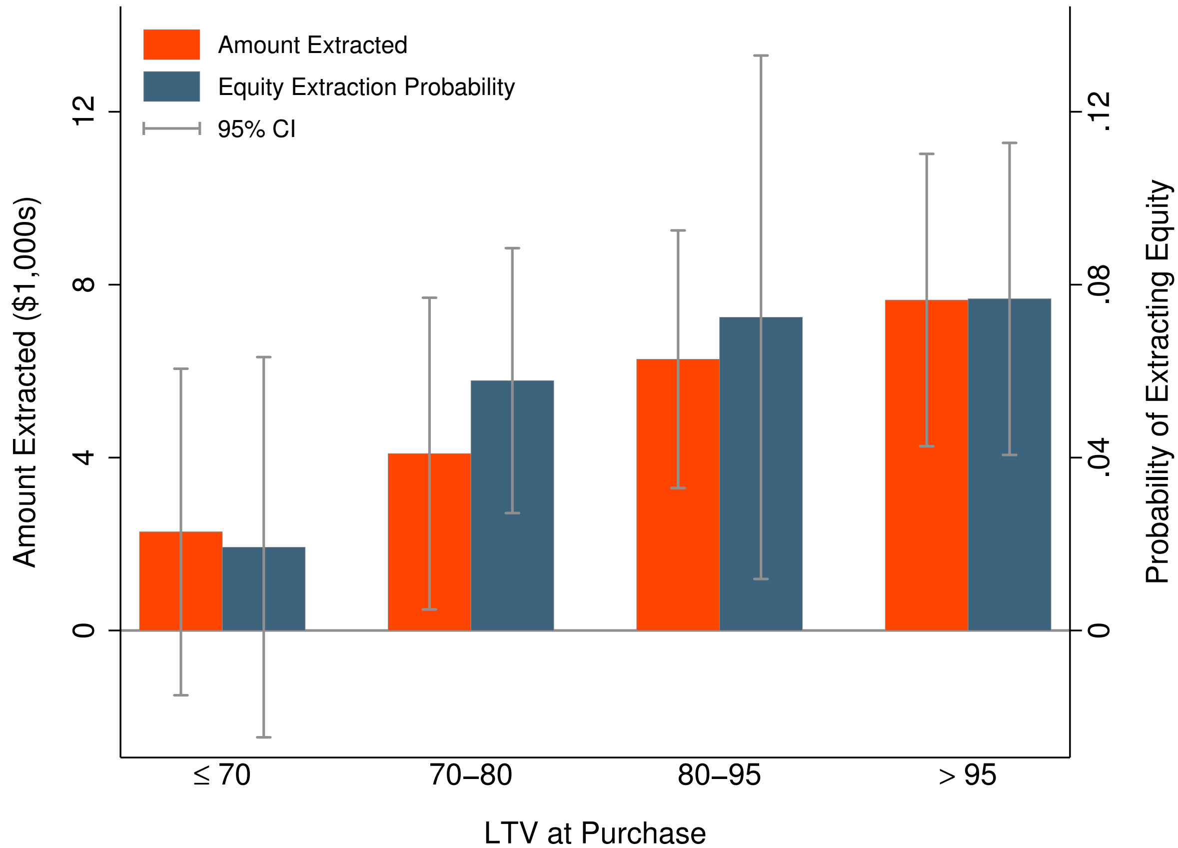
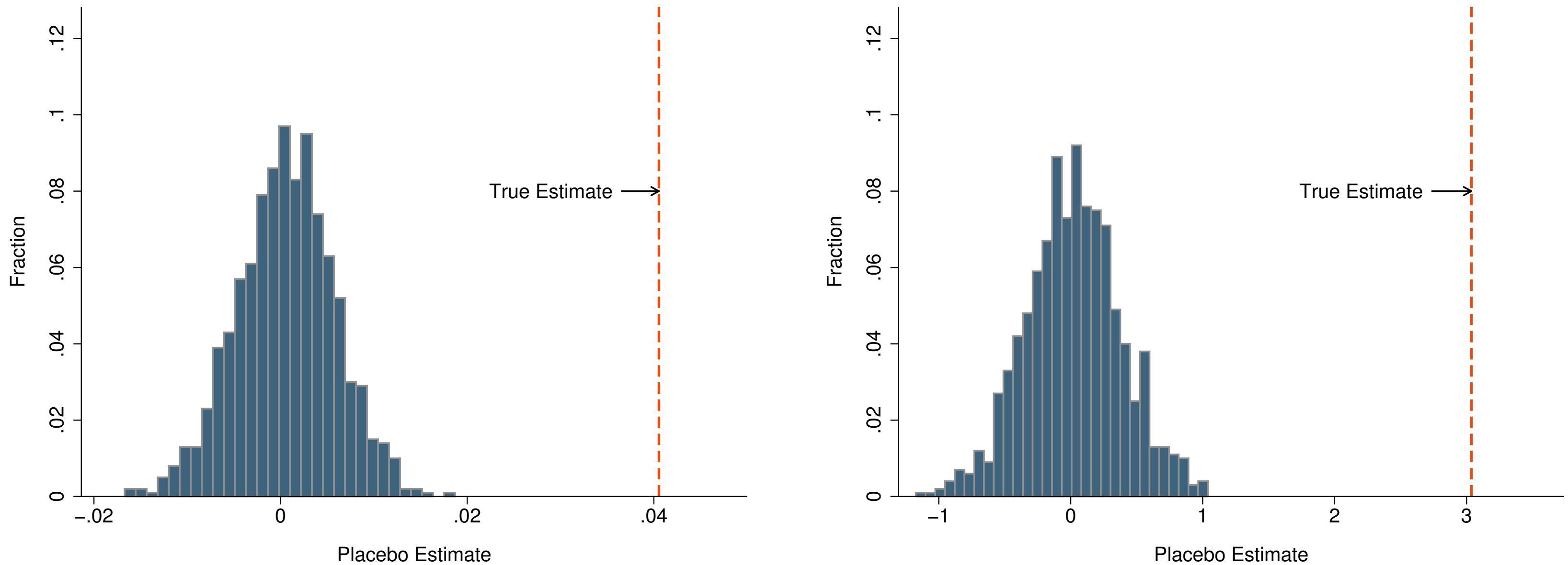


FIG. 8.—Heterogeneity Across the Distribution of Initial Leverage in the Effect of Expiring Price Controls on the Borrowing Behavior of MPDU Homeowners. This figure reports estimates of the effect of expiring price controls on the borrowing behavior of MPDU homeowners derived from a difference-in-differences regression that allows the effect to vary according the 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 percent confidence intervals for each estimate are also shown and are based on standard errors that were 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 with in the range indicated on the x-axis. The regressions also included 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 be able to accurately measure initial leverage, the sample was 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).



Panel A. Probability of Extracting Equity

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

FIG. 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 were taken from column 3 of Table 3 for Panel A. and column 3 of Table 4 for Panel B. Each placebo estimate was 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 pre-period or the post-period was 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, 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.

TABLE 1
SUMMARY STATISTICS FOR PROPERTIES, TRANSACTIONS, AND ANNUAL MEASURES OF EQUITY EXTRACTION

	Full Sample		Analysis Sample					
	All Properties		All Properties		Non-MPDUs		MPDUs	
	Panel A. Property Characteristics							
Square Footage (1000's)	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 ($1000's)	444.09	(330.85)	418.09	(290.70)	432.81	(294.80)	237.37	(140.34)
Loan Amount ($1000's)	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 ($1000's)	7.63	(40.97)	8.45	(40.75)	8.85	(42.08)	4.63	(24.37)
Amount Extracted $\vert > 0$ ($1000's)	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	

NOTE.—This table presents descriptive statistics for both the full sample of single-family residential properties with non-missing housing characteristics contained in the DataQuick assessment file (columns 1–2) and the restricted sample used in the analysis (columns 3–8). 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 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 0.5th and 99.5th percentiles in the full sample.

