ACCESS TO HOME EQUITY AND CONSUMPTION: EVIDENCE FROM A POLICY EXPERIMENT

Sumit Agarwal and Wenlan Qian*

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 a 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.

I. Introduction

The majority of a household’s wealth is stored in either financial markets or their house (Case, Quigley, & Shiller, 2005, 2013). A large literature has looked at the impact of home owners’ housing wealth on individual consumption and savings (e.g., Campbell & Cocco, 2007; Gan, 2010; Scholnick, 2013).1 More recently, Keys et al. (2014) documented 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 for 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 the economic effect is small even among the subgroup of individuals more likely to have a strong response.

Moreover, we have limited understanding on whether and how consumption responds to a negative shock to home equity access, despite 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 twelve 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, the average household experienced decreasing access to (cash-out) refinancing against their home equity (Mian & Sufi, 2012). Mian, Rao, and Sufi (2013) use house price and consumption data by postal code 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 led to both house price declines and tightened credit access.

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 apartments in the public housing market was extended from three years to five years.2 According to the minimum occupation period requirement, a public housing owner must live in the apartment 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: this person’s house price had appreciated, on average, $811,400 (the exchange rate in June 2010 was 1 S$1 = U.S.$0.71) 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

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* Agarwal: Georgetown University; Qian: NUS Business School.

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

2 In contrast to other countries (e.g., the United States), the Singapore government typically announces and implements policy changes without a lengthy process of discussion (see also another unanticipated government program discussed in Agarwal & Qian, 2014). We also verified by searching newspaper articles using Factiva: while there were sixteen articles discussing the policy right after the announcement that the minimum occupation period for HDB resale flats was increased from three to five years (i.e., August 30–31, 2010), there was no single article in the local news media on the issue during the five months before the policy announcement that corresponds to the prepolicy window in our data (April 1, 2010–August 29, 2010).
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 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 country’s population. 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 perform a more powerful test based on monthly transaction-based spending by individuals from an administrative data set 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 on 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 (U.S.$2.1 trillion versus U.S.$2.3 trillion) (U.S. Census Bureau, 2012). Similar to the United States, debit and credit cards are important mediums of disposable consumption in Singapore: together they make up close to 30% 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% in Singapore in 2011. This is comparable to the United States, where half of consumers in the United States have a credit card, and total credit card debt was close to U.S.$1 trillion in 2012 with 40% of revolving debt (U.S. Census Bureau, 2012). Therefore, our data provide a more complete and accurate measurement of individual consumption at a high frequency.

In addition, our data allow us to study the role of credit in consumption response at the individual level. 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; the credit card is the dominant instrument. By restricting 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 the debit card (i.e., accumulated 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 home owners. 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 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 at 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 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 4.1% per month during the six-month postpolicy period, relative to the change for consumers in the control group. However, we find no spending response using cash or checks, a which are typically used for nondiscretionary and expensive consumption items. Moreover, the decrease in card consumption is driven by nondurable 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 27% in their total card spending during the six-month postpolicy period. This is equivalent to an 8% decrease in total consumption over that period (considering that card spending comprises 30% of the total consumption) and is also equivalent to 4.4% of the treated consumer’s average monthly income. In addition, we show that this result is unlikely to be driven by a decrease in the level of housing wealth that could potentially result from slower house price growth rates after the policy shock.

Home equity serves as a self-insuring mechanism that helps individuals smooth consumption intertemporally (Lustig & Van Nieuwerburgh, 2005, 2010). Since home equity becomes more illiquid after the policy shock, consumption smoothing through credit cards becomes more costly, making a 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 evi-

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4 Our data do not explicitly provide cash or 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).
dence that the consumption response to the policy shock is concentrated in credit card spending. There is no consumption response in debit card use.

We also further explore the heterogeneity in the consumption response. First, we expect the importance of home equity for consumption smoothing to depend 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, weaker access to 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 motive for precautionary saving. Using the level of the prepolicy 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. Finally, the consumption response is concentrated among the treated individuals living in areas that have experienced more price appreciation (and hence a greater increase in home equity) before the policy. This again supports the interpretation that housing policy affected consumption by restricting home owners’ capacity to extract home equity.

To further test the internal validity of our identification, we conduct a series of additional tests; all results remain qualitatively the same. Because the housing policy shock was unanticipated, the collective evidence in this paper suggests a causal impact of a decrease in home equity access on consumption.

