Access to Home Equity and Consumption: Evidence from a Policy Experiment[*]

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September 2015

[*] We thank Zhou Li for the excellent research assistance. We benefited from the comments of Brent Ambrose, Dragana Cvijanovic, Zahi Ben-David, Souphala Chomsisengphet, James Choi, Yuming Fu, David Hirshleifer, Erik Hurst, Soren Leth-Petersen, David Laibson, Jessica Pan, Ivan Png, N.R. Prabhalla, Amit Seru, Nick Souleles, Amir Sufi and seminar participants at Institute of Real Estate Studies 2013 and NUS.

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Abstract

Using unique consumer financial transactions of more than 56,000 consumers, we study the consumption response to a housing policy experiment in Singapore that resulted in a decrease in access to home equity. Using difference-in-differences analysis, we find a significant negative consumption response to the policy shock. Moreover, the consumption response is concentrated in credit card spending, and is stronger among individuals with limited access to credit market or with high precautionary saving motive. These results suggest that a decrease in access to home equity reduces the role of housing as a self-insurance mechanism for consumption smoothing.

Keywords: Consumption, Spending, Debt, Credit Cards, Home Equity, Household Finance, Banks, Loans, Durable Goods, Discretionary Spending, Precautionary Savings, Credit Constraints.

JEL Classification: D12, D14, D91, E21, E51, E62, G21, H31

1. Introduction

The majority of a household's wealth is stored either in financial markets or in their house (Case, Quigley, and Shiller, 2005, 2013). A large literature has looked at the impact of homeowners' housing wealth on individual consumption and savings (e.g., Campbell and Cocco, 2007; Gan, 2010; Scholnick, 2013)[1]. More recently, Keys et al. (2014) also document significant real economy implications of housing leverage, which highlights the important role of housing equity and leverage in aggregate. A relatively less studied question is how *access* to home equity affects consumption, which bears important implications on the exact channels through which households use their home equity to smooth consumption. One exception is Leth-Petersen (2010), who uses an exogenous increase in access to credit from home equity in Denmark to study the consumption response. The setting allows the author to draw causal inferences, but he finds no average treatment effect, and even among the subgroup of individuals more likely to have a strong response, the economic effect is small.

Moreover, we have limited understanding on whether and how consumption responds to a negative shock to home equity access, despite an increasing interest in the question since the beginning of the recent financial crisis. The United States experienced declines in both housing prices and consumption during the first 12 quarters after the onset of the crisis in 2007, with an aggregate decline in housing prices of more than 10% and a consumption decline of similar magnitude. Furthermore, at the same time, an average household experienced decreasing access to (cash-out) refinancing against their home equity (Mian and Sufi, 2012). Mian, Rao, and Sufi (2013) use zip code–level house price and consumption data and find a pattern consistent with an unprecedented buildup in household leverage, followed by a shock to household collateral and a sharp collapse in household spending. One challenge is to distinguish the consumption response to the decreasing access to home equity (using refinancing or other forms of credit) from the response to declining house prices, because the two forces are intertwined: the crisis leads to both house price declines *and* tightened credit access.

[1] Using aggregate data, several studies provide evidence that housing wealth affects consumption (Skinner, 1989; Carroll, Otsuka, and Slacalek, 2011; Case, Quigley, and Shiller, 2013). Other related studies include Engelhardt (1996), Hoynes and McFadden (1997), Hurst and Stafford (2004), Agarwal (2007).

In this paper, we exploit a unique housing policy experiment in Singapore that isolates a negative shock to home equity access and study the associated consumption response. Specifically, the government agency Housing and Development Board (HDB), in an effort to cool down the heated housing market and reduce speculative activities, announced (and implemented at the same time) on August 30, 2010 that the minimum occupation period for resale flats in the public housing market was extended from 3 years to 5 years.[2] The minimum occupation period means that a public housing owner must live in the flat for the minimum requisite period before he or she is allowed to resell it. Since public housing owners are prohibited from cash-out refinancing against their home equity, this new policy directly affects their only option to access their home equity. This (negative) shock to home equity access is economically significant. To illustrate, a typical HDB owner had accumulated a significant amount of home equity at the time of the policy: his house price had appreciated, on average, S$ 114,000 (the exchange rate in June 2010 was 1 S$ = 0.71 US$) over the previous three years. The new policy not only directly affected recent owners (who had purchased their homes less than five years ago), but it also changed the expected liquidity (i.e., ease of access) in the future for other home owners. In contrast to studies of the U.S. housing market during the recent financial crisis, our sample does not experience a concurrent decrease in the level of home equity; house prices continue to rise after the policy shock. This nice feature allows us to disentangle the access to home equity effect from the housing wealth level effect.

We use a unique panel data set of consumer financial transactions, obtained from the leading bank in Singapore, which has a market share of more than 80% of the entire population in the country. For this analysis, we use the credit card and debit card spending transactions of a representative sample of more than 56,000 consumers in Singapore to study how consumers respond to the housing policy shock that was announced and implemented in August 2010. Compared to Leth-Petersen (2010) who finds little consumption response to a similar shock using annual survey data, we are able to perform a more powerful test based on monthly

[2] In contrast to other countries (e.g., U.S.), the Singapore government typically announces and implements policy changes without a lengthy process of discussion (see also another unanticipated government program discussed in Agarwal and Qian (2014)). We also verified by searching newspaper articles using Factiva: while there were 16 articles discussing the policy right after the announcement that the minimum occupation period for HDB resale flats is increased from 3 years to 5 years (i.e., August 30-31, 2010), there is no single article in the local news media on the issue during the five months before the policy announcement that correspond to the pre-policy window in our data (April 1, 2010- August 29, 2010).

transaction-based spending by individuals from an administrative dataset that has little measurement error. Other recent research in the literature uses credit card spending to measure consumption (e.g., Gan, 2010). We further improve upon this measure by including data on the other important spending instrument—debit cards. The purchase volume on debit cards was similar in magnitude to that on credit cards in the United States in 2012 (2.1 trillion vs. 2.3 trillion U.S. dollars) (U.S. Census Bureau, Statistical Abstract of the United States, 2012). Similar to the U.S., debit and credit cards are important mediums of disposable consumption in Singapore: they together make up close to 30 percent of aggregate personal consumption in the country. Moreover, more than a third of consumers in the country have a credit card, and the total credit card debt as a percentage of GDP was over 2 percent in Singapore in 2011 (Department of Statistics Singapore, 2012). This is comparable with the U.S., where half of consumers in the United States have a credit card, and total credit card debt was close to a trillion dollars in 2012 with 40 percent of revolving debt (U.S. Census Bureau, Statistical Abstract of the United States:2012). Therefore, our data provide a more complete and accurate measurement of individual consumption at a high frequency.

In addition, our data allow us, at the individual level, to study the role of credit in the consumption response. Because consumers in our sample are not allowed to undertake cash-out refinancing, consumption smoothing against housing collateral must rely on other sources of credit, out of which credit card is the dominant instrument. By restricting the home owners' option to access home equity, the policy significantly increases the cost of consumption smoothing using credit cards due to a greater likelihood of accumulating a large amount of (costly) credit card debt for an extended period of time. This in turn makes debit card (i.e., accumulative savings) a relatively more appealing instrument for smoothing consumption. By comparing the spending response on credit cards and debit cards, we can also study the policy's nuanced effect on the channels of the spending response.

We implement a difference-in-differences identification strategy to study the impact of the decrease in home equity access on consumption. Because our data do not cover home ownership information, we cannot precisely identify the treated homeowners. Instead, we exploit a slightly different strategy and identify the ex-ante high home equity (low home equity) consumers to be in the treatment (control) group. The idea is that on average consumers with

more home equity respond more negatively to the policy than consumers with less home equity. Since the housing policy change was unanticipated, the level of the home equity before the policy is orthogonal to the announcement of the policy shock. This gives us a fuzzy identification: any impact we find of the housing policy on consumption is plausibly the lower bound of the true estimate. To validate our research design, we explicitly test for parallel trends in spending behavior before the policy announcement in all our analysis.

We find that after the negative shock to home equity access, the treatment group experiences a significant reduction in their total card spending relative to the control consumers. Overall, individuals in the treatment group decrease their total card spending by 4.2% per month during the six-month post-policy period, relative to the change for consumers in the control group. On the other hand, we find no spending response using cash or checks,[3] which are typically used for non-discretionary and expensive consumption items. Moreover, the decrease in card consumption is driven by non-durable spending. Therefore, the evidence suggests that the consumption primarily responds in discretionary or less expensive spending items. Overall, the treatment group experiences a cumulative decrease of 31% in their total card spending during the six-month post-policy period. This is equivalent to 10% decrease in total consumption over that period (considering that card spending comprising 30% of the total consumption), and is also equivalent to 5.1% of the treated consumer's average monthly income. In addition, we show that this result is unlikely driven by a decrease in the level of housing wealth that could potentially result from a slower house price growth rate after the policy shock.

Home equity serves as a self-insuring mechanism that helps individuals smooth consumption inter-temporally (e.g., Lustig and Van Nieuwerburgh, 2005, 2010). Since home equity becomes more illiquid after the policy shock, consumption smoothing through credit cards becomes more costly, making debit card a relatively more appealing instrument for smoothing consumption. Consequently, treated individuals not only are expected to reduce their overall consumption level, but they would also decrease spending first and primarily on credit cards. We find consistent evidence that the consumption response to the policy shock is concentrated in credit card spending. There is no consumption response in debit card use.

[3] Our data do not provide explicitly the cash/check spending information, but we try to infer the information from the number of bank transactions and the monthly change in bank balances (net of credit card and debit card spending).

We further explore the heterogeneity in the consumption response. First, we expect the importance of home equity for consumption smoothing depends on the degree of credit market completeness. Indeed, the consumption response is stronger among consumers with more restricted access to the credit market: these consumers are more reliant and thus are more vulnerable to such a decrease in home equity access. Second, a weaker access to the home equity after the policy shock reduces the consumption smoothing capacity and effectively increases uncertainty over future consumption. As a result, the reduced consumption smoothing capacity should have a greater impact on individuals with a higher precautionary saving motive. Using the level of the pre-policy bank account balance as the proxy, we find that individuals with a higher precautionary saving motive decrease their card spending more than those with a lower precautionary saving motive within the treatment group. Last, the consumption response is concentrated among the treated individuals living in areas that have experienced a higher price appreciation (and hence a greater increase in home equity) before the policy. This again supports the interpretation that the housing policy affected consumption by restricting home owners' capacity to extract home equity.