TABLE 2
THE EFFECT OF EXPIRING PRICE CONTROLS ON
THE TRANSACTION PRICES OF MPDU PROPERTIES

	(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

NOTE.—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. 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.

TABLE 3

THE EFFECT OF EXPIRING PRICE CONTROLS ON THE ANNUAL PROBABILITY OF EXTRACTING EQUITY AMONG OWNERS OF MPDU PROPERTIES

	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

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

TABLE 4
THE EFFECT OF EXPIRING PRICE CONTROLS ON TOTAL EQUITY EXTRACTED PER YEAR (IN $1,000S) AMONG OWNERS OF MPDU PROPERTIES

	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

NOTE.—This table reports difference-in-differences estimates of the effect of expiring MPDU price controls on the annual amount of equity extracted among owners of MPDU properties. 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. The property characteristics include a quadratic in the interior square footage of the home, dummies for the number of bathrooms and the number of stories, and an indicator for whether the property is a condo or townhome as well as the interaction of that indicator with the year fixed effects and all of the other property characteristics including property age. Subdivision trends are estimated by interacting the subdivision fixed effects with a linear time trend. Columns 1–4 report coefficient estimates from OLS regressions, while column 5 reports the marginal effects for the expected amount of equity extraction (censored and uncensored, treating censored as zero) from a tobit specification. The mean of the dependent variable among MPDU properties in the period prior to the first price control expiration is reported in the second to last row. Standard errors are reported in parentheses and are clustered at the subdivision level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

TABLE 5

THE EFFECT OF EXPIRING PRICE CONTROLS ON THE ANNUAL PROBABILITY OF PERMITTED RESIDENTIAL INVESTMENT AMONG OWNERS OF MPDU PROPERTIES

	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

NOTE.—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. The property characteristics include a quadratic in the interior square footage of the home, dummies for the number of bathrooms and the number of stories, and an indicator for whether the property is a condo or townhome as well as the interaction of that indicator with the year fixed effects and all of the other property characteristics including property age. Subdivision trends are estimated by interacting the subdivision fixed effects with a linear time trend. Columns 1–4 report coefficient estimates from linear probability models, while columns 5–6 report marginal effects from probit and logit specifications. The mean of the dependent variable among MPDU properties in the period prior to the first price control expiration is reported in the second to last row. The sample excludes properties located in the cities of Gaithersburg and Rockville, where permit application data is 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, it 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.

TABLE 6
THE EFFECT OF EXPIRING PRICE CONTROLS ON THE THREE-YEAR FORECLOSURE RATE AMONG EQUITY EXTRACTIONS SECURED AGAINST MPDU PROPERTIES

	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

NOTE.—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 non-purchase loans coded as equity extractions that were originated during the period 1997–2009. 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 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 1–3 report coefficient estimates from linear probability models, while columns 4–5 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.

Appendices

A Additional Institutional Detail

TABLE A.1
MPDU INCOME LIMITS – 2014

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

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

TABLE A.2
HISTORY OF MPDU CONTROL PERIOD RULES

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

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

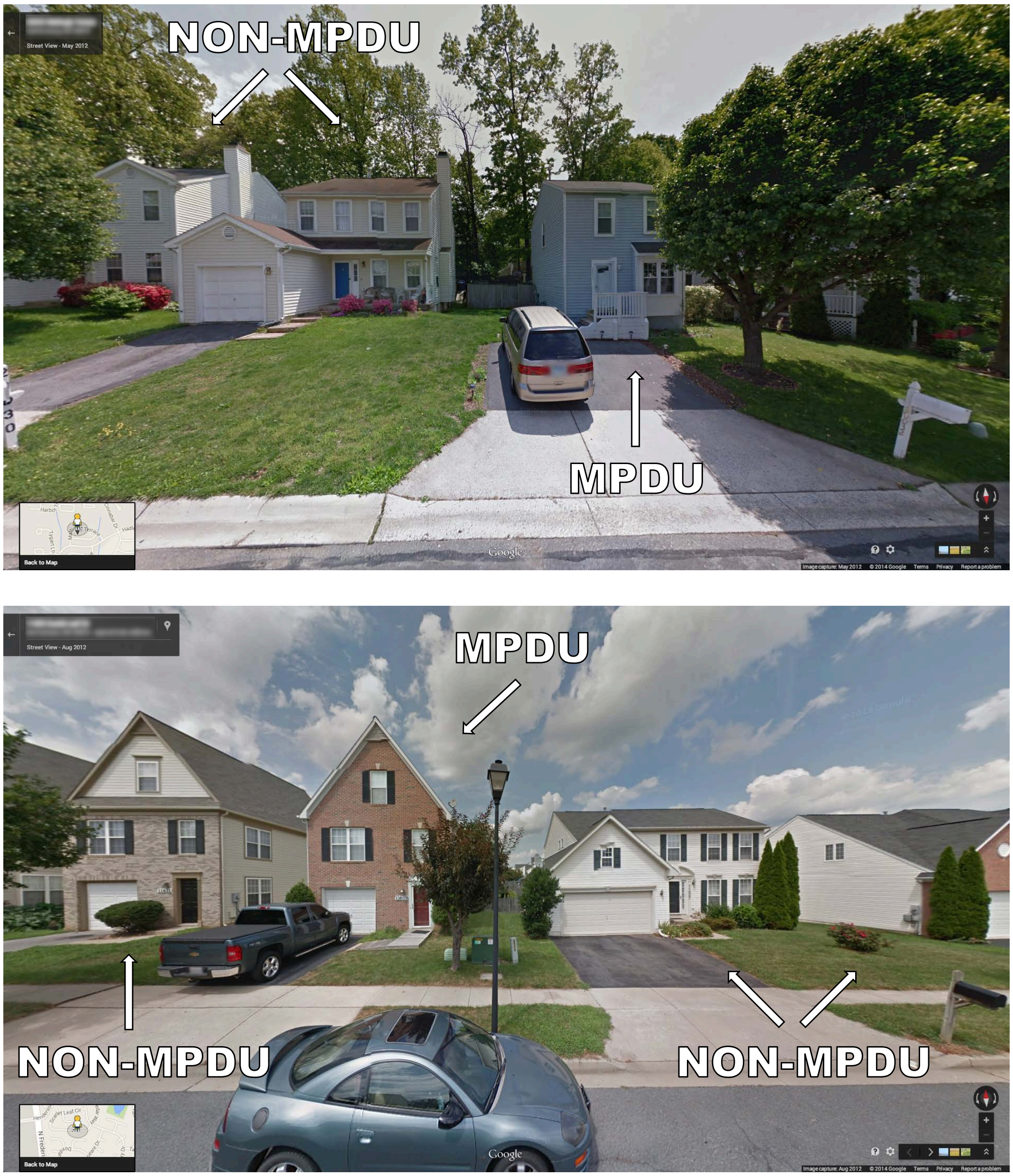


FIG. A.1.—Examples of MPDU Exterior Design. This figure presents images of the exterior of two representative MPDUs and nearby market-rate units. The MPDUs shown are located in different subdivisions. Images were accessed on June 24, 2014 and captured by Google Maps in May (top image) and August (bottom image) of 2012.