A large portion of wealth, for households and in the aggregate, is in housing. Therefore, identifying the channels through which housing wealth affects consumption bears important implications. We note that housing plays an equally important role in shaping household lifetime wealth and the aggregate economy in Singapore as it does in the United States. The rate of home ownership of the resident population in Singapore was 87.2% in 2010 (Department of Statistics Singapore), compared to 66.9% in the United States in 2010 (U.S. Census Bureau, 2012). Housing value (relative to income) is also significant in Singapore. The average price for an apartment developed and governed by the Housing and Development Board (HDB), in our sample period is equivalent to seven times the median individual annual income in Singapore. In comparison, the median house price is over three times the median household annual income in the United States (Zillow.com; U.S. Census Bureau, 2012). Therefore, the findings in this paper have broad policy implications. Specifically, a negative shock to home equity access, independent of a shock to the housing wealth level, has a causal impact on a decrease in consumption, likely through the channel of weakening the role of home equity as the consumption smoothing device. The paper also contributes to the vast literature on the consumption response to various income shocks.

The remainder of the paper is organized as follows. Section II discusses the housing market in Singapore and the specific policy experiment that occurred in August 2010. Section III describes the data. Section IV explains the identification strategy and the econometric methodology. Section V presents the empirical results. Section VI concludes.

II. Public Housing in Singapore and the 2010 Policy Experiment

In Singapore, there are two main types of residential property: public housing and private housing. Public housing, or HDB apartments, is open to most Singaporean citizens and permanent residents (except singles below 35 years old), and is heavily subsidized by the Singapore government. 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 apartments, which are developed and closely governed by the HDB. These apartments are located in housing developments, which are self-contained satellite towns with schools, groceries, clinics, food courts, and sports and recreational facilities. Such developments are located throughout the residential areas in the country with convenient access to public transportation; there is no geographical concentration distinct from the private housing market. A large variety of apartment types and layouts cater to various housing budgets.

One avenue for owning an apartment in the public housing market is through purchase of a new apartment directly from the HDB. The current mode of sale, known as the build-to-order (BTO) program, was launched in 2001. Qualified Singaporeans apply; those who are successful must then wait for more than four years before the building is completed. Because of the price subsidies offered for the new apartments, the eligibility criteria are strict: only citizens with a family that have an income below the stipulated cap can buy a new HDB apartment. In addition, the supply of new apartments is small relative to the entire housing stock. For example, the total public housing stock in 2013 was about 1.044 million units; the number of BTO supplied


7 The government subsidizes new apartments in the public housing market by selling them at below-market prices (which are stipulated by the government) to qualified Singaporeans. The government does not intervene in the resale market for, whose transaction prices are determined by market supply and demand. The subsidization also works partly through loan financing. Although home buyers can always borrow from banks, HDB offers loans to HDB apartment buyers with attractive financing terms for qualified buyers (e.g., low-income buyers).
on average 26,000 units annually from 2010 to 2012. For these reasons, the main way to own an HDB apartment is through the resale market, especially for eligible buyers who wish to move in immediately or those who do not qualify for new HDB apartments. For example, buyers qualified for owning HDB but ineligible for buying new apartments—permanent residents, single citizens older than 35, and citizens with a high income—can own resale HDB apartments. The government does not intervene on house prices in the resale market; the existing owners are allowed to sell their apartments on the open market to any eligible buyer at a mutually agreed-on price. However, most owners can sell their apartments only if they have met the minimum occupation period (MOP) requirement, which requires HDB owners to live in their residence for a minimum period of time before they are allowed to resell them (whether they purchase new or resale apartments).9

In response to a rapidly rising housing price in the resale market (see figure IA.1 in the online appendix) and to reduce speculative activities, the HDB announced on August 30, 2010, that it would extend the MOP for resale HDB apartments from three years to five years, effective immediately.10 Importantly, HDB apartment owners cannot refinance using their home equity (by law), so selling is the only way to access their home equity. As a result, this new policy directly reduced HDB resale apartment owners’ only option to access their home equity, which had risen in value for most home owners. The resale market’s price index experienced a cumulative increase of 41% from July 2007 to June 2010,11 suggesting that HDB owners on average accumulated a considerable amount of home equity during the period. A four-room HDB apartment (the most popular type) on average costs S$278,000 in the third quarter of 2007.12 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 S$1 = U.S.$.71). The new occupation period policy not only directly affected recent owners (those who had purchased an apartment less than five years ago); it also changed the expected future liquidity (i.e., ease of access) of home equity for the other home owners. Note that our sample does not experience a concurrent decrease in the level of housing wealth (as can be seen in figure IA.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.13

Interestingly, the Singapore government implemented another policy in early 2011 by distributing cash to Singaporean citizens in the growth dividend program (Agarwal & Qian, 2014). The policy has an objective of sharing the nation’s economic growth in the recent past. While the housing policy of extending the MOP in 2010 appears contractionary in the sense of limiting consumers’ resources, we note that policymakers have a consistent objective of ensuring sustainable economic growth. For this paper, we focus on the period before the distribution of the growth dividends to isolate the treatment effect due to the MOP policy.