To further test the internal validity of our identification, we use an alternative proxy to identify the high home equity (treatment) and low home equity (control) groups, as well as a matched sample analysis to control for observable differences between the treatment group and the control group. We obtain the same results. We also perform various falsification tests, either by using the same treatment and control group identification in a comparable housing market that is unaffected by the policy shock, or by using the same identification among the non-owner sub-population in the treated housing market. We find no effect in the unaffected housing market or among the non-owner subpopulation, which confirms our research design. Because the housing policy shock was unanticipated, these results suggest a causal impact of a decrease in home equity access on consumption.

A large portion of wealth, for households and in the aggregate, is stored in housing. Therefore, identifying the channels through which housing wealth affects consumption bears important macro implications. In this aspect, we note that housing plays an equally important role in shaping the household lifetime wealth and the aggregate economy in Singapore as it does in the United States. Home ownership rate of the resident population in Singapore is 87.2% in

2010 (Department of Statistics Singapore), compared to 66.9% in the U.S. in 2010 (U.S. Census Bureau). Housing value (relative to income) is also significant in Singapore. The average price for a HDB flat in our sample period is equivalent to seven times the median individual annual income in Singapore (Urban Development Authority of Singapore, Ministry of Manpower of Singapore). In comparison, the median house price is over three times the median household annual income in the U.S. (Zillow.com, U.S. Census Bureau). Therefore, findings in this paper bear policy implications in a broad context. Specifically, a negative shock to home equity access, independent from a shock to the housing wealth level, has a causal impact on consumption decrease, likely through the channel of weakening the role of home equity as the consumption smoothing device. Lastly, the paper also contributes to the vast literature on the consumption response to various income shocks.[4]

The remainder of the paper flows as follows. Section 2 discusses the housing market in Singapore and the specific policy experiment that occurred in August 2010. Section 3 describes the data. Section 4 explains the identification strategy and the econometric methodology. Section 5 presents the empirical results. Section 6 concludes.

2. Public Housing in Singapore and the 2010 Policy Experiment

In Singapore, the residential property market consists of two main types: public housing and private housing. Public housing, or HDB flats, is open to most Singaporean citizens and permanent residents (except singles < 35 years old), and is heavily subsidized by the Singapore government.[5] In 2009, HDB homes accounted for about 78% of the 1.13 million residential properties in Singapore. More than 80% of Singaporeans live in HDB flats, which are developed and closely governed by the Housing and Development Board (HDB). These flats are located in housing estates, which are self-contained satellite towns with schools, groceries, clinics, food courts, and sports and recreational facilities. Such estates are located throughout the residential

[4] Some recent studies include Shapiro and Slemrod (1995, 2003a, 2003b), Souleles (1999, 2000, 2002), Parker (1999), Browning and Collado (2001), Hsieh (2003), Stephens (2003, 2005, and 2006), Johnson, Parker, and Souleles (2006), Agarwal, Liu, and Souleles (2007), Aaronson, Agarwal, and French (2012) and Agarwal and Qian (2014).

[5] The government subsidizes new flats in the public housing market by selling them at below-market prices (that are stipulated by the government) to qualified Singaporeans. The government does not intervene in the market for resale flats, whose transaction prices are determined by the market's demand and supply. The subsidization also partly works through loan financing. Although home buyers can always borrow from banks, the Housing and Development Board offer loans to HDB flat buyers that have attractive financing terms for qualified HDB flat buyers (e.g., low income buyers).

areas in the country with convenient access to public transportation, and there is no geographical concentration (that is distinct from the private housing market). There are a large variety of flat types and layouts to cater to various housing budgets.

One avenue for owning a flat in the public housing market is through the purchase of a new flat directly from the HDB. The current mode of sale is known as the Build-To-Order program (BTO program, hereafter), launched in 2001. Qualified Singaporeans apply and, if successful, must wait for more than four years before the building is completed. Because of the price subsidization offered to the new flats, the eligibility criteria are strict: only citizens with a family that have an income below the stipulated cap can buy a new HDB flat. In addition, the new flats supply is small relative to the entire housing stock. For example, the total public housing stock in 2013 is about 1.044 million units, and the number of BTO supplies on average is 26,000 units per year during the period of 2010-2012.[6] For these reasons, the main way to own a HDB flat is through the resale market, especially for eligible buyers who wish to move in immediately, or for those who do not qualify for new HDB flats. For example, those buyers qualified for owning HDB but ineligible for buying new flats—namely, permanent residents, single citizens who are older than 35, and citizens with high income—can own resale HDB flats. The government does not intervene on house prices in the resale market: existing flat owners are allowed to sell their flats on the open market to any eligible buyer at a mutually agreed upon price. On the other hand, most flat owners can only sell their flats if they have met the Minimum Occupation Period (MOP) requirement, which requires HDB owners to live in their flats for a minimum period of time before they are allowed to resell them (whether they purchase new flats or resale flats).[7]

In response to a rapidly rising housing price in the resale market (see Figure 1) and to reduce speculative activities, the HDB announced on August 30, 2010, that it would extend the minimum occupation period for *resale* HDB flats from 3 years to 5 years, effective

[6] Please see http://www10.hdb.gov.sg/eBook/AR2013/keystatistics.html.

[7]The Minimum Occupation Period (MOP) requirement is strictly binding: even in the event of divorce, owners cannot sell their flats if they have not satisfied the MOP requirement (one party, typically the wife, is allowed to keep the flat). On the other hand, if owners default in the mortgage repayment and run up mortgage arrears, then the flat may be sold by the bank through a mortgagee sale, or compulsorily acquired by the HDB.

immediately.[8] Importantly, HDB flat owners cannot cash-out refinance against their home equity (by law), making selling the only way to access their home equity. As a result, this new policy directly reduced HDB resale flat owners' *only* option to access their home equity, which had risen in value for most homeowners in recent years. The resale market's price index experienced a cumulative increase of 41% from July 2007 to June 2010,[9] suggesting that HDB owners, on average, accumulated a considerable amount of home equity during the period. A four-room HDB flat (the most popular flat type) on average costs S$ 278,000 in the third quarter of 2007.[10] Assuming the average appreciation rate implied by the price index, the owner's home value had increased by S$ 114,000 by June 2010 (the exchange rate in June 2010 was 1 S$ = 0.71 US$). The new occupation period policy not only directly affected recent owners (those who had purchased a flat less than five years ago), but it also changed the expected future liquidity (i.e., ease of access) of home equity for the other homeowners. Note that our sample does not experience a concurrent decrease in the level of housing wealth (as can be seen in Figure 1); house prices continue to rise after the policy shock. This setting thus allows us to isolate the consumption response to a negative shock to home equity *access*.[11]

[Insert Figure 1 About Here]

Interestingly, the Singapore government implemented another policy in early 2011 by distributing cash to Singaporean citizens in the Growth Dividend Program (Agarwal and Qian, 2014). The policy has an objective of sharing the nation's economic growth in the past recent years. While the housing policy of extending the Minimum Occupation Period in 2010 appears contractionary in the sense of limiting consumers' resources, we note that policy makers have a consistent objective of ensuring a sustainable economic growth. For identification in this paper, we will focus on the period before the distribution of the Growth Dividends to isolate the treatment effect due to the Minimum Occupation Period policy.

[8]New HDB flat owners were already subject to the 5-year minimum occupation period restriction, so after August 2010 all HDB owners have to stay for at least five years before they can sell their flats.

[9] The price index series of the HDB resale market is obtained from the HDB website http://www.hdb.gov.sg/fi10/fi10321p.nsf/w/BuyResaleFlatResaleIndex?OpenDocument#Weights.

[10] http://www.hdb.gov.sg/fi10/fi10321p.nsf/w/BuyResaleFlatPrevMedianPrice?OpenDocument#2Q07.

[11] In section 5.3, we will discuss in greater detail on the alternative channel of consumption response through the policy's potential impact on the level of housing wealth.

3. Data

We use a unique proprietary dataset obtained from the leading bank in Singapore, which has more than 4 million customers, or 80 percent of the entire population of Singapore.[12] The entire dataset obtained from the bank contains consumer financial transactions between 2010:04 and 2012:03, including credit card and debit card transactions of more than 180,000 individuals, which is a random, representative sample of the bank's customers. For each individual in our sample, we have transaction-level information about his or her credit card and debit card spending during our sample period, including the transaction amount, transaction date, merchant name, and merchant category for each account. We also have the individual's bank checking account balance and number of transactions at the monthly level.[13] The data also contain a rich set of demographics about each individual, including age, gender, income, property type (i.e., whether the residence address is in the public housing market or private housing market), property address postal code,[14] nationality, ethnicity, and occupation.

This data set offers several advantages. First, our sample covers a large, representative panel of consumers with little measurement error, and it allows high frequency analysis. Compared to existing studies that use micro-level credit card data (e.g., Gan, 2010), we have more complete information on consumption of each individual in our sample. Rather than observing a single credit card account, we have information on every credit card, debit card, and checking account that those individuals have with the bank. Although we do not have information about accounts individuals have with other banks in Singapore, we suspect the measurement error is negligible

[12] As the largest bank in Singapore, it has more than twice the number of branches, and over four times the number of automatic teller machines (ATMs) than the other major banks in Singapore. On the other hand, the typical banking fees and other costs are quite similar between our bank and the other major banks in Singapore. Although we do not have information on whether consumers have other banking relationships, our bank is likely the dominant bank for our sample consumers' daily financial needs due to its greater convenience and comparable banking fees.

[13] The specific banking products that we study (credit card, debit card, and bank checking account) are similar to those used in the United States. Consumers are typically eligible for obtaining a bank checking account, and they can conduct banking transactions using branches, Automatic Teller Machines or ATMs (for cash withdrawals, transfers, or bill payment), checks, or online methods. The typical banking fees and other costs are quite standard as for a typical US bank, and moreover they are comparable with banking costs at other major banks in Singapore. Debit cards are linked to the bank checking account, and debit card transactions are drawn on the bank account balance. Similarly, credit cards are granted upon application to consumers who have met the bank's criteria (e.g., income, age, and credit profile). One interesting difference for credit cards is that all credit card holders with the bank have the same prevailing interest rate of 24% per annum, regardless of the credit card limit.