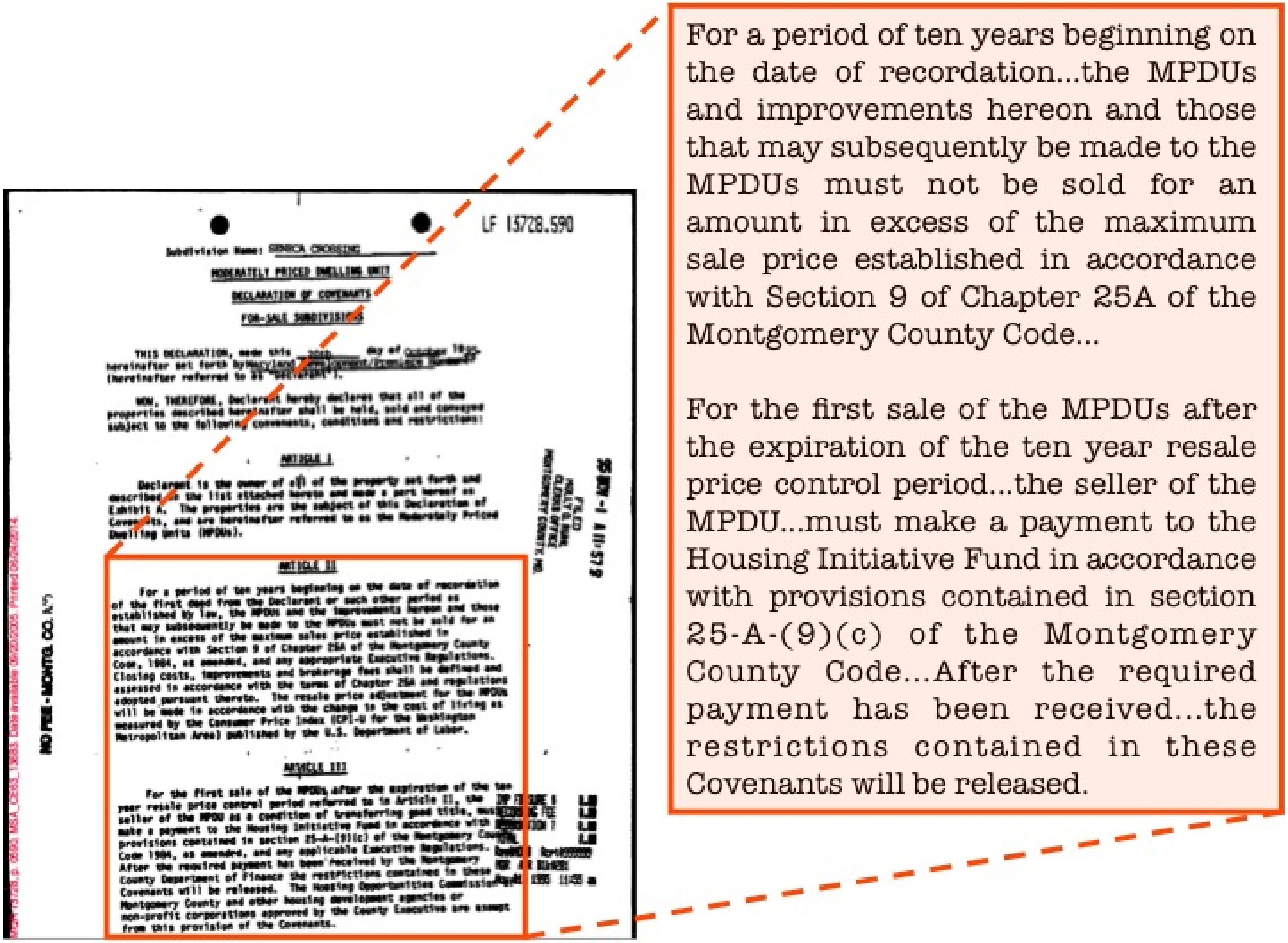


FIG. A.2.—Example Deed Restriction. This figure shows an example deed restriction for an MPDU originally sold by the developer on October 20, 1995. The deed was originally recorded in the Montgomery County Circuit Court (Land Records) MQR 13728, p. 0590, MSA_CE_63-13683 and was accessed online through MDLANDREC on June 24, 2014.

B Data

B.1 Matching MPDU Properties to the DataQuick Assessment File

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

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

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

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

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

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

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

B.2 Cleaning the Transaction and Loan-Level Data

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

Cleaning the Transactions Data

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

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

cases, two transactions were recorded on the same day for the same property with transaction prices that differed by less than one percent and only one record containing a positive or realistic loan amount. In these cases, I kept the record with the non-erroneous loan amount and dropped the 2,918 duplicate records. A few transactions were exact duplicates of another transaction on property, date, price, and loan amounts, but differed along some other dimension (typically an alternate lender name). In these cases, one of the duplicates was randomly dropped. This dropped 375 records. In some cases, two transactions were recorded on the same day for the same property where the buyer for the first transaction was listed as the seller on the second transaction and the price for the first transaction was clearly not a market price. In these cases, the intermediary was typically a title company, escrow company, or some other similar entity and only the transaction containing the market price was kept (dropping 656 intermediary transactions). Finally, I randomly dropped 474 transactions that were exact duplicates of another transaction on property and date but differed along some other dimension for unknown reasons as well as 165 transactions recorded on the same property in the same week but differing in some other way. In these cases, one of the records was kept to serve as a "placeholder" documenting the change in ownership that occurred on that day or during that week. These transactions were used in determining change of ownership, but their prices were not included in any analyses. The final sample contained 233,879 transactions involving one of the 286,484 single-family residential properties with non-missing housing characteristics contained in the assessment file.

Cleaning the Non-Purchase Loans Data

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

B.3 Constructing "Debt Histories"

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

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

[3] Since prices for MPDU properties are not permitted to appreciate faster than the rate of inflation during the control period, I use the Consumer Price Index for All Urban Consumers (CPI-U) to adjust prices for these proper-

amount is greater than 50 percent of the estimated current resale value, then the loan is used to initialize the first account balance and the second account balance is initialized at zero. If the loan amount is less than 50 percent of the current resale value, then the loan is used to initialize the second account balance and the first account balance is initialized at zero.

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

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

ties before the end of the control period and the hedonic price indices afterwards. Only transactions on the relevant side of the expiration date are used to derive the current resale price for MPDUs.

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

Validating the Equity Extraction Measure

While the deeds data provide exhaustive coverage of all loans secured against a property, the assumptions needed to determine whether a new loan adds to or replaces existing debt introduce measurement error in the equity extraction variable. [5] To gauge the magnitude of this error, Figure B.2 presents aggregate time series evidence comparing my equity extraction measure against two external measures that were calculated using data from which it is possible to directly determine whether a new loan adds to a borrower's existing debt. Panel A. plots the yearly average probability of equity extraction. The dashed grey line plots a measure of equity extraction that was calculated by Bhutta and Keys (2014) using nationally representative data from the Equifax Consumer Credit Panel (CCP). The CCP data tracks individual debt obligations at a quarterly frequency and provides a near complete picture of the liability side of household balance sheets. Using this data, Bhutta and Keys (2014) define equity extraction as any instance in which an existing homeowner's total mortgage debt increases by more than five percent. The two solid lines were calculated using the DataQuick data and the measure of equity extraction discussed above. They plot the fraction of properties from which equity was extracted in each year for all properties in Montgomery County (orange circles) and for the restricted set of properties in my analysis sample (blue squares). [6] The two methods of measuring equity extraction generate remarkably similar time series. Both the DataQuick series and the Equifax series increase rapidly during the period 1999–2003 before reaching a peak of roughly 20 to 25 percent and eventually declining and leveling off at around 5 percent by 2010. [7] The correlations between the Equifax measure and the two DataQuick series are also reported in the figure and are greater than or equal to 0.95 in both cases. Panel B. provides an alternative way of validating my measure—in this case, plotting the fraction of all refinance loans that in the DataQuick data that I code as cash-out. The dashed grey line plots a similar series taken from Freddie Mac's Quarterly Cash-Out Refinance Report. This series reports the share of all refinance mortgages in Freddie Mac's portfolio that were at least five percent larger than the loan they replaced. Again, the two methods for measuring the cash-out share generate very similar time series. Both the DataQuick series and the Freddie Mac series show clear cyclicality in the early and late 2000s, mimicking the cyclicality of interest rates during that period. The correlations in this case are slightly lower but nonetheless still quite high (0.87 for the full sample and 0.82 for the analysis sample). Taken together, the evidence pre-

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

[6] The criteria used to select the analysis sample are described in detail in Section 4.3.