III. Data

We use a unique proprietary data set obtained from the leading bank in Singapore that has more than 4 million customers, or 80% of the entire population of Singapore.14 The data set contains consumer financial transactions between April 2010 and March 2012, including credit card and debit card transactions of more than 180,000 individuals; it 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 checking account balance and number of transactions per month.15 The data also contain a rich set of demographics about each individual: 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,16 nationality, ethnicity, and occupation.

13 In the online appendix, we discuss in greater detail the alternative channel of consumption response through the policy’s potential impact on the level of housing wealth.
14 As the largest bank in Singapore, it has more than twice the number of branches and over four times the number of ATMs than the other major banks in Singapore. 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 one for our sample consumers’ daily financial needs due to its greater convenience and comparable banking fees.

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, 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 U.S. bank and, moreover, are comparable to 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 annual prevailing interest rate of 24% regardless of the credit card limit.

15 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 ten apartment units.
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 microlevel credit card data (e.g., Gan, 2010), we have more information on the 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 given the bank’s market share. 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 important, a particular advantage of our data is that we can study the role of credit at the individual level in understanding the consumption response. The consumers in the study are not allowed to engage in cash-out refinancing, consumption smoothing against 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, allow us to better understand heterogeneity in consumers’ consumption response.

Similar to Agarwal and Qian (2014), who used the same data set 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 consumers who have a bank account, debit card, and credit card account with the bank. We exclude individuals/accounts that are inactive—accounts with no monthly spending for at least half of the sample period (e.g., for twelve months if the account enters our sample in April 2010). Out of 180,000 consumers in our sample, over 115,000 are active account holders with all three accounts with the bank.17

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 Singaporeans who are living in public housing; 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, we have complete records prior to the policy announcement for more than 56,000 (Singaporean) consumers. The sample period in study covers five months before and six months after the policy announcement, which coincided with the policy implementation (April 2010–February 2011). The main reason we do not extend the postpolicy window beyond the six-month period that other policy changes (e.g., stimulus programs) were made in early 2011 (Agarwal & Qian, 2014) that involve income pay-out to these individuals and potentially confound the analysis and interpretation.

IV. 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.18 Ideally, a perfect control group in this study would be renters who have no home equity and are not affected by the housing policy change. In such a case, the estimated change in consumption of the affected home owners, 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 home ownership information, we cannot precisely identify the treated home owners. 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. Because of the eligibility criteria for buying HDB properties, single individuals are much less likely to own public housing in Singapore. For example, during our time period, single consumers were prohibited from owning any type of public housing if they were under 35 years old.19

This proxy gives us a fuzzy identification: any impact of the housing policy on consumption we find is likely the

17 All our results continue to hold if we relax these specific data filtering requirements.

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

19 Single Singaporeans can purchase in the resale public housing market only if they are older than 35 unless they are orphans (see the following link for details: http://www.hdb.gov.sg/h10/h10321p.nsf/w/BuyResaleFlatSingleScheme/OpenDocument). Single Singaporeans under age 35 are prohibited from buying directly from the government (in the BTO market) 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.
Note that married consumers on average live in locations spending information for married and single Singaporeans. We test on the validity of these identification strategies. Finally, we perform a matched-sample analysis on a control group because these individuals are renters and do not allowed to own public housing by law (except in very rare cases). 35. Singles under age 35, regardless of nationality, are not restricted to a subsample of consumers who are under age 18. The consumption response of the treatment group is a downward-biased estimate of the true consumption response of the treatment group. 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 explicitly test for any difference in consumption between the two groups and during the pre-policy 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 pretreatment period (i.e., parallel trends in the pretreatment 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 because these individuals are renters and do not have home equity. This specification alleviates the measurement error. Finally, we perform a matched-sample analysis, as well as various diagnostic checks and falsification tests on the validity of these identification strategies.

Table 1 provides summary statistics of demographics and spending information for married and single Singaporeans. 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.

