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Winners, Losers, and Near-Rationality: Heterogeneity in the MPC out of a Large Stimulus Tax Rebate[1]

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Abstract

This paper documents heterogeneity in consumption responses to a large stimulus tax rebate based on household exposure to a housing price cycle. Linking geocoded household expenditure and financial transactions data to local housing price indices in Japan, we estimate a U-shaped pattern in the marginal propensity to consume with respect to housing price growth. Recipients living in areas with the smallest housing price gains during the 1980s spent 44% of the 1994 rebate within three months of payment, compared to 23% among recipients in areas which experienced the largest housing price gains. While we find limited heterogeneity in marginal propensities to consume among households in less-affected areas, MPCs are higher for younger, renter households with no debt residing in more-affected areas. These findings are consistent with near-rational households for which the pricing shock was small relative to permanent income spending a larger fraction of the tax rebate. Our analysis suggests fiscal stimulus payments primarily induce spending among "winner" households who face minimal exposure to housing price cycles.

Keywords: fiscal stimulus, tax rebate, marginal propensity to consume, near-rationality, housing price cycles, liquidity constraints

JEL classification: D15, E21, E62, H31, R31

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1 Introduction

Housing price cycles generate large swings in household wealth and often precede severe economic downturns. Households which purchased homes when prices were high witness the erosion of a major portion of their portfolio during busts, while renter households now find homeownership to be relatively more affordable (Sinai & Souleles 2005). Amid these conditions, fiscal stimulus payments, such as the 2001 and 2008 income tax rebates (Johnson et al. 2006; Parker et al. 2013) and the 2011-2012 payroll tax holiday in the U.S. (Sahm et al. 2012, 2015), aim to induce households to spend by providing temporary tax relief. But which households benefit the most from such policies, and what are the implications of heterogeneous household experiences during real estate booms for the aggregate expenditure response to stimulus payments during recessions?

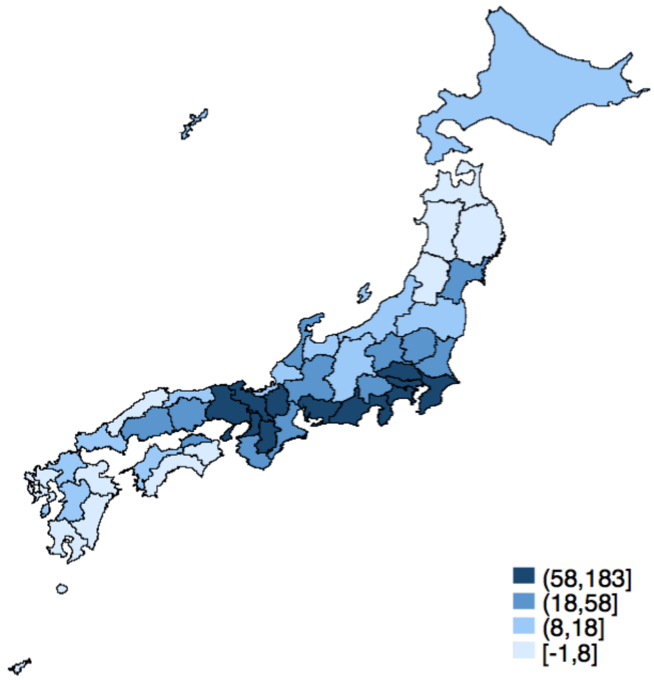
In this paper, we use geocoded household survey data to document new sources of heterogeneity in households' marginal propensity to consume (MPC) out of a large, one-time stimulus tax rebate in 1994 Japan. Linking households to local housing price indices, we estimate a U-shaped pattern in MPCs with respect to housing price growth during the preceding boom period (1985-1990). Recipients living in areas with the smallest housing price gains during the 1980s spent 47% of the rebate within three months of receipt, compared to 24% among recipients in the most-affected areas, and no statistically or economically significant response among recipients in areas only moderately exposed to the housing cycle. We further explore the determinants of these responses using common proxies for liquidity constraints and consumption commitments, including age, debt service ratios, mortgage payments, and volatility in cash holdings. We find minimal heterogeneity in MPCs among households in less-affected areas, but show that expenditure responses are driven by younger, renter households with no debt in the most-affected areas.

Since many fiscal stimulus programs are implemented through the income tax system, aggregate payment amounts tend to be concentrated in areas with many high-income households. Figure 1 shows the spatial distribution of housing price growth during the 1980s real estate boom in Japan (Panel A), along with the distribution of aggregate payments from an income tax rebate enacted in 1994 (Panel B). Rebate recipients in areas which experienced the largest boom-bust cycles in housing prices also received a disproportionate share of outlays from the tax rebate. Households in the top quartile of prefectures by 1980s housing price growth received 57% of the total 30 billion USD allocated to two or more person households. The efficacy of tax rebates in stimulating consumption thus depends critically on the extent to which taxable income is positively related to marginal propensities to consume.