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

sented in Figure B.2 suggests that any measurement error in my equity extraction variable is not substantial enough to affect the ability of that variable to accurately measure changes in equity extraction over time.

B.4 Estimating Local House Price Indices

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

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

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

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

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

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

quarter t. Then the price index for that planning area and quarter is given by

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

where quarter zero is the base period used to normalize the index. Figure B.3 plots the price indices for all 28 planning areas normalized to 100 in the first quarter of 2000. In general, prices in Montgomery County evolved similarly to the national housing market over this period; however, there is substantial heterogeneity even within the county. Some rural areas barely saw any changes in prices over this period, while prices nearly tripled in some of the more volatile areas of the county.

B.5 Matching the DataQuick Transactions Data to HMDA

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

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

I then matched the data using a straightforward iterative process that proceeded in 6 steps, rematching unmatched transactions and loans on increasingly lenient criteria at each step. In the first step, each transaction was matched to a loan using the year in which the transaction occurred, the census tract number, the loan type (Conventional, FHA, VA), the exact lender name, and the exact loan amount. [10] In cases where there were multiple matches, one of them was randomly

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

[10] HMDA rounds loan amounts to the nearest \$1,000. Accordingly, I used a rounded version of the DataQuick

assigned as being the true match while the rest were considered unmatched.

Remaining unmatched observations were then re-matched again based only on year, census tract, exact lender name, and exact loan amount, with multiple matches being randomly assigned as in the first step. The next two steps repeated the first two but used a truncated version of the lender name containing only the first 5 letters. The last two steps omitted the lender name entirely. Any observations remaining after this process are considered unmatched.

In total, 70.8 percent of the 233,870 transactions in the cleaned DataQuick file were matched at some point in the procedure. Of those, approximately 76 percent were matched in the first step requiring an exact match on the full lender name and including the loan type. Of the matched observations, 12.6 percent were randomly assigned due to having multiple matches.

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

B.6 Residential Building Permits Data

The data used to measure residential investment activity was obtained from the Montgomery County Department of Permitting Services and includes address-level information on all residential building, home improvement, and mechanical permits issued by the department since 2000. Each record includes information on the date the permit was applied for, the street address for the associated property, the type of work to be performed, and the type of structure on

loan amount to conduct the merge, but required that the rounded amounts agree exactly in both datasets. Census tracts were all converted to reflect boundaries as of the 2000 census using a crosswalk file provided by the U.S. Census Bureau.

which the work will be performed. The permits data covers all areas of the county except for the cities of Gaithersburg and Rockville, which have their own permitting departments. The data covers both new construction and any major renovations, alterations, improvements, or additions to a home as well as any work performed on the heating, ventilation, and air-conditioning (HVAC) system. I drop any records where the listed structure type is clearly non-residential (e.g. "Restaurant," "Commercial," "Industrial") as well as any records for which the listed work type is not related to the actual improvement or alteration to the property (e.g. "Inspect and Approve", "Information"). The remaining dataset contains 148,647 unique permit applications, which I match to the DataQuick assessment file using the same approach used to match the list of MPDU addresses (see Appendix B.1) but allowing for multiple permits to match to the same property. Of the original 148,647 permits, a total of 137,510 (92.5 percent) were matched to a DataQuick property at some stage of the matching procedure. These permits were then used to construct the annual property-level panel used in the analysis as described in Section 7.

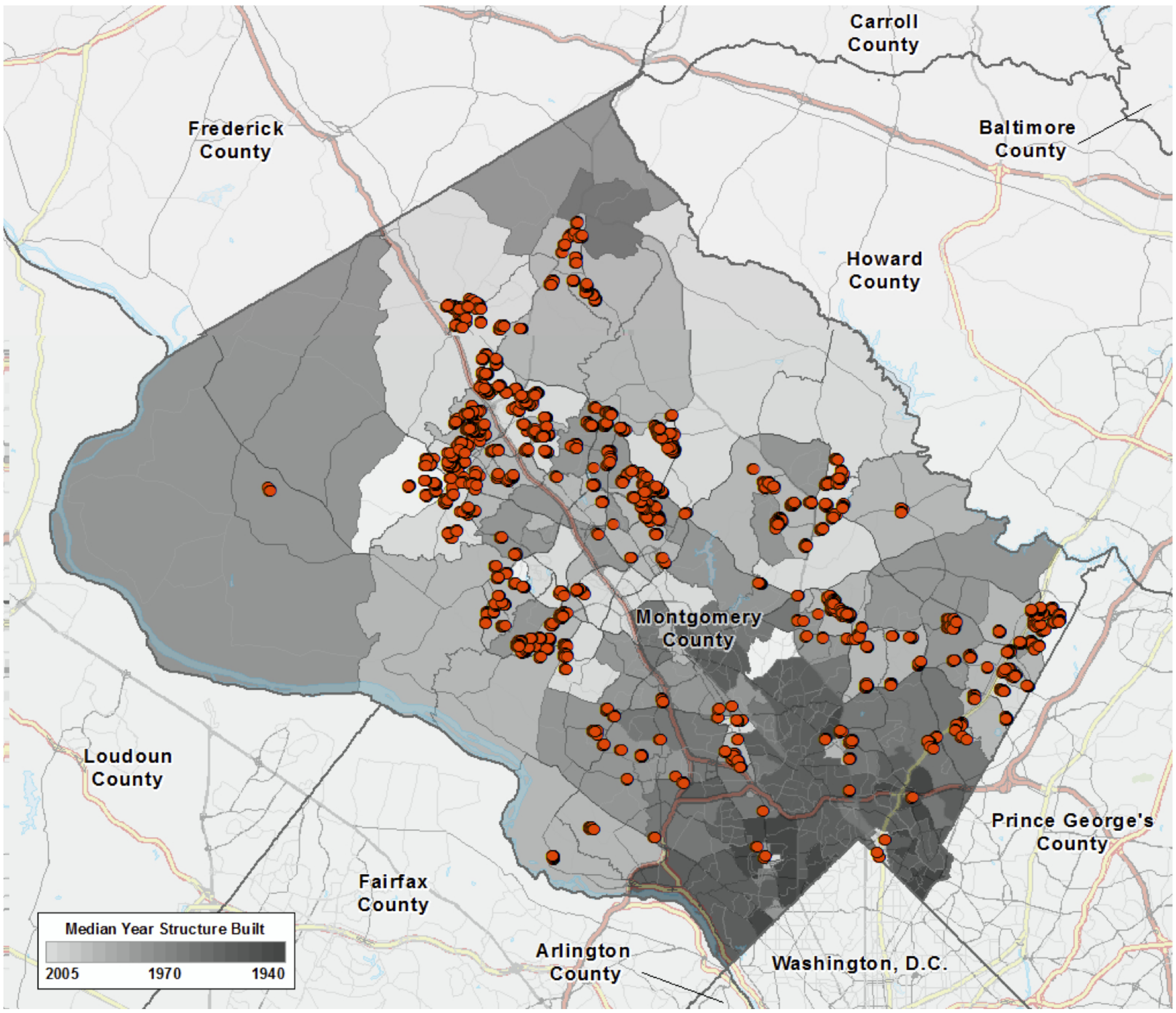
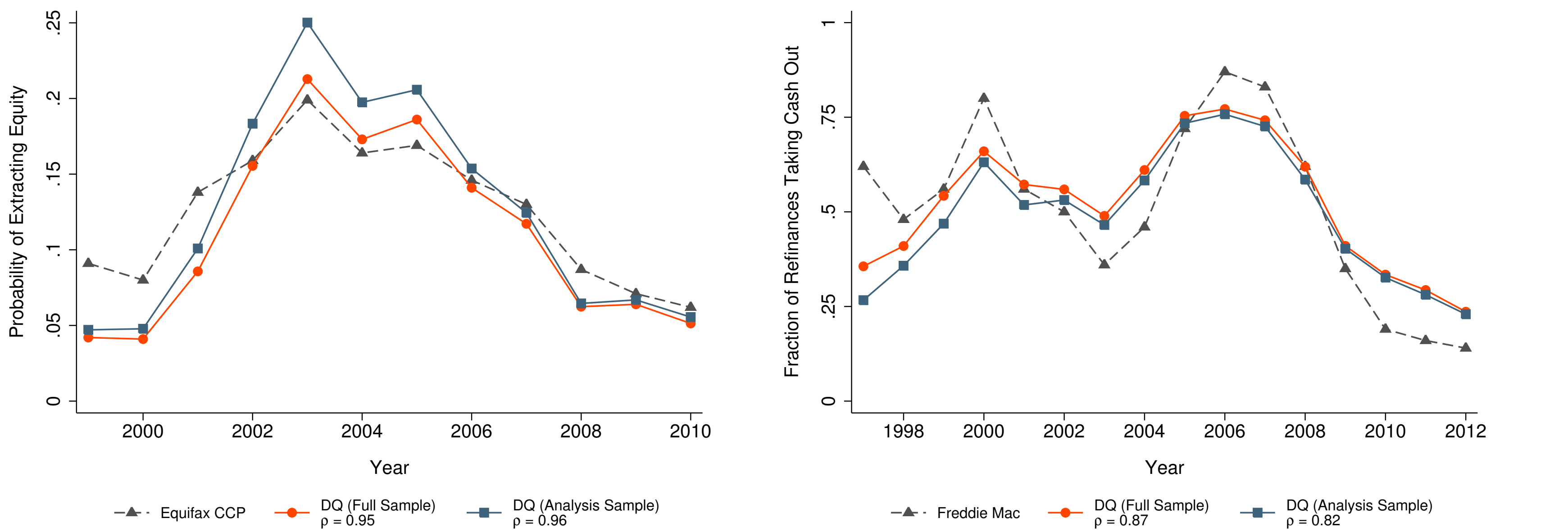