Although 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 incomes and checking account balances and are older. They spend S$123 more per month than single consumers. To the extent that the identifying assumption lies in the “parallel trends” assumption, the difference in the level of spending between the treatment group and the control group is of lesser concern, and we will test explicitly for the parallel trends between the two groups before the policy shock. 20

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:

\[
\log(Y_{i,t}) = \beta_{\text{post}} \times 1_{\text{treatment}} \times 1_{\text{post}} + \beta_{\text{pre}} \times 1_{\text{treatment}} \\
\times 1_{\text{pre}} + \alpha_i + \gamma_t + e_{i,t}. \tag{1}
\]

The dependent variable, \( \log(Y_{ij}) \), represents the natural logarithm of the dollar amount of spending for individual \( i \) at the end of month \( t \). \( 1_{\text{treatment}} \) is a dummy indicator for the treatment group (i.e., equal to 1 for married consumers as of the month before the policy announcement). \( 1_{\text{post}} \) is a dummy variable equal to 1 for the months after the announcement of the housing policy (after August 2010). \( 1_{\text{pre}} \) is a dummy variable equal to 1 for the four months before the housing policy announcement (May 2010–August 2010). \( \gamma_t \) is the year-month dummy, used to absorb (location fixed effects). This is because we are potentially including treated home owners in our control group (i.e., some single Singaporeans are home owners), and these miscategorized 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.

Table 1.—Summary Statistics

<table>
<thead>
<tr>
<th>Variable</th>
<th>Control</th>
<th>SD</th>
<th>Treatment</th>
<th>SD</th>
<th>Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td>Monthly total spending</td>
<td>704</td>
<td>597</td>
<td>827</td>
<td>689</td>
<td>123***</td>
</tr>
<tr>
<td>Monthly credit card spending</td>
<td>426</td>
<td>531</td>
<td>499</td>
<td>592</td>
<td>73***</td>
</tr>
<tr>
<td>Monthly debit card spending</td>
<td>305</td>
<td>341</td>
<td>354</td>
<td>392</td>
<td>49***</td>
</tr>
<tr>
<td>Income</td>
<td>3,812</td>
<td>2,561</td>
<td>4,496</td>
<td>3,043</td>
<td>684***</td>
</tr>
<tr>
<td>Checking account balance</td>
<td>285,677</td>
<td>41,236</td>
<td>291,222</td>
<td>43,029</td>
<td>5,545***</td>
</tr>
<tr>
<td>Average house price</td>
<td>4.01</td>
<td>9.70</td>
<td>4.32</td>
<td>9.10</td>
<td>0.22***</td>
</tr>
<tr>
<td>Age</td>
<td>0.43</td>
<td>0.50</td>
<td>0.45</td>
<td>0.50</td>
<td>0.02***</td>
</tr>
<tr>
<td>Ethnicity: Chinese (%)</td>
<td>0.88</td>
<td>0.32</td>
<td>0.85</td>
<td>0.36</td>
<td>–0.03***</td>
</tr>
<tr>
<td>Number of people</td>
<td>31,498</td>
<td></td>
<td>25,243</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

This table reports summary statistics of our treatment group (married) and the control group (single) from April 2010 to February 2011. “Monthly total (credit card/debit card) spending” is the amount of the total (credit/debit card spending) in Singapore by a typical individual in a typical month. “Income” is the average monthly salary between April and August 2010. “Checking account balance” is the average monthly bank checking account balance between April and August 2010. “Average house price” is the average quarterly public housing price (at the postal sector level) between 2000 and 2012, obtained from the Singapore Housing and Development Board (www.hdb.gov.sg). “Age” is the age reported by the individual in August 2010. All dollar amounts are in the local currency (S$), and the average exchange rate during our sample period is S$1-U.S.$0.75 (Monetary Authority of Singapore).

20 We also plot the unconditional spending dynamics for the treatment group and the control group during the period (see figure IA.2 in the online appendix), and 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.
the seasonal variation in consumption expenditures, as well as the average of all other concurrent aggregate factors; \( \alpha_i \) is the individual dummy variable included to absorb differences in consumption preferences at the individual level. \( \beta_{post} \) in equation (1) captures the average log change in monthly spending for a treated individual after the housing policy announcement (compared to April 2010; the first month in our sample period), relative to the change in spending of the control group. In other words, \( \beta_{post} \) is the estimated treatment effect. Similarly, \( \beta_{pre} \) measures the average log difference in monthly spending for a treated individual between the four pretreatment months (May–August 2010) and the first (baseline) month in our sample period (April 2010), relative to the difference in the spending of the control group. Thus \( \beta_{pre} \) should be 0 if our difference-in-differences research design is valid.