[14] Unlike the United States, where a zip code represents a wide area with a large population, a postal code in Singapore represents a building. Specifically, there is a unique postal code for a single family house as well as for a building with 10 apartment units.

given the market share of the bank. For example, an average Singaporean consumer has three credit cards, which is also the number of credit cards an average consumer has in our data set. In other words, we are picking up the majority of credit card and debit card consumption of these households. More importantly, a particular advantage of our data is that we can study the role of credit, at the individual level, in understanding the consumption response. Because the consumers in the study are not allowed to engage in cash-out refinancing, consumption smoothing against the housing collateral must reply on other lines of credit, with credit cards being the dominant means. By comparing the spending response on credit cards and debit cards, we can study whether consumption responds to the policy shock through consumer credit or using consumers' own liquid wealth. In addition, the richness of our transaction-level information as well as the individual demographics allows us to better understand heterogeneity in consumers' consumption response.

Similar to Agarwal and Qian (2014) that use the same dataset to analyze a different question, we aggregate the data at the individual month level. Credit card spending is computed by adding monthly spending over all credit card accounts for each individual. Debit card spending is computed by adding monthly spending across debit card accounts for each individual. Because we want to capture each sampled individual's entire spending during our time frame, we include only those consumers who have a bank account, debit card, and credit card account with the bank at the same time.[15] We exclude individuals/accounts that are inactive (i.e., accounts with zero monthly spending for at least half of the sample period (e.g., for 12 months if the account enters our sample in 2010:04)). Out of 180,000 consumers in our sample, there are over 115,000 number of active account holders who have all three accounts with the bank.[16]

Given that the housing policy announced on August 30, 2010 targeted public properties in Singapore to be an exogenous shock to home equity access, we restrict our main test sample to Singaporean individuals who are living in public housing (i.e., we exclude foreigners and those who live in the private housing market). To measure the effect among individuals who are truly exposed to the shock, we further restrict the sample to all Singaporeans older than 25. As a result, the set of eligible consumers for whom we have complete above-mentioned records prior to the

[15] In unreported results, we also confirm that our main results remain to hold without requiring the three account restriction.

[16] All our results continue to hold if we relax these specific data filtering requirements.

policy announcement includes more than 56,000 (Singaporean) consumers. The sample period in study covers 5 months before and 6 months after the policy announcement, which coincided with the policy implementation (i.e., 2010:04-2011:02). The main reason we do not extend the post-policy window beyond the 6 month period is because there are other policy changes (e.g., stimulus programs) in the early 2011 (Agarwal and Qian, 2014), which involves income pay out to these individuals and potentially confound the analysis and interpretation.

4. Identification and Empirical Strategy

The main analysis in this paper studies the consumption response to the public housing policy change, announced and implemented on August 30, 2010. The housing policy extends the required occupation period for home owners, which effectively reduces access to their home equity.[17] Ideally, a perfect control group in this study would be renters who have zero home equity and are not affected by the housing policy change. In such a case, the estimated change in consumption of the affected homeowners, relative to the change in consumption among the untreated consumers, would capture the magnitude of the consumption response to the negative shock to home equity access.

Because our data do not cover homeownership information, we cannot precisely identify the treated homeowners. Instead, we exploit a slightly different identification strategy based on the idea that, on average, consumers with a higher level of ex-ante home equity respond more negatively to the housing policy than consumers with a lower level of ex-ante home equity. We thus perform a difference-in-differences study by comparing the consumption response to the policy between high home equity Singaporeans and low home equity Singaporeans. We use marital status as our proxy for home equity. The eligibility criteria for buying HDB properties make single individuals much less likely to own public housing in Singapore. For example, during this time period, single consumers are prohibited from owning any type of public housing until they are older than 35.[18]

[17] An additional advantage of our setting is that the policy shock unlikely affects labor mobility of homeowners, given that Singapore is a small city-state with good urban planning and all major employment centers are easily accessible via public transportation. As a result, the consumption response of the treated homeowners following the policy shock would capture the effect of reduced access to home equity.

[18] Single Singaporeans can only purchase in the resale public housing market if they are older than 35 unless they are orphans (please see the following link for details

This proxy gives us a fuzzy identification: any impact of the housing policy on consumption we find is likely the lower bound of the true estimate. This is because we are potentially including treated homeowners in our control group (i.e., some single Singaporeans are homeowners), and these miscategorised individuals might respond to the housing policy shock. Moreover, some married individuals could be renting and are thus not affected by the policy shock. These unaffected individuals in the treatment group should not respond to the policy shock, making the average consumption response of the treatment group a downward biased estimate of the true consumption response of the treatment group. In sum, the fuzzy identification strategy raises the hurdle of identification and produces a conservative estimate of the consumption response to the housing policy.

Admittedly, the treatment (married Singaporeans) and the control (single Singaporeans) may not be a well-balanced sample due to observable and unobservable differences between the two groups, which could affect their consumption patterns. In our analysis, we will explicitly test for any difference between the treatment and control groups in the consumption during the pre-treatment period. If our research design is valid, we expect to observe an economically and statistically insignificant difference in spending between the two groups during the pre-treatment period (i.e., parallel trends in the pre-treatment period).

Furthermore, we use an alternative proxy for the level of ex-ante home equity and find similar results. Specifically, we restrict to a subsample of consumers who are under age 35. Singles under age 35, regardless of nationality, are not allowed to own public housing by law (except in very rare circumstances). We thus use singles under age 35 as our control group, as these individuals are renters and do not have home equity. This specification alleviates the measurement error as mentioned above. Lastly, we perform a matched-sample analysis, as well as various diagnostic checks and falsification tests on the validity of these identification strategies.

[Insert Table 1 About Here]

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http://www.hdb.gov.sg/fi10/fi10321p.nsf/w/BuyResaleFlatSingleScheme?OpenDocument). Single Singaporeans under age 35 are prohibited from buying directly from the government (i.e., BTO market, or Built-to-Own) during our sample period, but a recent policy change allows them to buy smaller units in the BTO market in specific locations after July 2013.

Table 1 provides summary statistics of demographics and spending information for married and single Singaporeans. First, note that married consumers, on average, live in locations where the regional average transaction prices between 2010 and 2012 are higher, which lends support to our assumption of using marital status as a proxy for the level of the ex-ante home equity.

Even though marital status is likely orthogonal to the housing policy shock, it may be endogenously related to wealth and other demographic variables. Married consumers on average have higher monthly income and checking account balances and are older. Married consumers spend S$ 123 more per month than single consumers. To the extent that the identifying assumption of the difference-in-differences analysis lies in the "parallel trends" assumption, the difference in the level of spending between the treatment and control groups is of lesser concern and we will test explicitly for the parallel trends between the two groups before the policy shock.

We analyze the response of credit card spending and debit card spending to the housing policy shock, using a difference-in-differences analysis. First, we study the average monthly spending response using the following specification:

$$\log(Y_{i,t}) = \beta_{post} \times 1_{treatment} \times 1_{post} + \beta_{pre} \times 1_{treatment} \times 1_{pre} + \alpha_i + \gamma_t + \epsilon_{i,t} \quad (1)$$

The dependent variable, $\log(Y_{i,t})$, represents the logarithm of the dollar amount of spending for individual *i* at the end of month *t*. $1_{treatment}$ is a dummy indicator for the treatment group, i.e., equal to one for married consumers (as of the month before the policy announcement) in the main analysis. 1_{post} is a dummy variable equal to one for the months after the announcement of the housing policy (i.e., > 2010:08). 1_{pre} is a dummy variable equal to one for the four months before the announcement of the housing policy (i.e., 2010:05-2010:08). γ_t is the year-month dummy, used to absorb the seasonal variation in consumption expenditures as well as the average of all other concurrent aggregate factors; α_i is the individual dummy included to absorb differences in consumption preferences at the individual level. β_{post} in Equation (1) captures the average percentage change in monthly spending for a treated individual after the housing policy announcement (compared to 2010:04, the first month in our sample period), relative to the change in spending of the control group. In other words, β_{post} is the estimated treatment effect. Similarly, β_{pre} measures the average percentage difference in monthly spending for a treated

individual between the four pre-treatment months (2010:05-2010:08) and the first (baseline) month in our sample period (2010:04), relative to the difference in spending of the control group. Thus β_{pre} should be zero if our difference-in-difference research design is valid.

In addition, we study the dynamics of the spending response. Specifically, we estimate the following distributed lag model:

$$\log(Y_{i,t}) = \sum_{s=-4}^{5} \beta_s \times 1_{treatment} \times 1_{\,month\,s} \; + \alpha_i + \gamma_t + \epsilon_{i,t} \qquad (2)$$

Following Gross and Souleles (2002), the results can be interpreted as an event study. The coefficient β_s measures the treatment group's spending change (in percentage term) in month *s* after the policy announcement as a percentage from month -5 (or 2010:04, the absorbed month), relative to the change in spending of the control group. Therefore, the *cumulative* coefficient $b_s \equiv \sum_{t=-4}^{s} \beta_t$ gives the cumulative change in spending after *s* months (from month -4), *s = 0-5*. The dynamic pattern of the spending response helps us understand the trajectory of the housing policy. We include the pre-treatment months (*s = -4, … , -1*) to explicitly test for the existence of pre-treatment trend differences (on a month-to-month basis) between the treatment group and the control group. Similar to the static specification (Equation (1)), we expect to see a zero cumulative spending response for *s < 0*.

Unless indicated otherwise, equations (1) and (2) are estimated using ordinary least squares (OLS), and the standard errors are adjusted for heteroskedasticity across accounts as well as serial correlation within individuals.