FIG. B.1.—Geographic Distribution of MPDU Properties within Montgomery County, Maryland. This figure shows the location of all MPDU properties that were successfully matched to a property in the DataQuick assesment file (N=7,404). MPDU properties are marked with an orange circle. Census tracts within Montgomery County are shaded according to the median year built for all housing units in the census tract as reported in the 2010 American Community Survey.



Panel A. Probability of Extracting Equity

Panel B. Cash-Out Refinance Share

FIG. B.2.—Validating the Home Equity Extraction Measure. This figure provides evidence validating the accuracy of the home equity extraction measure derived from the DataQuick deeds records against nationally representative aggregate series derived from other sources. Panel A. plots the yearly aggregate probability of extracting equity. Panel B. plots the yearly fraction of refinance mortgages that were cash-out. In both panels, the solid lines were generated using the equity extraction measure for Montgomery County derived from DataQuick for the full sample (orange circles) and the analysis sample (blue squares). In Panel A., the dashed grey line was taken from Bhutta and Keys (2014) and constructed using nationally representative borrower-level data from the Equifax Consumer Credit Panel. In Panel B., the dashed grey line was constructed using data from Freddie Mac's Quarterly Cash-Out Refinance Report. The correlations between the DataQuick measures and the corresponding national aggregate measures in each panel are reported in the legend. The time span in Panel A. is shorter than that of Panel B. because Bhutta and Keys (2014) only report their measure for the period 1999–2010.

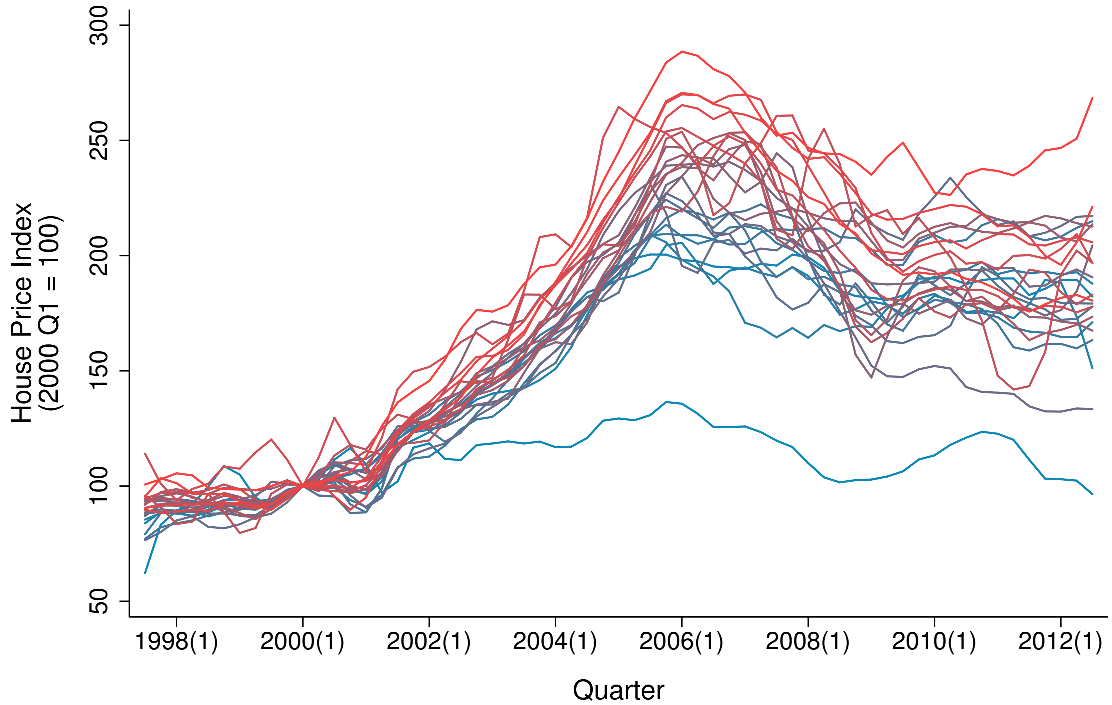
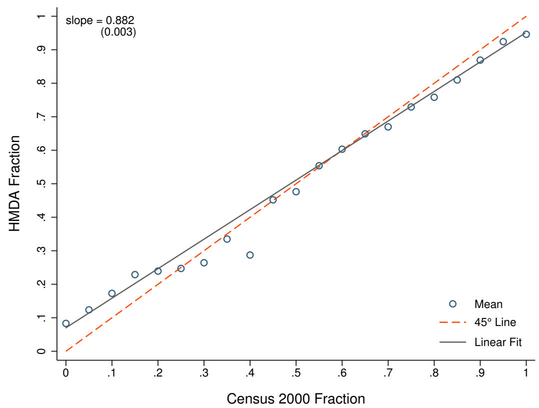
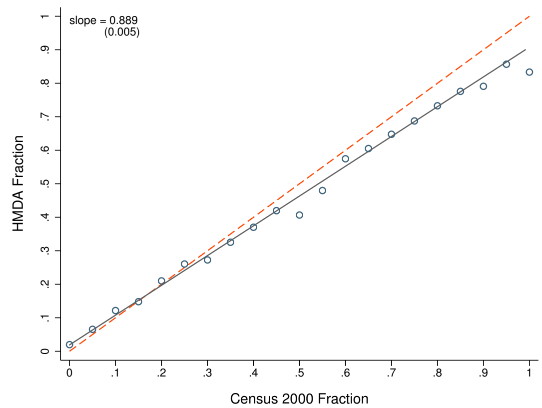