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

\[
\log(Y_{it}) = \sum_{s=-5}^{5} \beta_s \times 1_{treatment} \times 1_{months} + \alpha_i + \gamma_t + \epsilon_{it}.
\]

Following Gross and Souleles (2002), the results can be interpreted as an event study. The coefficient \( \beta_s \) measures the treatment group’s spending change (in percentage term) in month \( s \) after the policy announcement as a percentage from month \(-5\) (or April 2010, the absorbed month), relative to the change in spending of the control group. Therefore, the cumulative coefficient \( \beta = \sum_{s=-4}^{0} \beta_s \) 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 pretreatment months (\( s = -4, \ldots, -1 \)) to explicitly test for the existence of pretreatment trend differences (on a month-to-month basis) between the treatment group and the control group. Similar to the static specification in equation (1), we expect to see a 0 cumulative spending response for \( s < 0 \).

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

V. Results

A. 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 plus credit card spending) by the treatment group. Overall, individuals in the treatment group decrease their total card spending by 4.1% per month (compared to the fifth month before the policy announce-

\[ \text{Table 2: The Average Spending Response to Housing Policy} \]

<table>
<thead>
<tr>
<th>Panel A</th>
<th>(1) Log(monthly total spending)</th>
<th>(2) Log(monthly credit card spending)</th>
<th>(3) Log(monthly debit card spending)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Married ( \times 1_{post} )</td>
<td>(-0.042***)</td>
<td>(-0.099***)</td>
<td>0.024</td>
</tr>
<tr>
<td>(-3.06)</td>
<td>(-4.78)</td>
<td>(1.27)</td>
<td></td>
</tr>
<tr>
<td>Married ( \times 1_{pre} )</td>
<td>(-0.016)</td>
<td>0.002</td>
<td>0.011</td>
</tr>
<tr>
<td>(-1.24)</td>
<td>(0.11)</td>
<td>(0.65)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>3.962***</td>
<td>4.301***</td>
<td>556.00</td>
</tr>
<tr>
<td>Observations</td>
<td>532,520</td>
<td>532,520</td>
<td>532,520</td>
</tr>
<tr>
<td>( R^2 )</td>
<td>0.550</td>
<td>0.656</td>
<td>0.552</td>
</tr>
</tbody>
</table>

Panel B

<table>
<thead>
<tr>
<th></th>
<th>(1) Log(monthly total spending)</th>
<th>(2) Log(monthly credit card spending)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Married ( \times 1_{post} )</td>
<td>(-0.023)</td>
<td>(-0.046***)</td>
</tr>
<tr>
<td>(-1.03)</td>
<td>(-2.97)</td>
<td></td>
</tr>
<tr>
<td>Married ( \times 1_{pre} )</td>
<td>(-0.018)</td>
<td>(-0.016)</td>
</tr>
<tr>
<td>(-0.89)</td>
<td>(-1.16)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>2.201***</td>
<td>5.281***</td>
</tr>
<tr>
<td>Observations</td>
<td>532,520</td>
<td>532,520</td>
</tr>
<tr>
<td>( R^2 )</td>
<td>0.475</td>
<td>0.565</td>
</tr>
</tbody>
</table>

This table shows the average spending response of Singaporeans living in public housing to the housing policy announced (and implemented) on August 30, 2010. \( 1_{post} \) is a dummy variable that equals 1 for the months after the announcement of the public housing policy; September 2010–February 2011. \( 1_{pre} \) is a dummy variable that equals 1 for the four months prior to the announcement of the public housing policy: May–August 2010. Individual and year-month fixed effects included. See 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. Significant at **1%; **5%, and *10%.

21 In interpreting this and subsequent results, we compute the percentage change from the coefficient estimate \( X \) on the log spending differences by the formula \( \exp(X) - 1 \). For example, the coefficient of \(-0.042\) in column 1 of table 2, panel A, implies a percentage change of \( \exp(-0.042) - 1 = -4.1\% \).
null that $\beta_{pre}$ is equal to $\beta_{post}$). Collectively, this set of results validates our 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 using 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.\(^{22}\)

To better understand the noresponse using cash or checks (which overall accounts for 70% of the entire consumption), we note that consumption using those instruments is primarily nondiscretionary. 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 debit or credit cards).\(^{23}\) Therefore, the evidence suggests that consumption primarily responds, 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 data set.

To further study the nature of spending response, we also decompose the total card spending into durable and nondurable 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 nondurable 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 spending behavior using cash and checks. Collectively, the evidence suggests that consumption primarily responds in nondurable or discretionary spending items.