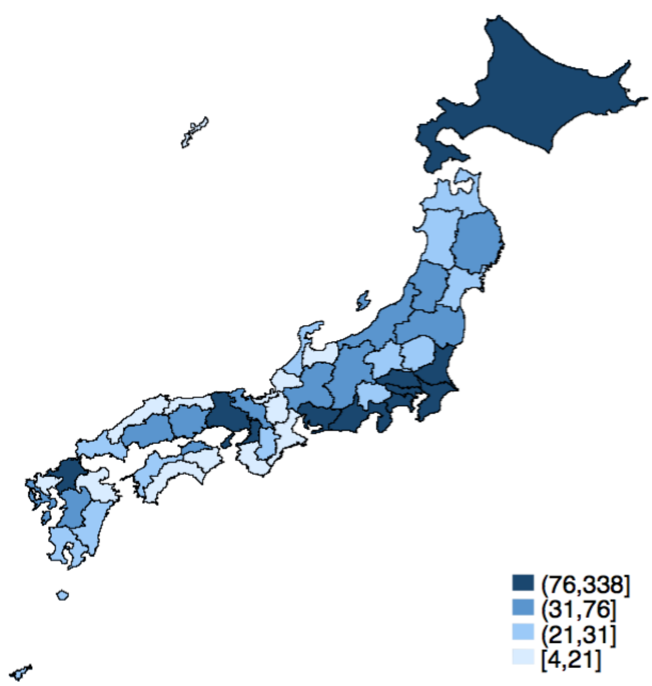
Yet, our results suggest that income is an imperfect tag for the magnitude of expenditure responses to stimulus payments. In the absence of general equilibrium spillovers of expenditures across regions, the U-shaped patterns in MPCs with respect to the housing cycle would imply the government could generate a larger aggregate response by shifting outlays towards households in less risky housing markets or those with limited exposure to housing price cycles. We estimate

FIGURE 1. Spatial Distribution of Housing Land Price Growth vs. Tax Rebate Outlays

A. Cumulative Housing Price Growth (1985-1990, percent)



B. Aggregate Income Tax Rebate (billions of real 2010 yen)



Sources: Authors' tabulations based on household data from the Family Income and Expenditure Survey (FIES). Residential land price growth rates constructed using the methods outlined in LaPoint (2020).

that in an extreme counterfactual scenario where all payments were remitted to the households least exposed to housing market risk, the 1994 stimulus rebates would have boosted aggregate non-durable PCE by an additional 18%. More generally, a fiscal stimulus policy which targets the "losers" from housing price cycles may be a losing policy.

Our work relates to a substantial body of research estimating the expenditure response to fiscal stimulus programs during the Great Recession in the U.S. The Japanese tax rebate we study in this paper amounted to 30 billion USD in payments to two or more person households between June and July 1994 (37 billion USD including single households), and 55 billion USD in total payments disbursed in 1994 (Watanabe et al. 1999). The average recipient household received $900 from the June-July event, or roughly $300 per person. Hence, in terms of per capita payments and aggregate outlays, the 1994 tax program was similar in magnitude to the 2001 U.S. tax rebate studied in Shapiro & Slemrod (2003) and Johnson et al. (2006), which included 38 billion USD in payments of $300 to single households and $600 to married households.[1]

To estimate MPCs, we use a triple differences (DDD) design which compares households during the tax cut period to otherwise similar households in non-tax cut years. The third difference comes from further comparisons within each of those groups between households residing in areas which experienced recent housing price cycles of different magnitudes. This results in the fairly weak identifying assumption that there was no other shock in June-July 1994 affecting the relative expenditure patterns of tax cut recipients within a group of prefectures which experienced similar housing price growth in the 1980s. We show that the distribution of rebate amounts relative to a household's normal monthly income is similar across dosages of the housing price shock. This suggests our finding of regional heterogeneity in MPCs is unlikely to be mechanically driven by non-linearities in the income tax schedule.

In our baseline results which average across geographic areas, we find recipient households spent 11% of the June-July 1994 rebate on non-durables and 15% on total expenditures within a quarter of receipt. These estimates are lower than the non-durable expenditure responses which range from 20% to 40% in the literature on the U.S. tax rebates in 2001 and 2008 (Johnson et al. 2006; Parker et al. 2013), but are similar to the MPC out of a 6 billion dollar shopping coupon stimulus program enacted in Japan in 1999 (Hsieh et al. 2010). One possibility is that the payments we study may be less salient because the 1994 rebates were remitted through payroll tax withholding in the form of higher after-tax pay in stimulus months rather than as a separate payment. This was the main argument of Sahm et al. (2012) for why the 2009 withholding reduction boosted spending by only 13%, compared to the 25% estimate of Shapiro & Slemrod (2009) for the 2008 payments.[2]

[1] The tax rebate component of the 2008 Economic Stimulus Act studied in Shapiro & Slemrod (2009) and Parker et al. (2013) amounted to 100 billion USD, with payment amounts ranging from $300 to $600 for single tax filers, and from $600 to $1,200 for married couples. The June-July 1994 payments amounted to 0.7% of 1994 Japanese GDP, the 2001 U.S. rebates were 0.4% of 2001 U.S. GDP, and the 2008 U.S. rebates were 0.7% of 2008 U.S. GDP.