5. Results

5.1. Unconditional Spending Response

Before carrying out the econometric analysis, we first look at the unconditional mean of the monthly spending around the housing policy (2010:05–2011:02) of the married (treatment) and single (control) consumers. Figure 2 shows that the treatment group consumers consistently have a higher spending level than the control group consumers throughout the period, consistent with the summary statistics reported in Table 1. Moreover, the spending difference between the two groups of consumers remains steady before August 2010, lending support to the parallel

trends assumption for our difference-in-differences analysis. After August 2010, the spending gap visibly decreases, especially for credit card spending.[19]

[Insert Figure 2 About Here]

5.2. The Average Spending Response

Table 2 shows the average response after applying Equation (1) to spending. We first show the consumption response for the entire sample in Panel A. The first column shows the average response of monthly total card spending (i.e., debit card spending + credit card spending) by the treatment group. Overall, individuals in the treatment group decrease their total card spending by 4.2% per month (compared to the 5th month before the policy announcement, i.e., 2010:04), relative to the change for consumers in the control group. The effect is both statistically and economically significant. Decomposing total card spending into credit card spending and debit card spending suggests that the consumption decrease of the treatment group is concentrated in credit card spending. The treatment group consumers reduce their credit card spending by 9.9% per month after August 2010 (statistically significant at 1%), relative to the consumers in the control group (column 2, Table 2 Panel A). In contrast, there is little difference in the change in debit card spending between the treatment and control groups: the coefficient in column 3 of Panel A is positive (0.024) and more importantly is statistically indistinguishable from zero.

[Insert Table 2 About Here]

As a validation of our research design, we find that the coefficients β_{pre} for all three columns in Panel A of Table 2 are all small and insignificant. Specifically, the pre-treatment dummy coefficients for credit card and debit card spending (column 2 and 3) are positive and insignificant both economically and statistically. For the total card spending (column 1), although the β_{pre} coefficient is negative, it is statistically insignificant and economically small (F-test rejects the null that β_{pre} is equal to β_{post}). Collectively, this set of results validates our

[19] Note that the spending trend is in general flat (or even slightly positive). This is likely due to the overall growing economy in Singapore during this time period. Hence, our tests will focus on identifying a decreasing spending gap between the treatment and control group, which indicates a relative decrease (slowing down) in the spending behavior of the treatment group compared to that of the control group during this time period.

research design: there is no difference in the spending trend before the policy announcement, and the treatment group's spending decreased *only* after the policy shock that decreased their access to home equity.

Ideally we also want to test if individuals change their consumption via cash, checks, or other forms of debit transactions against their liquid wealth. Our data do not provide transaction-level information on cash and check spending, but we compute indirect measures based on the number of bank transactions and the monthly bank balance. In unreported results, we find no response in cash or check spending in the treatment group.[20]

To better understand the no-response using cash or checks (which overall accounts for 70% of the entire consumption), we note that consumption using those instruments are primarily non-discretionary. For example, people use cash or checks for big and recurring expenses such as tuition, mortgage, rent, and car loan payments (which they cannot pay using either debit or credit cards).[21] Therefore, the evidence suggests that the consumption primarily responds more, upon the policy shock that reduces the access to home equity, in discretionary or less expensive spending items. This is also consistent with Agarwal and Qian (2014) who find a large consumption response to another exogenous policy shock primarily through debit and credit cards using the same dataset.

To further study the nature of spending response, we also decompose the total card spending into durable and non-durable categories. Durable spending items on credit and debit cards include apparel, electronics, computers, appliances, and home or office furnishings. The results in Table 2 Panel B suggest that the total card consumption response is concentrated in the non-durable component, whereas durable spending does not experience a significant decline after the housing policy shock. This is consistent with our finding of no visible change in their spending behavior using cash and checks. The result on the (lack of) spending response in durable items

[20] Using the number of bank debit transactions, we find no evidence that treated individuals increase their number of bank debit transactions. In addition, we assume that individuals deposit their monthly income into and pay their credit card balance from their accounts at this particular bank, and then we estimate (a noisy measure of) monthly cash/check spending as follows: bank balance at the start of the month+ income – total card spending – bank balance at the end of the month. We find no difference in the change in cash/check spending between the treatment group and the control group.

[21] We confirm this using our credit and debit transaction level data -- looking through the transaction category codes, merchant names, and transaction types -- and do not find a *single* transaction for mortgage, rent, and auto loan payments in over 18 million debit card and credit card transaction.

implies that consumers prolong the usage of their existing durable goods in response to the policy shock. Collectively, the evidence suggests that the consumption primarily responds more, upon the policy shock that reduces the access to home equity, in non-durable or discretionary spending items.

5.3. Is Consumption Responding to the Change in the Level of Housing Wealth?

The housing policy announced on August 30, 2010 directly affected homeowners' access to home equity. An alternative channel potentially at work is the housing wealth effect that may result (indirectly) from the policy's (negative) effect on house price growth. However, a priori, it is not clear that the policy should have a negative effect on homeowners' housing wealth. Although the policy will reduce the supply of homes on the market by extending the minimum holding period of home owners, the same rule will decrease the housing demand at the same time as well especially for repeat home buyers (who are restricted to the extended minimum occupation period). Indeed we see no price decline after the policy announcement—the overall price has an upward trend (Figure 1)—suggesting that tightening of home equity access is likely singled out in our setting.

We perform additional analysis to further address the possibility that the policy might have influenced the potential housing demand/supply and changed the (expected) price appreciation rate. Our year-month fixed effects can partly capture this effect by controlling for the aggregate price trend across the entire market. However, we recognize that there is significant heterogeneity in house price growth, especially across different regions (see Appendix A). It may be that the consumption response primarily stems from consumers who live in regions that experienced higher house price appreciation in the past and that are therefore expected to grow at a much slower rate after the policy shock. To investigate this possibility, we repeat the specification in Panel C of Table 2 by replacing time fixed effects with region-time fixed effects. We allow each region (i.e., postal sector in Singapore) to have a different time trend, and then test the consumption response of the treatment group relative to that of the control group within the same region, where house price dynamics are more homogeneous.[22] If

[22] There are 81 postal sectors (out of which 52 contain residential properties) in Singapore, a country with a total area size of 701 km^2 (as a reference, New York City has an area of 1,213 km^2). Therefore, house price level and growth likely are homogeneous at this disaggregated level.

consumption indeed responds to the indirect effect on house price growth, we would expect the coefficient estimates to differ from those in Panel A of Table 2 and have a smaller magnitude.

Results in Panel C of Table 2 indicate that the consumption response we observe in Panel A of Table 2 is unlikely due to a change in the level of housing wealth. The estimated coefficients for the key variables, as well as the R-squares, hardly differ. Given that postal sectors define locations at a reasonably fine level (see Panel A of Appendix A) in Singapore, the minuscule (virtually zero) incremental explanatory power of including region-time fixed effects suggests that the consumption response is unlikely due to the (perceived) change in the level (or growth) of housing wealth.

5.4. The Dynamics of the Spending Response

In this section, we study the dynamic pattern of the consumption response during the 6-month period following the housing shock to gauge the expansionary impact of the policy change (Equation (2)). As a validation exercise, we include the pre-policy month dummies as well. Figure 3 graphs the entire paths of cumulative coefficients b_s, $s >= -4$, along with their corresponding 95 percent confidence intervals. The results can be interpreted as an event study.

[Insert Figure 3 About Here]

First, we confirm the average treatment effect in Table 2 and highlight that the cumulative spending response is economically significant. Compared to the control group, the treatment group experiences a cumulative decrease of 31% in their total card spending (from event month -4 to event month 6, relative to the calendar month 2010:04 (i.e., the fifth month before the policy announcement). The 95% confidence intervals suggest that the spending decrease is statistically significant by the 6th month upon the policy announcement. It is worth noting that the spending response is concentrated in the card spending, as we identify no response via other payment instruments such as cash and checks. As cash and checks constitute about 70% of individuals' consumption methods, the documented cumulative decrease in total card spending is equivalent to about 10% decrease of total consumption in the six-month period after the policy shock. Also, given an average monthly income of S$4,549 and an average monthly card spending of S$743 for the treatment group in the month of 2010:04 (baseline month),

it implies that the total dollar value of the card spending decrease amounts to S$231 during the six month period after the policy implementation, which is equivalent to 5.1% of the treated consumer's average monthly income. While economic significant, this estimate is likely a noisy assessment of the true policy impact on consumption, not only because our estimated consumption response is plausibly the lower bound of the true impact due to our fuzzy identification design, but also because of the estimation imprecision as implied by the fairly wide confidence interval.

In addition, the drop in total card spending is greater on credit cards: the treatment group experiences a cumulative credit card spending decrease of 58% during the same period. On the other hand, there is little spending response in the debit cards: the cumulative coefficients are positive and statistically insignificant throughout the post-policy period.

Second, there is no reversal of the trend in the card spending response during our sample period. This suggests that the consumption response to the housing policy shock is permanent. We also estimated the longer run consumption response by including periods beyond the six-month post-policy window. Our results remain qualitatively the same: there is a strong negative consumption response using a longer sample and it is concentrated in credit card spending. The main reason we choose to focus on results with the six-month post-policy period is that there were other policy changes (e.g., government stimulus programs) in February 2011, which result in positive income shocks to the population and could confound the interpretation of our analysis.

Lastly, in all three panels of Figure 3, we show no divergence in spending in the four months before the policy announcement: the cumulative spending difference between the treatment and control group in that period is both statistically and economically insignificant. In other words, the cumulative spending decrease from event month -4 to event month 6 (-31%, shown in Panel A of Figure 3) is driven by the spending decrease *after* policy announcement and implementation. Combined with results in Table 2, this pattern lends further support to our difference-in-differences research design.

5.5. Mechanism: the role of home equity as self-insurance

In this section, we further study the mechanism through which the shock affects consumption. Home equity serves as a self-insurance device that helps individuals smooth consumption inter-temporally (e.g., Lustig and Van Nieuwerburgh, 2005, 2010). Since HDB owners in Singapore cannot cash-out refinance, consumption smoothing against their illiquid home equity has to rely on other sources of credit, and credit card would be one primary source for individuals, especially constrained individuals to achieve consumption smoothing. Put differently, credit card facilitate consumption smoothing by allowing individuals to "borrow" from future wealth (resulting from house sale proceeds) for current consumption. Consistent with this argument, Agarwal and Qian (2014) find that individuals use credit card to increase spending and smooth their consumption after the announcement of a cash windfall from the government but before they receive the cash.