### B. Dynamics of the Spending Response

In this section, we study the dynamic pattern of the consumption response during the six-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 1 graphs the entire paths of cumulative coefficients $b_s, s = -4, -3, \ldots, 5$, along with their corresponding 95% 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 vertical bar indicates the month that the policy becomes effective (September 2010).

\(^{22}\) 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 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.

\(^{23}\) 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 transactions.
nomically significant. Compared to the control group, the treatment group experiences a cumulative decrease of 27% in their total card spending (from event month -4 to event month 6, relative to the calendar month April 2010, the fifth month before the policy announcement). The 95% confidence intervals suggest that the spending decrease is statistically significant by the sixth month upon the policy announcement. It is worth noting that the spending response is concentrated in card spending; we identify no response using cash and checks. Because as cash and checks constitute about 70% of individuals’ consumption methods, the documented cumulative decrease in total card spending is equivalent to about an 8% decrease of total consumption in the six-month period after the policy shock. Also, given an average monthly income of $4,549 and average monthly card spending of $743 for the treatment group in April 2010 (baseline month), it implies that the total dollar value of the card spending decrease amounts to $200 during the six months after the policy implementation, which is equivalent to 4.4% of the treated consumer’s average monthly income. While economically 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 44% during the same period. There is little spending response in the debit card: the cumulative coefficients are positive and statistically insignificant throughout the postpolicy 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 postpolicy 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 postpolicy period is that there were other policy changes (e.g., government stimulus programs) in February 2011 that result in positive income shocks to the population and could confound the interpretation of our analysis.

Finally in all panels of figure 1, 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 (by 27%, shown in panel A of figure 1) is driven by the spending decrease after the policy announcement and implementation. Combined with results in table 2, this pattern lends further support to our difference-in-differences research design.

C. Mechanism: The Role of Home Equity as Self-Insurance

In this section, we study the mechanism through which the policy shock affects consumption. Home equity serves as a self-insurance device that helps smooth consumption intertemporally (Lustig & Van Nieuwerburgh, 2005, 2010). Since HDB owners in Singapore cannot cash out their re-financing, consumption smoothing against their illiquid home equity has to rely on other sources of credit, and a credit card is one primary source for individuals, especially constrained individuals, to achieve consumption smoothing. Put differently, credit cards 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 cards 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); 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 the 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 postpolicy 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.

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 that consumers face. Therefore, we use the available credit limit (i.e., the contractual credit limit minus the outstanding credit card debt) during the pretreatment period to identify credit market access. Individuals with a low level of
Table 3.—Role of Credit Market Access

<table>
<thead>
<tr>
<th></th>
<th>(1) Log(monthly total spending)</th>
<th>(2) Log(monthly credit card spending)</th>
<th>(3) Log(monthly debit card spending)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Married ( \times 1_{post} )</td>
<td>-0.022</td>
<td>-0.032</td>
<td>0.015</td>
</tr>
<tr>
<td></td>
<td>(−1.56)</td>
<td>(−1.52)</td>
<td>(0.78)</td>
</tr>
<tr>
<td>Married ( \times \text{Low Available CC} )</td>
<td>-0.100***</td>
<td>-0.331***</td>
<td>0.043**</td>
</tr>
<tr>
<td>Limit ( \times 1_{post} )</td>
<td>(−6.73)</td>
<td>(−12.46)</td>
<td>(2.05)</td>
</tr>
<tr>
<td>Married ( \times 1_{pre} )</td>
<td>-0.016</td>
<td>0.002</td>
<td>0.011</td>
</tr>
<tr>
<td></td>
<td>(−1.24)</td>
<td>(0.12)</td>
<td>(0.64)</td>
</tr>
<tr>
<td>Observations</td>
<td>532,520</td>
<td>532,520</td>
<td>532,520</td>
</tr>
<tr>
<td>( R^2 )</td>
<td>0.550</td>
<td>0.656</td>
<td>0.552</td>
</tr>
</tbody>
</table>

This table shows the spending response heterogeneity among high- and low-credit-card-limit individuals in the treatment group. "Low available credit limit" is a dummy variable that takes a value of 1 if the percentage difference between the consumer’s granted credit limit and average outstanding credit card debt between April and August 2010 is in the bottom 30% of the sample distribution, and 0 otherwise. See 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. Constants are not reported. T-statistics are reported in parentheses under the coefficient estimates. Statistical significance at ***1%, **5%, and *10%.

available credit limit likely are more restricted in using the credit card to smooth their consumption. Results are reported in table 3.25

In table 3, the coefficient estimate on Married \( \times 1_{post} \) 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 \( \times \text{Low Available Credit Limit} \times 1_{post} \), 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 that 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 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 3.1% 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 28% more 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 cost of consumption smoothing between credit card and debit card, especially for credit-constrained consumers who face a higher risk of running into costly credit card debt.