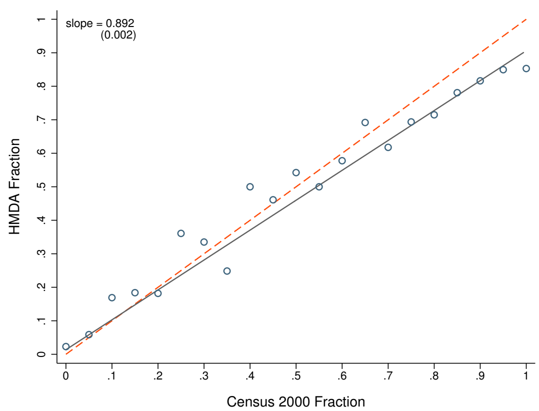
FIG. B.3.—Quarterly Hedonic House Price Indices for Montgomery County Planning Areas. This figure plots quarterly constant-quality hedonic price indices over the period 1997–2012 for each of the 28 local planning areas designated by the Montgomery County Planning Department. Each index was generated from the predicted values of a regression of (log) transaction price on a series of property characteristics, seasonal dummies, and planning area by quarter fixed effects as described in ??. All series are normalized to 100 in the first quarter of 2000 and are shaded according to their maximum value obtained over the entire period.



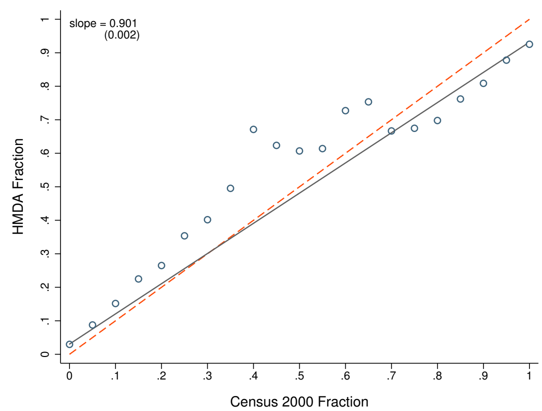
Panel A. White



Panel B. Black

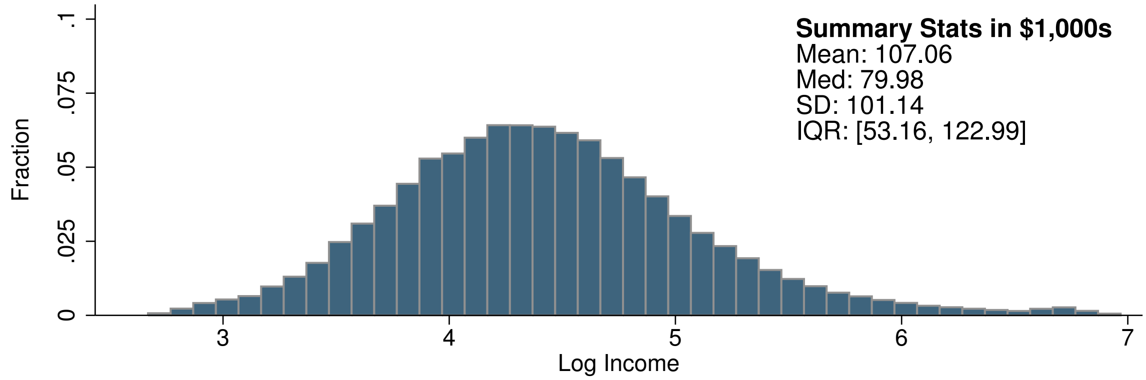


Panel C. Hispanic

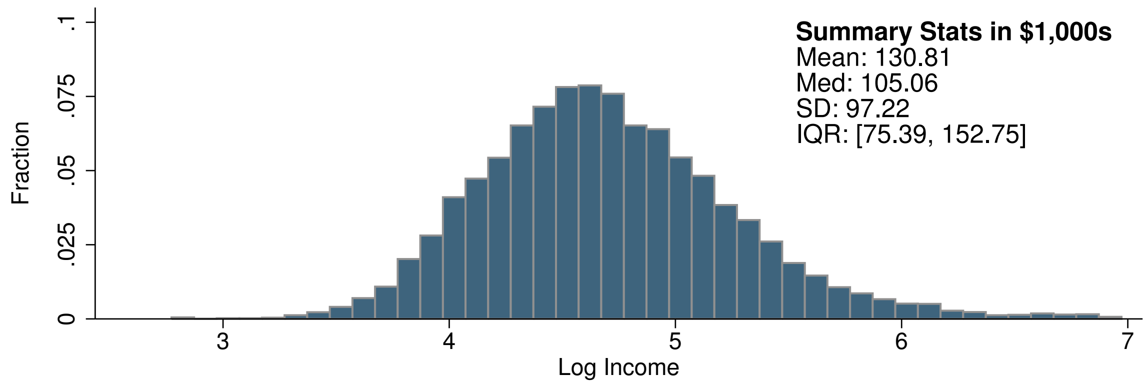


Panel D. Asian

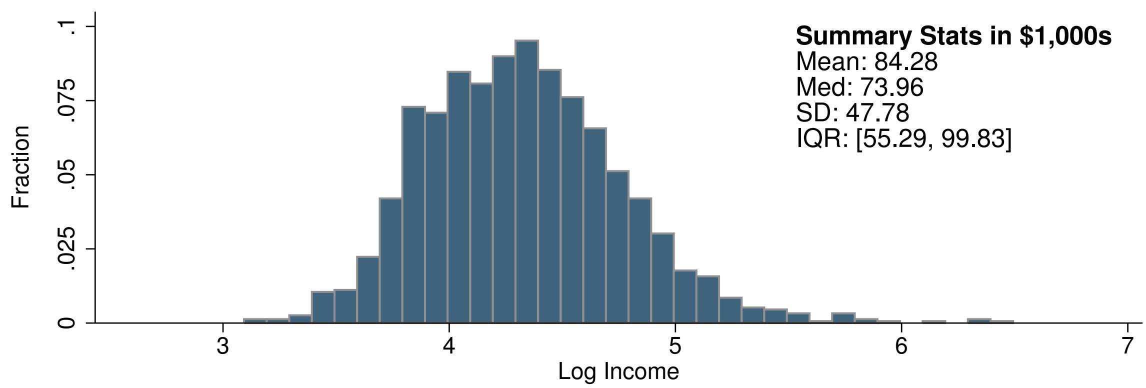
FIG. B.4.—Validating the DataQuick HMDA Match. This figure presents evidence validating the quality of the match between DataQuick housing transactions and HMDA loan applications. In each panel, the blue circles plot the fraction of matched transactions belonging to the indicated race as reported in HMDA on the y-axis against the fraction of households with the same surname as the home buyer who belong to that race as implied by the list of the 1,000 most popular surnames provided by the U.S. Census on the x-axis. The solid black line in each panel is the fit from a linear regression fit in the underlying microdata. The slope coefficient from that regression and its standard error are also reported in each panel. The dashed orange line is the 45-degree line.