By restricting the home owners' option to access home equity, the policy not only reduces the level of consumption smoothing (reflected in the decreased consumption level), but it also changes the economics of consumption smoothing via different instruments. Since home owners will not be able to convert illiquid housing wealth into liquid wealth in the near future after the policy shock, the cost of consumption smoothing using credit cards significantly increases due to a greater likelihood of accumulating a large amount of (costly) credit card debt for an extended period of time. This in turn makes debit card a relatively more appealing instrument for smoothing consumption. Consequently, treated individuals not only reduce their overall consumption level in response to the illiquidity shock, but they will also decrease spending first and primarily on credit cards (to minimize the cost of running into costly credit card debt), leading to a relatively higher share of their post-policy spending on debit cards. In this regard, the comparison of credit card versus debit card spending response in Table 2 is consistent with the channel.

5.5.1. The role of credit market access

Furthermore, the extent to which individuals are affected by the shock depends on the completeness of credit or insurance markets. Presumably individuals with ready access to the credit or insurance market are better able to smooth their consumption, and these individuals should be less affected by the policy change as their consumption relies less on their home equity. Specifically, individuals' differential access to the credit card market captures heterogeneity in

the degree of completeness of credit market faced by consumers. Therefore, we use the available credit limit (i.e., the contractual credit limit minus the outstanding credit card debt) during the pre-treatment period to identify credit market access. Individuals with a low level of available credit limit likely are more restricted in using the credit card to smooth their consumption.[23] Results are reported in Table 3.[24]

[Insert Table 3 About Here]

In Table 3, the coefficient estimate on *Married* × 1_{post} captures the consumption response, relative to the control group, of the omitted subgroup (i.e., high available credit limit, or relatively unconstrained consumers) in the treatment group. The estimation coefficient on the triple interaction variables, *Married* × *Low available credit limit* × 1_{post}, measures the incremental consumption response of the more credit-constrained consumers in the treatment group, relative to the response of high available credit limit and less constrained consumers in the treatment group.

Column 1 of Table 3 shows the total card spending response is driven by the low credit limit consumers within the treatment group. The high credit limit consumers in the treatment group experience little consumption change either economically or statistically, relative to the change in the control group. We find similar results for credit card spending (column 2, Table 3). Specifically, the high credit limit consumers in the treatment group reduce their credit card spending by 3.2% per month after the policy announcement, relative to the change in the control group. The effect is statistically insignificant. However, the low credit card limit consumers experience a much greater reduction in their credit card spending: they reduce their spending by an additional 33% than the high credit card limit consumers in the treatment group (and the difference between the high and low credit limit consumers in credit card spending response is statistically significant at 1%). This is consistent with the idea that the policy changes the relative

[23] We also use the contractual credit limit as a proxy of credit market access and results are qualitatively the same.

[24] In general, we use the following specification to study the heterogeneity in responses to the housing policy across different groups of individuals:

$$\log(Y_{i,t}) = \beta_{post} \times 1_{treatment} \times 1_{post} + \beta_{g1,post} \times 1_{g1,treatment} \times 1_{post} + \cdots + \beta_{g(N-1),post} \times 1_{g(N-1),treatment} \times 1_{post}\beta_{pre} \times 1_{treatment} \times 1_{pre} + \alpha_i + \gamma_t + \epsilon_{i,t,}, \quad (3)$$

where N is the number of subgroups of consumers that we decompose into ($g1$ standards for the 1st group,…, $g(N-1)$ standards for the (N-1)th group, and the Nth group is the absorbed group and thus un-shown in Equation (3)).

cost of consumption smoothing between credit cards and debit cards, especially for the credit constrained consumers who face a higher risk of running into costly credit card debt.

Lastly, the positive coefficient of debit card spending for the low credit limit consumers in column 3 of Table 3 is also consistent with their response to the higher cost of consumption smoothing using credit cards. Given a large reduction in their credit card spending, and under the plausible assumption that these individuals still engage in some consumption smoothing to maintain a certain level of consumption, they will increase their debit card spending. Note that the total card consumption still experiences a decrease of more than 10% for the credit constrained individuals, which suggests that the increase in debit card spending indeed reflects their response to re-adjust their spending share among different instruments.

5.5.2. The role of precautionary saving motive

Reduced access to home equity negatively affects its role as a self-insuring mechanism, increases uncertainty over future consumption, and thus will have a greater impact on individuals with a stronger precautionary saving motive. We use the amount of the bank account balance during the pre-policy period to identify the existence of precautionary saving motive. An individual with a high level of pre-policy bank account balance (i.e., in the top tercile of the cross-sectional distribution) is more likely to have high precautionary saving motive. The additional results in Table 4 Panel A are consistent with the hypothesis. Consumers in the treatment group that have a high bank account balance have a greater reduction in their card spending during the post-policy months. Specifically, these individuals on average reduce their credit card spending by 7.9% more per month than consumers in the treatment group with a low bank account balance (statistically significant at 1%).

[Insert Table 4 About Here]

We also test the idea that consumers with a higher income risk face more uncertainty regarding their future income and thus will have a stronger precautionary saving motive as well. To identify consumers with a higher income risk, we use educational level as the proxy—individuals with a lower educational level likely face more uncertainty in their future income trajectory. We create a dummy, *No college degree,* which takes a value of one if the consumer's highest educational achievement is lower than college degree. To isolate the income

level effect that is correlated with educational level, we further focus on the treated individuals with no college degree *and* with a high current income level (i.e., their pre-policy income is in the top tercile of the sample distribution).

Results in Table 4 Panel B show that treated individuals who have no college degree *and* have a high level of current income experience a stronger decrease in consumption; their total card spending decreases by an additional 4.7% per month after the policy implementation (statistically significant at the 1% level), which is concentrated in the credit card spending (by an additional of 15.8% per month and statistically significant at the 1% level). This is consistent with the interpretation that individuals with high income risk tend to have stronger precautionary savings motive and thus respond more strongly to the housing policy shock.

In addition, we study how the role of credit constraints and precautionary saving motive interact. For example, individuals with a high level of accumulated savings (i.e., as measured by the bank's checking account balance) may at the same time have low available credit, and thus previous results cannot isolate the credit or precautionary saving effect. To this end, we create a new measure of liquid funds by combining the available credit limit and the checking account balance. A low level of total liquid funds thus gives a more accurate measure of the liquidity constraints, which allows us to identify the net effect of credit (liquidity) constraints and precautionary saving motive.

Then we interact the treatment dummy ($Married \times 1_{post}$) with a dummy that takes one if the individual's liquid funds during the pre-policy months (2010:05-2010:08) is in the bottom tercile of the sample distribution. Results in Table 4 Panel C show that the consumption response is stronger among treated individuals who have a lower level of total liquid funds. On the other hand, we find that treated individuals with a high level of total liquid funds also experience a decrease in their spending, although the effect is smaller (and statistically weaker) compared to those with a low level of total liquid funds. These findings suggest that both mechanisms are at work, and the effect of credit (or liquidity) constraints is stronger in explaining the consumption response.

5.5.3. Home equity channel vs. consumption commitment channel

In addition to the policy's role in restricting home owners' capacity to use their home equity to smooth consumption, another plausible mechanism is through an increase in risk aversion due to heightened commitment in their flats (Chetty and Szeidl, 2007). The extension of Minimum Occupation Period makes it more difficult for owners to get rid of the "commitment" associated with the loan taken to finance the home purchase. To further differentiate the mechanisms, we perform another heterogeneity analysis. If the first mechanism (based on the policy's impact on home equity extraction) is dominant, we should observe a stronger response among people with a higher level of home equity, whereas the commitment channel would predict a greater effect among people with a higher level of debt-to-value (and hence lower home equity). Although we do not directly observe the amount of home equity, we exploit the observation that there has been a fast price appreciation in the HDB market during the years before the policy: *ex ante* home equity is influenced by historic price fluctuations, and people living in areas where house prices grew rapidly in the years leading up to the policy likely have more home equity. We compute the average house price growth rates at the postal sector level based on the transaction prices in the HDB resale market during 2007-2009 (please see Appendix A for the distribution of the postal sectors and the price growth rates). These rates would capture, on average, the increase in home equity (due to capital appreciation) for those who have owned HDB resale flats in a given postal sector for three years before 2010. Results in Table 5 indicate that the treated consumers living in higher price growth areas experience a stronger consumption reduction, primarily through credit card spending. Their credit card spending on average decrease by 5.4% more per month than those treated consumers living in lower price growth areas during the six-month post-policy period. The effect is statistically significant at the 5% level. The results in Table 5 thus support the interpretation that the policy restricted owners' capacity to extract home equity to smooth consumption.

[Insert Table 5 About Here]

5.6. An Alternative Identification of the Treatment and Control Groups

We conduct a stronger test based on marital status to identify treatment and control groups. As mentioned earlier, single Singaporeans under age 35 are prohibited from owning public housing. They should be unaffected by the policy and hence constitute a natural control

group for the diff-in-diff analysis.[25] To control for the age effect, we consider using married Singaporeans under age 35 as the treatment group. The reason we do not use this specification as our main test is the following. We have a much smaller and non-representative sample if we were to restrict it to individuals under age 35. As a result, we use this as an additional test to study the internal validity of our marital status–based identification strategy. Consistent with results in Panel A of Table 2, we observe a large and significant (6.3% per month) negative consumption response in total card spending among individuals under age 35 (Table 6). The consumption response results are stronger in credit card spending, as the average monthly decrease in credit card spending is 13.6% (statistically significant at 1%).[26]

[Insert Table 6 About Here]

5.7. Matched Sample Analysis

One challenge with the current identification based on marital status lies in the fact that married and single consumers differ quite significantly along observable dimensions (Table 1 Panel A). While the pre-policy parallel trends in spending between the two groups mitigate the concern regarding our empirical strategy, we conduct one more robustness test to control for the difference in observables between the two groups.

Specifically, we perform a nearest neighbor propensity score match on the married and single individuals in the main test sample, using observables including pre-policy income, pre-policy checking account balance, female dummy, and Chinese ethnicity dummy.[27] In Table 7 Panel A, we report the summary statistics of the matched sample of married and single consumers in the public housing market. Compared to the full sample (Table 1 Panel A), the

[25] To increase the sample size, we also include foreign singles under the age of 35 in the control group for this test because they do not qualify for owning public housing either, but our results also hold if we restrict our control sample to Singaporean singles under age 35.