Finally, 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 credit-constrained individuals, which suggests that the increase in debit card spending indeed reflects their response to readjust their spending share among different instruments.

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 has a greater impact on individuals with a stronger precautionary saving motive. We use the amount of the bank account balance during the prepolicy period to identify the existence of precautionary saving. An individual with a high prepolicy bank account balance (i.e., in the top 30% of the cross-sectional distribution) is more likely to engage in high precautionary saving. The additional results in table 4, panel A are consistent with the hypothesis. Consumers in the treatment group who have a high bank account balance have a greater reduction in their card spending during the postpolicy months. Specifically, these individuals on average reduce their credit card spending by 7.6% more per month than consumers in the treatment group with a low bank account balance (statistically significant at 1%).

We also test the idea that consumers with a higher income risk face more uncertainty regarding their future income and thus will have stronger precautionary saving as

24 We also use the contractual credit limit as a proxy of credit market access, and results are qualitatively the same.

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

\[
\log(Y_{it}) = \beta_0 \times 1_{treatment} \times 1_{post} + \beta_{g1,post} \times 1_{g1,treatment} \\
\times 1_{post} + \cdots + \beta_{g(N−1),post} \times 1_{g(N−1),treatment} \\
\times 1_{post} + \beta_{gN,pre} \times 1_{treatment} \times 1_{pre} + \gamma_i + \gamma_t + \epsilon_{it},
\]  

where \( N \) is the number of subgroups of consumers we decompose into: \( g1 \) standards for the 1st group, \ldots, \( g(N−1) \) standards for the \( (N−1) \)th group, and the \( N \)th group is the absorbed group and thus not shown in equation (3).
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 1 if the consumer’s average checking account balance between April and August 2010 is in the top 30% of the sample distribution and 0 otherwise. “No college degree” is a dummy equal to 1 if the consumer’s highest educational achievement is lower than a college degree and 0 otherwise. “High income” is a dummy equal to 1 if the consumer’s average monthly income in the prepolicy months (May–September 2010) is in the top 30% of the sample distribution and 0 otherwise. “Low total liquid funds” is a dummy that takes a value of 1 of the average of a consumer’s combined liquid funds between April and August 2010 is in the bottom 30% of the sample distribution and 0 otherwise. 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. Constants are not reported. T-statistics are reported in parentheses under the coefficient estimates. Statistical significance at ***, **, and *10%.

Results in table 4, panel B show that treated individuals who have no college degree and with a high current income level (their prepolicy income is in the top 30% of the sample distribution).

Panel A:

<table>
<thead>
<tr>
<th></th>
<th>(1) Log(monthly total spending)</th>
<th>(2) Log(monthly credit card spending)</th>
<th>(3) Log(monthly debit card spending)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>prepost</td>
<td>prepost</td>
<td>prepost</td>
</tr>
<tr>
<td>Married × High Checking Balance × 1post</td>
<td>−0.097***</td>
<td>−0.074***</td>
<td>0.027</td>
</tr>
<tr>
<td></td>
<td>(−3.31)</td>
<td>(−3.57)</td>
<td>(1.38)</td>
</tr>
<tr>
<td>Married × 1prev</td>
<td>0.016</td>
<td>0.002</td>
<td>0.011</td>
</tr>
<tr>
<td></td>
<td>(0.10)</td>
<td>(0.64)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>532,520</td>
<td>532,520</td>
<td>532,520</td>
</tr>
<tr>
<td>R²</td>
<td>0.550</td>
<td>0.656</td>
<td>0.552</td>
</tr>
</tbody>
</table>

Panel B

<table>
<thead>
<tr>
<th></th>
<th>(1) Log(monthly total spending)</th>
<th>(2) Log(monthly credit card spending)</th>
<th>(3) Log(monthly debit card spending)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>prepost</td>
<td>prepost</td>
<td>prepost</td>
</tr>
<tr>
<td>Married × High Checking Balance × 1post</td>
<td>−0.098***</td>
<td>−0.074***</td>
<td>0.027</td>
</tr>
<tr>
<td></td>
<td>(−3.42)</td>
<td>(−3.57)</td>
<td>(1.38)</td>
</tr>
<tr>
<td>Married × 1prev</td>
<td>0.016</td>
<td>0.002</td>
<td>0.011</td>
</tr>
<tr>
<td></td>
<td>(0.10)</td>
<td>(0.64)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>532,520</td>
<td>532,520</td>
<td>532,520</td>
</tr>
<tr>
<td>R²</td>
<td>0.550</td>
<td>0.656</td>
<td>0.552</td>
</tr>
</tbody>
</table>