Panel A. National Sample



Panel B. Full Analysis Sample



Panel C. MPDU Only Sample

FIG. B.5.—Distribution of Homebuyer Income. This figure plots the distribution of (log) homebuyer income (in real 2012 $1,000s) as reported on loan applications contained in the HMDA data for three separate samples. Panel A. shows the distribution for the set of all approved purchase-mortgage applications filed in the United States between 1997 and 2012 for which the borrower reported a non-missing income ($N = 62,267,113$). Panel B. restricts the sample to include only mortgage applications that were successfully matched to a transaction contained in the primary analysis sample ($N = 21,696$). Panel C. further restricts the sample to include only applications that matched to a transaction in the analysis sample that involved an MPDU property ($N = 1,523$). Each panel also reports the mean, median, standard deviation, and interquartile range in levels for the plotted log income distribution. A bin width of 10 log points is used in all three panels.

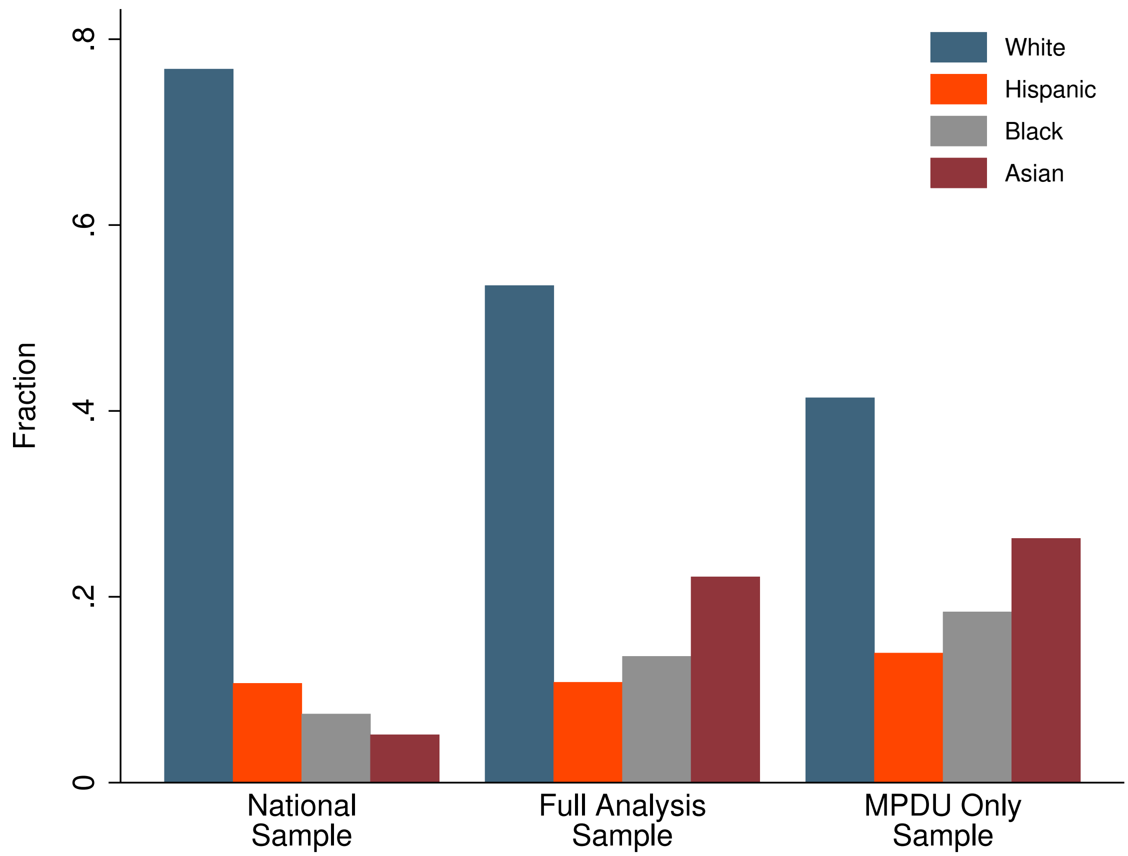


FIG. B.6.—Distribution of Homebuyer Race. This figure plots the distribution of homebuyer race as reported on loan applications contained in the HMDA data for three separate samples. The first set of bars shows the racial breakdown for the set of all approved purchase-mortgage applications filed in the United States between 1997 and 2012 for which the borrower reported a non-missing race or ethnicity of either White, Black, Hispanic, or Asian ($N = 57,710,449$). The second set of bars restricts the sample to include only mortgage applications that were successfully matched to a transaction contained in the primary analysis sample ($N = 19,780$). The third set of bars further restricts the sample to include only applications that matched to a transaction in the analysis sample that involved an MPDU property ($N = 1,427$). Racial shares are calculated only within the sample of applications with one of the indicated races so that the height of the bars adds to one within each sample. The four categories are mutually exclusive, meaning that a borrower is categorized as Hispanic if she reports an ethnicity of Hispanic regardless of which race she reports.

C Additional Results and Robustness Checks

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

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

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

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

where $Sold_{ist}$ is an indicator for whether property i in subdivision s was sold in year t, and all other variables are as described in Section 5 in reference to equation (10). The coefficient of interest is β_2, which measures the differential change in the turnover rate for MPDUs relative to non-MPDUs following the expiration of the price control, holding constant individual housing characteristics and aggregate differences in turnover rates across subdivisions and over time.

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

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

To give a sense of the dynamics of the turnover effect, Figure C.1 plots estimates from a version of equation (C.3) that allows the effect of the price control to differ separately for MPDUs and non-MPDUs by year relative to the first control period expiration (as described in the discussion of equation (11) in Section 5). Specifically, the series in orange squares plots the coefficient estimates on a set of dummies indicating whether the year of observation falls in a given *relative* year as measured from the year the first MPDU in the relevant subdivision expired (relative year zero). This series measures the trend in the turnover rate for non-MPDU properties around the time the price control expired. Relative year −1 is the omitted category so that all estimates should be interpreted as relative to the year prior to when the first price control in the subdivision expired. Similarly, the series in blue circles shows the trend for MPDU properties. This line plots the sum of the relative year main effects (the series in orange squares) and the interaction of those effects with an indicator for whether the property is an MPDU. The figure also reports the 95 percent confidence interval for that sum. All controls included in column 3 of Table C.1 were also included in the regression. As the figure makes clear, the turnover rate at MPDU properties exhibits a sharp departure from it's pre-period trend precisely in the year the first price control expires while there is no corresponding change for non-MPDU properties. Moreover, the trends for MPDUs and non-MPDUs are statistically indistinguishable in the period prior to the expiration of the price control and only diverge beginning in the year of expiration.

C.2 Propensity Score Matching Estimates of the Borrowing Response

Another potential concern with the main difference-in-differences estimates provided in Section 6 is that they rely on standard OLS estimation, which can be sensitive to differences in the distribution of covariates across treatment and control groups and relies heavily on extrapolation in areas where the covariates do not overlap (Imbens, 2004). In this section, I explore the sensitivity of the main results to an alternative estimation approach which restricts attention to the set of properties with overlapping characteristics and constructs the counterfactual outcome for each MPDU property using a locally weighted average of the outcomes among the non-MPDU properties whose characteristics are most similar. Specifically, I provide estimates based on the local linear propensity score matching difference-in-differences estimator developed in Heckman et al. (1997, 1998).

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

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

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

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

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

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

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

With the matched outcomes in hand, I can then directly calculate the propensity score matching difference-in-differences estimate given by equation (C.4). To calculate the standard error for this estimate, I bootstrap the entire process using a stratified resampling procedure that randomly samples properties with replacement in a way that ensures that the number of properties sampled from each subdivision stays the same. Table C.3 reports the matching estimates for each of the three main outcomes—log transaction prices, the annual probability of extracting equity, and the total amount of equity extracted per year. The estimated effect for all three outcomes is positive and precisely estimated. The equity extraction estimates are almost identical to the main OLS difference-in-differences estimates reported in Table 3 and Table 4. Similarly, the price effect is slightly larger but qualitatively similar to the main estimates reported in Table 2. Together, these results suggest that the main estimates are not being greatly affected by the fact that OLS relies on extrapolation in regions of the covariate space with poor overlap.