[26] Although the debit card spending response is positive in this specification, it's small relative to the negative spending response on credit cards and thus we observe a strong negative response in total card spending (which is the sum of credit card spending and debit card spending). One way to interpret the positive debit card spending coefficient is that married consumers in this age group have different preferences in the spending instrument compared to single consumers, since married consumers have higher debit card spending in the pre-policy period as well (i.e., Married × 1_{pre} coefficient).

[27] We did not explicitly match based on age. This is because on average married consumer are much older (as can be seen from Table 1) and the resulting match will include disproportionately older single individuals in the matched sample, leading to the concern of oversampling the owners in the singles' population (by law, singles older than 35 can own public housing). Instead we restrict our sample to individuals younger than 40 to ensure a comparable age distribution between the treatment and control group.

difference in observables between the treatment and the control group diminishes significantly. In particular, the matched treatment and the matched control group exhibit no significant difference, statistically and economically, in their income, bank checking account balance, and the proportion of Chinese ethnicity. Although we still observe a statistically significant difference in their age (by 2.7 years) and female proportion (by 2%), those differences are economically small. Overall, the matched sample is balanced along key socioeconomic dimensions.

[Insert Table 7 About Here]

Then we repeat the same specification as in Table 2 Panel A on the matched sample, and results are reported in Table 7 Panel B. Results resemble those in the main analysis: the matched treatment group experiences a decrease in total card spending, and the consumption response is concentrated in credit card spending. The effects are both statistically and economic significant. This result, albeit on a much smaller subsample of consumers, provides further evidence on the internal validity of our main result. Specifically, they suggest that the observed consumption decrease among married individuals in the public housing market is unlikely driven by confounding factors associated with marital status.

5.8. Falsification Tests

We implement two falsification tests to investigate alternative explanations for our main results. We first compare the consumption response between married and single consumers residing in the private housing market. Our data contain a variable that identifies the property type, based on which we can determine consumers *residing* in the private housing market. These Singaporeans are not affected by the minimum occupation requirement policy in the public housing market in their capacity to access home equity.[28] Therefore, if the consumption response identified in Table 2 is truly due to the public housing policy, we would expect that any change in spending behavior after August 2010 among married consumers living in private

[28] Although the government also announced, at the same time in August 2010, a cooling measure in the private housing market that imposes a 3% transaction tax for those selling within one year of purchase, it was not a binding constraint on home equity access, since cash-out refinancing is a feasible option against private properties. In other words, the private housing policy does not change the home equity access option for consumers residing in the private housing market, regardless of whether they are owners and renters.

housing would be no different from that of single consumers living in private housing. However, if the different spending response among married consumers compared to single consumers is due to other confounding factors associated with marital status that make married individuals spend less over time, or other events in August 2010 that affected *only* married individuals, we expect to find a similar differential consumption response between married and single consumers in the private market.

Results in column 1 of Table 8 show that married consumers living in the private housing market do not reduce their total card consumption during the six-month period after the policy announcement, relative to single consumers living in the private housing market. The difference-in-difference estimate coefficient (*Married* × 1_{post}) is positive and statistically insignificant. This provides evidence that our main results are unlikely driven by other unobservable factors associated with marital status.

[Insert Table 8 About Here]

Second, we perform another falsification test using the foreigner population living in the public housing market. By law, foreigners (who are not permanent residents of Singapore) cannot own public housing and thus should not experience any spending change after the policy announcement regardless of their marital status. We repeat the analysis in Table 2 Panel A by comparing the married and single consumers in the foreigner population who live in the public housing market. Since permanent resident owners in the public housing market are still subject to the minimum occupation period rule, we remove Chinese and Indian nationals from the foreigner population, as these two nationalities comprise more than 80% of the permanent resident population in Singapore in 2010 (Department of Statistics, 2011).[29] Our data do not indicate the permanent resident status of the foreigner population, but the aggregate statistics suggest that non-Chinese or non-Indian foreigners are a good proxy for the unaffected foreigners. Consistently, we find no evidence of total card spending change among the married foreigners relative to that of the single consumers (column 2, Table 8). Even though the coefficient estimate for the treatment group post-policy dummy is negative (-0.015), it is both statistically and economically small.

[29] Please refer to Table 1 in the Census of Population 2010 for the detailed statistics (http://www.singstat.gov.sg/publications/publications_and_papers/cop2010/census_2010_release1/cop2010sr1.pdf).

The two falsification tests are based on much smaller samples. Therefore, we conduct one last robustness analysis to dispel the concern that the lack of significant negative spending response in the falsification tests is simply a power issue. Specifically, we generate a random subsample, with a comparable size as in the falsification tests, of that used in the main analysis (i.e., Singaporeans who live in the public housing market and are older than 25) and repeat the analysis in Table 2 Panel A. We repeat the exercise 100 times and report the average coefficient in column 3 of Table 8. The result suggests that married Singaporeans experience a consistently large negative total card spending response (-4.9% per month) in the randomly selected smaller subsamples. 49% of the 100 regression coefficients are statistically significant at least at the 10% level. This result contrasts with those in the two falsification tests especially in economic magnitude, suggesting that the lack of effect in the falsification tests are unlikely due to sample size issue.

6. Conclusion

In this paper, we exploit a unique housing policy experiment in Singapore that reduced homeowners' access to their home equity. Specifically, in August 2010, the Housing and Development Board (HDB), in an effort to cool down the heated housing market and reduce speculative activities, extended the Minimum Occupation Period for resale HDB flats in the public housing market from 3 years to 5 years. We find no decrease in housing prices in our sample; rather, the negative shock is on consumers' ability to access their home equity. This allows us to test whether consumption drops after a negative shock on the option to cash out, even if house prices do not fall.

We use a unique panel dataset of consumer financial transactions to identify the consumption response. Using difference-in-differences analyses, we find a significant negative consumption response to the policy shock. More interestingly, the consumption response is concentrated in credit card spending, and there is no consumption response in debit card usage or cash/check spending. Individuals with more limited access to the credit market, or individuals with stronger precautionary saving motive respond more strongly to the policy shock. These results suggest that a decreased access to home equity weakens the role of home equity as a self-insurance mechanism for individuals to smooth consumption, and thereby causes a negative

consumption response especially among people facing less complete credit market or those who are more concerned about future consumption uncertainty.

Our results provide new insights about consumer responses to negative shocks associated with access to home equity that are especially relevant after the Great Recession in the United States. Restricting home equity access *alone* can affect consumption growth. Policy makers around the world can use such cooling measures to slow down consumption, allowing them to control inflation and economic growth.

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Figure 1. Public Housing in Singapore: Price Trend

This figure shows the price index series of the resale prices in the public housing sector in Singapore between 2006 and 2012. We obtain the price index data from the website of Housing Development Board in Singapore. http://www.hdb.gov.sg/fi10/fi10321p.nsf/w/BuyResaleFlatResaleIndex?OpenDocument#Weights

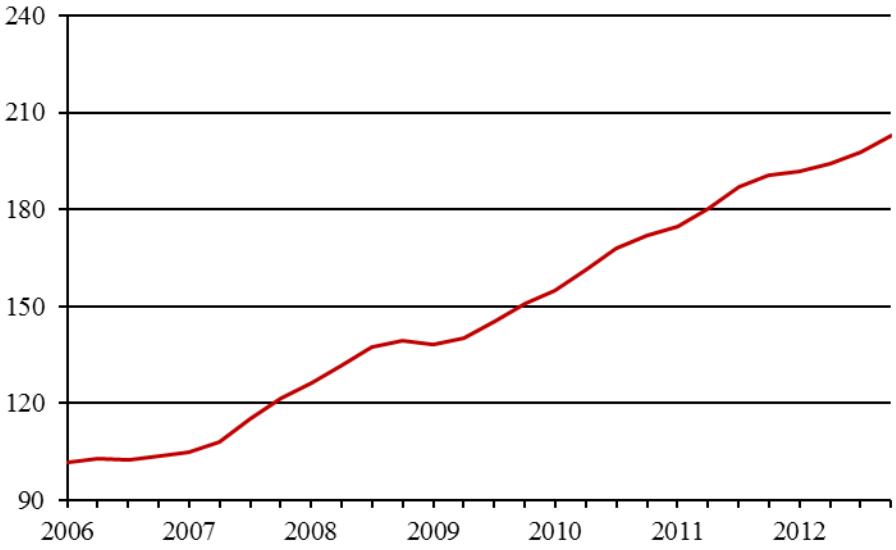

Figure 2. Unconditional Mean of Monthly Spending

This figure shows the mean spending for the *Married (treatment)* and *Single (control)* groups during our sample period of 2010:04–2011:02. We use marital status to empirically identify the level of home equity of consumers. For ease of comparison, we plot using the log of the average of monthly total card spending, monthly credit card spending, and monthly debit card spending, respectively. The x-axis denotes calendar year-month, and y-axis denotes the mean spending amount (in log). The vertical bar indicates the first month the policy becomes effective (i.e., 2010:09).