Panel C

<table>
<thead>
<tr>
<th></th>
<th>(1) Log(monthly total spending)</th>
<th>(2) Log(monthly credit card spending)</th>
<th>(3) Log(monthly debit card spending)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>prepost</td>
<td>prepost</td>
<td>prepost</td>
</tr>
<tr>
<td>Married × High Checking Balance × 1post</td>
<td>−0.098***</td>
<td>−0.074***</td>
<td>0.027</td>
</tr>
<tr>
<td></td>
<td>(−3.42)</td>
<td>(−3.57)</td>
<td>(1.38)</td>
</tr>
<tr>
<td>Married × 1prev</td>
<td>0.016</td>
<td>0.002</td>
<td>0.011</td>
</tr>
<tr>
<td></td>
<td>(0.10)</td>
<td>(0.64)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>532,520</td>
<td>532,520</td>
<td>532,520</td>
</tr>
<tr>
<td>R²</td>
<td>0.550</td>
<td>0.656</td>
<td>0.552</td>
</tr>
</tbody>
</table>

This table shows the spending response heterogeneity among high- and low-precautionary-saving individuals in the treatment group. “High checking balance” is a dummy variable that takes a value of 1 if the consumer’s average checking account balance between April and August 2010 is in the top 30% of the sample distribution and 0 otherwise. “No college degree” is a dummy equal to 1 if the consumer’s highest educational achievement is lower than a college degree and 0 otherwise. “High income” is a dummy equal to 1 if the consumer’s average monthly income in the prepolicy months (May–September 2010) is in the top 30% of the sample distribution and 0 otherwise. “Low total liquid funds” is a dummy that takes a value of 1 of the average of a consumer’s combined liquid funds between April and August 2010 is in the bottom 30% of the sample distribution and 0 otherwise. 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. Constants are not reported. T-statistics are reported in parentheses under the coefficient estimates. Statistical significance at ***, **, and *10%

Home equity channel versus consumption commitment channel. In addition to the role of policy 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 to their apartment (Chetty & Szeidl, 2007). The extension of the MOP 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 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 rose rapidly in the years leading up to the policy likely have more home equity.

Using the published transaction price data in the public housing market from HDB, we compute the average house price growth rates at the postal sector level based on transaction prices in the HDB resale market from 2007 to 2009 (see figure IA.3 in the online appendix for the distribution of the postal sectors and the price growth rates). 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 and 82 postal sectors in Singapore, but many are unbuilt areas.

### D. Robustness Checks

We have performed a battery of additional tests to verify the robustness of our results. First, to address the concern that the policy might have influenced the potential housing demand and supply and changed the (expected) price appreciation rate, we include region-time fixed effects to capture house price dynamics at the more local level. Second, we exploit the fact that singles under age 35 are prohibited from owning public housing and conduct a stronger difference-in-differences test: we use singles under age 35 as the control group and married consumers under age 35 as the treatment group. Third, to better control for the observable differences between the treatment group and the control group in our main specification, we perform a nearest-neighbor propensity score match on the married and single individuals based on their demographics and financial variables. Based on the matched sample, we repeat the main analysis. Finally, we perform two additional falsification tests by using the same treatment and control group identification in a comparable housing market that is unaffected by the policy shock and by using the same identification among the nonowner subpopulation in the treated housing market.

All of our results continue to hold, and we leave the detailed discussion to the online appendix.

### VI. Conclusion

In this paper, we exploit a unique housing policy experiment in Singapore that reduced home owners’ access to their home equity. In August 2010, the HDB, in an effort to cool down the heated housing market and reduce speculative activities, extended the MOP for resale HDB apartments in the public housing market from three years to five 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 data set 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 interesting, the consumption response is concentrated in credit card spending; there is no consumption response in debit card use or cash or check spending. Individuals with more limited access to the credit market or individuals with stronger precautionary saving respond more strongly to the policy shock. These results suggest that 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 a less complete credit market or those who are concerned about future consumption uncertainty.

Our results provide new insights into 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. Policymakers around the world can use such cooling measures to slow consumption, allowing them to control inflation and economic growth.

REFERENCES


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