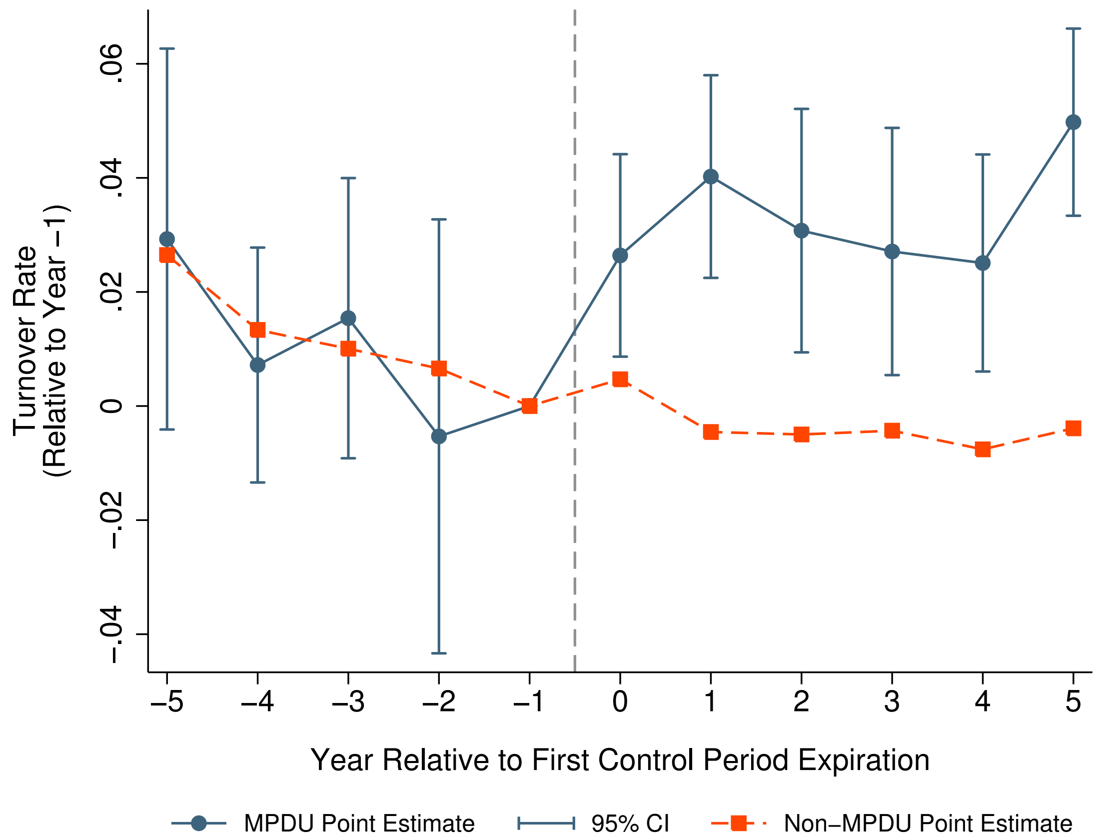


FIG. C.1.—Dynamic Effects of Expiring Price Controls on Turnover at MPDU Properties. This figure reports estimates of the effect of expiring price controls on the annual turnover rate at MPDU properties derived from a flexible difference-in-differences regression that allows the effect to vary by year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

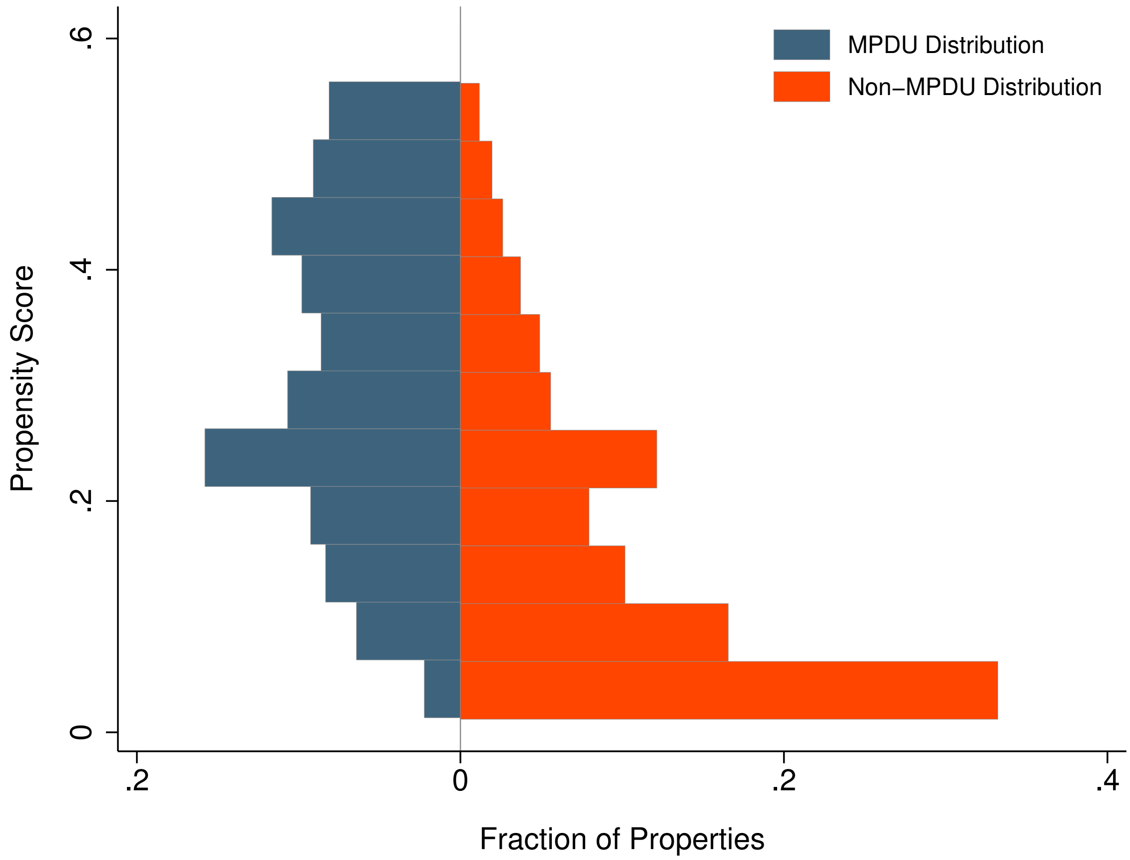


FIG. C.2.—Propsensity Score Overlap. This figure shows the overlap in the distribution of estimated propensity scores for MPDU and non-MPDU properties in the region of common support. The propensity score was estimated using a simple probit regression of the MPDU dummy on a set of property characteristics that included the interior square footage of the home, the year it was built, the number of bathrooms, the number of stories and an indicator for whether the property is a condo or townhouse. These covariates were entered in a flexible fashion that included cubic splines in square footage and year built as well as the linear interaction of both of those variables with a fully interacted set of dummies for the number of bathrooms and the number of stories. All of these terms were then further interacted with the condo dummy. The region of common support is defined as the set of all propensity scores that are larger than the maximum of the first percentile in the distribution of propensity scores in both sets of properties and smaller than the minimum of the 99th percentile of propensity scores in both distributions.

TABLE C.1

THE EFFECT OF EXPIRING PRICE CONTROLS ON TURNOVER AT MPDU PROPERTIES

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

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

TABLE C.2
COVARIATE BALANCE WITHIN TERCILES OF THE PROPENSITY SCORE DISTRIBUTION

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

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

TABLE C.3
PROPENSITY SCORE MATCHING DIFFERENCE-IN-DIFFERENCES ESTIMATES

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

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