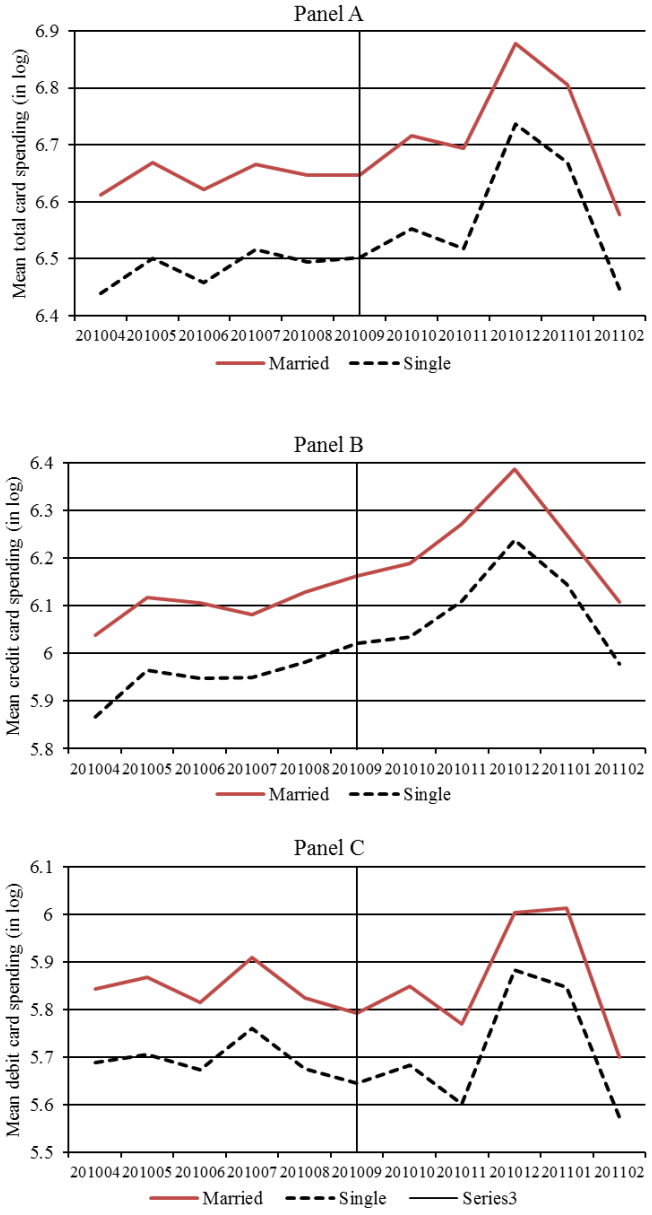

Figure 3. Estimated Spending Response Dynamics

This figure plots the entire paths of cumulative coefficients b_s, s = -4, -3, -2, …, 4, 5, along with their corresponding 95 percent confidence intervals, of total card spending (Panel A), credit card spending (Panel B) and debit card spending (Panel C) response as estimated from Equation (2). The x-axis denotes the sth month relative to the policy implementation month, and the y-axis shows the cumulative percentage change in spending among *married* Singaporeans after the public housing policy announcement (relative to the change in spending among *single* Singaporeans). The vertical bar indicates the first month the policy becomes effective (i.e., 2010:09).

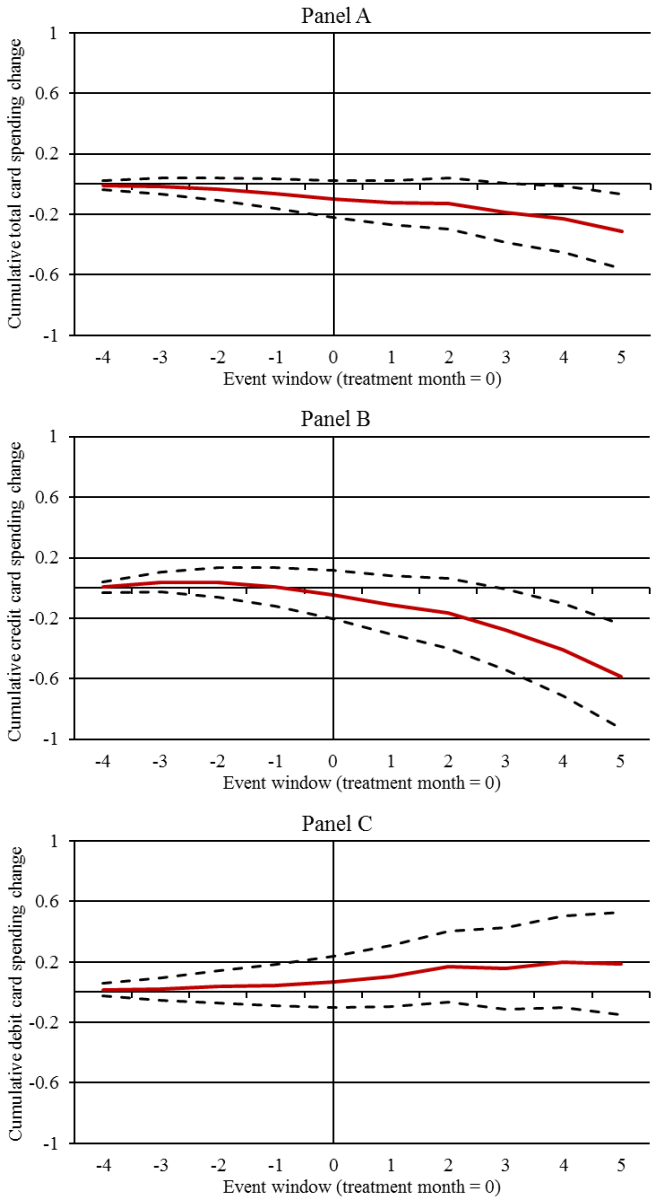

Table 1. Summary Statistics

This table reports summary statistics of our treatment and control groups during the period of 2010:04–2011:02. We use marital status to empirically identify the level of home equity. We include only those consumers who have a bank account, debit card, and credit card account with the bank at the same time. In addition, we include only individuals who are Singaporean (older than 25) and live in public housing. We exclude individuals/accounts that are inactive (i.e., accounts with zero monthly spending for at least half of the sample period (e.g., for 12 months if the account enters our sample in 2010:04). *Monthly total spending* (*Monthly credit card spending*, *Monthly debit card spending*) is the amount of the total spending (credit card spending, debit card spending) in Singapore by a typical individual in a typical month. *Income* is the average monthly salary between 2010:04 and 2010:08 in the local currency (S$). *Checking account balance* is the average monthly bank checking account balance between 2010:04 and 2010:08 in the local currency (S$). *Average house price* is the average quarterly public housing price (at the postal sector level) between 2000 and 2012 in the local currency (S$), obtained from the Singapore Housing and Development Board (www.hdb.gov.sg). *Age* is the age reported by the individual. *Female* is a dummy variable that takes a value of 1 if the individual is a female. *Married* is a dummy variable that takes a value of 1 if the individual is married. *Ethnicity: Chinese* is a dummy variable that takes a value of 1 if the individual's ethnic group is Chinese. All the dollar amounts are in the local currency (S$), and the average exchange rate during our sample period is 1 S$ = 0.75 US dollar (source: Monetary Authority of Singapore).

	Single (Control)		**Married (Treatment)**		**Diff. (Treatment – Control)**
Variable	**Mean**	**Std. dev.**	**Mean**	**Std. dev.**	
Monthly total spending	704	597	827	689	123***
Monthly credit card spending	426	531	499	592	73***
Monthly debit card spending	305	341	354	392	49***
Income	3,812	2,561	4,496	3,043	684***
Checking account balance	17,126	35,633	24,436	44,029	7,311***
Average house price	285,677	41,236	291,222	43,029	5,545***
Age	38.01	9.70	43.23	9.10	5.22***
Female (%)	0.43	0.50	0.45	0.50	0.02***
Ethnicity: Chinese (%)	0.88	0.32	0.85	0.36	-0.03***
# people	31,498		25,243		

Table 2. The Average Spending Response to Housing Policy

This table shows the average spending response of Singaporean living in public housing to the housing policy announced (and implemented) on August 30, 2010. The sample includes all Singaporeans (>25) living in public housing and the period is from 2010:04 to 2011:02. 1_{post} is a dummy that equals one for the months after the announcement of the public housing policy (i.e., 2010:09–2011:02). 1_{pre} is a dummy that equals one for the four months prior to the announcement of the public housing policy (i.e., 2010:05–2010:08). Panel A shows the average consumption response for the entire sample, with individual and year-month fixed effects included. In Panel B, we decompose the total card spending into durable and non-durable category and study the consumption response by category, with individual and year-month fixed effects included. To investigate whether the consumption response is due to the change in house price growth (and housing wealth) in public housing, we include region-time fixed effects to control for region-specific house price dynamics in Panel C. Please refer to Table 1 for definitions of the other variables. Standard errors are clustered at the individual level. T-statistics are reported in parentheses under the coefficient estimates, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A	(1) **Log(monthly total spending)**	(2) **Log(monthly credit card spending)**	(3) **Log(monthly debit card spending)**
Married × 1_{post}	-0.042***	-0.099***	0.024
	(-3.06)	(-4.78)	(1.27)
Married × 1_{pre}	-0.016	0.002	0.011
	(-1.24)	(0.11)	(0.65)
Constant	5.716***	3.962***	4.301***
	(1,000.13)	(498.36)	(556.00)
Observations	532,520	532,520	532,520
R-squared	0.550	0.656	0.552

Panel B	(1)	(2)
	Log(monthly total spending)	
	Durables	**Non-Durables**
Married × 1post	-0.023	-0.046***
	(-1.03)	(-2.97)
Married × 1pre	-0.018	-0.016
	(-0.89)	(-1.16)
Constant	2.201***	5.281***
	(240.63)	(823.20)
Observations	532,520	532,520
R-squared	0.475	0.565

Panel C	(1) **Log(monthly total spending)**	(2) **Log(monthly credit card spending)**	(3) **Log(monthly debit card spending)**
Married × 1_{post}	-0.039***	-0.097***	0.024
	(-2.80)	(-4.67)	(1.29)
Married × 1_{pre}	-0.012	0.004	0.012
	(-0.96)	(0.26)	(0.67)
Constant	5.665***	3.648***	4.207***
	(66.84)	(26.04)	(32.78)
Observations	532,158	532,158	532,158
R-squared	0.551	0.656	0.552

Table 3. The Role of Credit Market Access

This table shows the spending response heterogeneity among high and low credit card limit individuals in the treatment group. The sample includes all Singaporeans (>25) living in public housing and the period is from 2010:04 to 2011:02. *Low available credit limit* is a dummy variable that takes a value of one if the consumer has a low available credit limit (i.e., when the difference between the granted credit limit and the average outstanding credit card debt between 2010:04 and 2010:08 is in the bottom tercile of the sample distribution), and zero otherwise. Please refer to Tables 1 and 2 for definitions of variables. Individual and year-month fixed effects are included, and standard errors are clustered at the individual level. T-statistics are reported in parentheses under the coefficient estimates, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

	(1) **Log(monthly total spending)**	(2) **Log(monthly credit card spending)**	(3) **Log(monthly debit card spending)**
Married × 1_{post}	-0.022	-0.032	0.015
	(-1.56)	(-1.52)	(0.78)
Married × Low available CC limit × 1_{post}	-0.100***	-0.331***	0.043**
	(-6.73)	(-12.46)	(2.05)
Married × 1_{pre}	-0.016	0.002	0.011
	(-1.24)	(0.12)	(0.64)
Constant	5.716***	3.962***	4.301***
	(1,000.34)	(498.76)	(555.99)
Observations	532,520	532,520	532,520
R-squared	0.550	0.656	0.552

Table 4. The Role of Precautionary Savings Motive

This table shows the spending response heterogeneity among high and low precautionary saving motive individuals in the treatment group. The sample includes all Singaporeans (>25) living in public housing and the period is from 2010:04 to 2011:02. In Panel A, we measure the extent of precautionary savings motive using the level of the bank's checking account balance. Specifically, *High checking balance* is a dummy variable that takes a value of one if the consumer's average checking account balance between 2010:04 and 2010:08 is in the top tercile of the sample distribution, and zero otherwise. In Panel B, we use educational level to proxy for income risk (and thus the extent of precautionary savings motive). In particular, *No college degree* is a dummy equal to one if the consumer's highest educational achievement is lower than the college degree, and zero otherwise. *High income* is a dummy equal to one if the consumer's average monthly income in the pre-policy months (2010:05-2010:08) is in the top tercile of the sample distribution, and zero otherwise. In Panel C, we construct a variable to measure total liquid funds by combining the available credit limit and checking account balance. *Low total liquid funds* is a dummy that takes a value of one if the average of a consumer's combined liquid funds between 2010:04 and 2010:08 is in the bottom tercile of the sample distribution, and zero otherwise. Please refer to Tables 1 and 2 for definitions of variables. Individual and year-month fixed effects are included, and standard errors are clustered at the individual level. T-statistics are reported in parentheses under the coefficient estimates, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

	(1) **Log(monthly total spending)**	(2) **Log(monthly credit card spending)**	(3) **Log(monthly debit card spending)**
Panel A:			
Married × 1$_{post}$	-0.036**	-0.074***	0.027
	(-2.47)	(-3.31)	(1.38)
Married × High checking balance × 1$_{post}$	-0.020	-0.079***	-0.010
	(-1.50)	(-3.57)	(-0.54)
Married × 1$_{pre}$	-0.016	0.002	0.011
	(-1.24)	(0.10)	(0.64)
Constant	5.716***	3.962***	4.301***
	(1,000.09)	(498.50)	(555.98)
Observations	532,520	532,520	532,520
R-squared	0.550	0.656	0.552
Panel B			
Married × 1$_{post}$	-0.028*	-0.051**	0.021
	(-1.93)	(-2.30)	(1.08)
Married × No college degree ×	-0.047***	-0.158***	0.009
High income × 1$_{post}$	(-3.81)	(-7.87)	(0.51)
Married × 1$_{pre}$	-0.016	0.002	0.011
	(-1.24)	(0.11)	(0.65)
Constant	5.716***	3.962***	4.301***
	(1,000.18)	(498.44)	(556.00)
Observations	532,520	532,520	532,520
R-squared	0.550	0.656	0.552

Panel C			
Married × 1_{post}	-0.024*	-0.037*	0.019
	(-1.72)	(-1.73)	(1.00)
Married × Low total liquid funds × 1_{post}	-0.098***	-0.342***	0.025
	(-6.46)	(-12.49)	(1.14)
Married × 1_{pre}	-0.015	0.002	0.011
	(-1.23)	(0.14)	(0.64)
Constant	5.716***	3.962***	4.301***
	(1,000.26)	(498.69)	(555.97)
Observations	532,520	532,520	532,520
R-squared	0.550	0.656	0.552

Table 5. Home Equity vs. Consumption Commitment

This table shows the spending response heterogeneity in the treatment group among postal sector areas that have experienced high and low HDB housing price growth during the 2007-2009 period. The sample includes all Singaporeans (>25) living in public housing and the period is from 2010:04 to 2011:02. *High price growth* is a dummy variable that takes a value of one if the treated consumers live in a postal sector that experienced an above median average growth rate in the HDB resale price during 2007-2009, and zero otherwise. There are 82 postal sectors in Singapore, but many are unbuilt areas. In our sample, we obtain the average transaction prices for 52 postal sectors in each year between 2006 and 2009, based on which we compute the average of the annual growth rate. Please see Appendix A for the geographical distribution of the postal sectors and the average price growth rate in the resale HDB market during 2007-2009. Please refer to Tables 1 and 2 for definitions of variables. Individual and year-month fixed effects are included, and standard errors are clustered at the individual level. T-statistics are reported in parentheses under the coefficient estimates, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

	(1) **Log(monthly total spending)**	(2) **Log(monthly credit card spending)**	(3) **Log(monthly debit card spending)**
Married × 1_{post}	-0.037**	-0.081***	0.021
	(-2.53)	(-3.71)	(1.10)
Married × High price growth × 1_{post}	-0.018	-0.054**	0.007
	(-1.31)	(-2.37)	(0.37)
Married × 1_{pre}	-0.016	0.002	0.011
	(-1.24)	(0.10)	(0.65)
Constant	5.716***	3.962***	4.301***
	(1,000.13)	(498.37)	(556.00)
Observations	532,520	532,520	532,520
R-squared	0.550	0.656	0.552

Table 6. The Average Spending Response to the Housing Policy: an Alternative Specification

This table reports the average spending response of Singaporeans living in public housing using an alternative specification. Specifically, we carry out the analysis in Table 2 Panel A by restricting to the subsample of individuals younger than 35. Singles under age 35, regardless of nationality, are not allowed to own public housing by law (except in very rare circumstances). We thus use singles under age 35 (Singaporean and foreigner) as our control group, as these individuals are unlikely to have any home equity. To control for the age effect, we use married Singaporeans under age 35 as the treatment group in this analysis. The sample period is from 2010:04 to 2011:02. Please refer to Tables 1 and 2 for in the main body of the paper for definitions of the other variables. Individual and year-month fixed effects are included, and standard errors are clustered at the individual level. T-statistics are reported in parentheses under the coefficient estimates, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively

	(1) **Log(monthly total spending)**	(2) **Log(monthly credit card spending)**	(3) **Log(monthly debit card spending)**
Married × 1_{post}	-0.063**	-0.136***	0.075**
	(-2.47)	(-3.32)	(2.20)
Married × 1_{pre}	-0.020	-0.005	0.051
	(-0.86)	(-0.17)	(1.64)
Constant	5.580***	3.481***	4.417***
	(583.02)	(260.77)	(360.21)
Observations	188,403	188,403	188,403
R-squared	0.547	0.680	0.531

Table 7. The Average Spending Response to the Housing Policy: Matched Sample Analysis

This table reports the average spending response of Singaporeans living in public housing using the matched sample approach. We perform a nearest-neighbor propensity score match for the married and single consumers in the main test sample, based on observables including pre-policy income, pre-policy checking account balance, female dummy, and Chinese ethnicity dummy. Panel A reports the summary statistics of the matched sample of married and single consumers in the public housing market. Then we repeat the same specification as in Table 2 Panel A on the matched sample and report the results in Panel B. Please refer to Tables 1 and 2 for in the main body of the paper for definitions of the other variables. Individual and year-month fixed effects are included, and standard errors are clustered at the individual level and reported in parentheses under the coefficient estimates, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A

	Matched Single (Control)		**Matched Married (Treatment)**		**Diff. (Treatment-Control)**
	Mean	**Std. dev.**	**Mean**	**Std. dev.**	
Monthly income	3,669	2,312	3,734	2,351	65
Checking account balance	12,781	27,847	13,328	26,440	547
Age	31.9	4.2	34.6	3.6	2.7***
Female (%)	49.5	50.0	47.5	49.9	-2.0**
Ethnicity: Chinese (%)	87.5	33.1	87.8	32.7	0.4
# people	4,929		4,929		

Panel B

	(1) **Log(monthly total spending)**	(2) **Log(monthly credit card spending)**	(3) **Log(monthly debit card spending)**
Married × 1_{post}	-0.081**	-0.115**	-0.008
	(-2.46)	(-2.31)	(-0.20)
Married × 1_{pre}	-0.041	-0.021	-0.032
	(-1.38)	(-0.57)	(-0.80)
Constant	5.714***	3.818***	4.433***
	(420.65)	(202.00)	(247.21)
Observations	93,626	93,626	93,626
R-squared	0.548	0.669	0.534

Table 8. Falsification Tests

This table reports results of several falsification tests. The dependent variable is the monthly total card spending (in log) in all three columns. In column (1), we replicate the analysis in Table 2 Panel A, during the same period (2010:04 - 2011:02), in the sample of Singaporeans (>25) who reside in the private housing. In column (2) we restrict the sample to foreigners (excluding Chinese and Indian nationals) who live in public housing. In column (3), we randomly select, with 100 iterations, a subsample of the main test sample (i.e., Singaporeans > 25 living in public housing) and report the average regression coefficients for those 100 regressions. The square bracket below the average coefficient shows the percentage of the 100 regressions on the random subsample that have a statistically significant coefficient (with p value < 0.10). The observation reported in column (3) is the average number of observations in the 100 regressions. Please refer to Tables 1 and 2 for definitions of the other variables. Individual and year-month fixed effects are included, and standard errors are clustered at the individual level. In column (1) and (2), t-statistics are reported in parentheses under the coefficient estimates, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)
	Log(monthly total card spending)		
	Private housing	**Foreigners in HDB**	**Randomized main test subsample**
Married× 1_{post}	0.019	-0.015	-0.049
	(0.65)	(-0.41)	[49%]
Married× 1_{pre}	-0.003	0.004	-0.021
	(-0.10)	(0.14)	[12%]
Constant	5.928***	5.438***	
	(509.80)	(365.11)	
Observations	146,422	97,786	106,524
R-squared	0.576	0.544	

Appendix A. The Average Transaction Price within the HDB Resale Market

Panel A: Distribution of Postal Sectors in Singapore

Panel B: Distribution of the Average Annual Price Growth within the HDB Resale Market by Postal Sector (2007–2009)