[2] However, Graziani et al. (2016) study a more recent payroll tax cut and find much larger *ex post* reported survey spending of 36% of funds disbursed over 2011.

Another story is that the average spending response to the 1994 tax cut was dampened by the existence of "balance sheet" households who smooth their savings or debt repayment. Sahm et al. (2015) find that roughly one-third of households responded to the 2011-2012 payroll tax holiday by maintaining a fixed level of savings. Agarwal et al. (2007) and Agarwal & Qian (2014) uncover a similar finding using credit card data of 2001 rebate recipients and surprise 2011 stimulus payments in Singapore, respectively, and show unconstrained consumers were the most likely to pay down debt. In contrast, we find only a small response of, at most, a 2% increase in mortgage repayments in June 1994. Moreover, this response is completely offset by negative growth in repayment amounts after July 1994, suggesting some households borrow against future income by making a larger mortgage payment in the month preceding rebate receipt.[3] Balance sheet repair is thus unlikely to be a prominent mechanism underlying our results.

We leverage a detailed combination of data on household expenditures and financial transaction flows to distinguish between responses due to liquidity constraints versus household exposure to the housing price cycle. A large literature dating from Zeldes (1989) argues that empirical proxies for liquidity constraints help explain heterogeneous responses to anticipated income shocks. Misra & Surico (2014) run quantile regressions of survey expenditures on 2001 and 2008 rebate payments and find the responses were concentrated among households with high levels of mortgage debt and low-income, renter households. Similarly, Broda & Parker (2014) use scanner data to show that spending responses to the 2008 rebates were concentrated among low-income and low-wealth households. Another series of papers emphasizes the importance of cash on hand and asset liquidity (Kaplan & Violante 2014; Kaplan et al. 2014; Jappelli & Pistaferri 2014), or marginal interest rates (Kreiner et al. 2019) for explaining heterogeneity in the MPC out of stimulus payments.

Our findings paint a more complicated picture of the role of liquidity constraints in generating MPC heterogeneity. Although we find some proxies for low liquidity, such as youth and renter status, predict higher MPCs in the cross-section, these patterns are not consistent across other types of liquidity constraints such as cash volatility or debt service ratios. Our results on heterogeneity by liquidity measures are also not consistent across groups of households sorted by exposure to the housing price cycle. In particular, the U-shaped pattern in MPCs with respect to housing price growth is driven by a combination of younger, renter households with no debt in more exposed areas and statistically uniform spending responses by liquidity constraints within less exposed areas.

These outcomes accord with the near-rationality hypothesis advanced in Browning & Crossley (2001) and Kueng (2015; 2018) and supported by recent evidence from Christelis et al. (2019) and Fagereng et al. (2019), who find that MPCs decline with the size of income shocks. At both ends of the U-shape, spending responses to the rebates were driven by recipients for whom the pricing shock had a limited effect on permanent income. In the less exposed areas, housing prices hardly moved during the 1980s and 1990s, whereas in the more exposed areas, renter households and those with

[3] Mortgage debt is the predominant form of debt for households in our setting, with 37% of households holding a mortgage. Payday loans and credit card debt are virtually non-existent during this time period.

no mortgage debt were relatively less exposed to the housing cycle than their indebted homeowner counterparts. For these less exposed households, rebate amounts would have been small relative to permanent income, leading to smaller utility losses from non-smoothing and thus larger spending responses. In closely related work, Dhungana (2018) documents a similar U-shaped relationship between MPCs out of the 2008 rebates and housing price growth at the MSA level, but only for individuals who face self-reported financing constraints. He develops a heterogeneous agent life-cycle model à la Kaplan & Violante (2014) with mortgage refinancing and default options, yet this model fails to generate the non-monotonic relationship observed in the data. This supports our arguments in favor of a more straightforward narrative based on near-rationality.

Finally, in classifying winners and losers in the context of fiscal stimulus policy, this paper complements research on the redistributive consequences of monetary policy. Doepke & Schneider (2006) demonstrate that young households with fixed-rate mortgages benefit from inflation at the expense of rich, older households. Wong (2019) finds larger consumption responses to interest rate shocks among young homeowners due to the prevalence of fixed-rate mortgage refinancing. Auclert (2018) identifies three channels which determine the effectiveness of monetary policy: the Fisher channel in Doepke & Schneider (2006), the interest rate exposure channel of Wong (2019) and Eichenbaum et al. (2019), and earnings heterogeneity through profit sharing. Taken together with our results, these channels suggest winners and losers from fiscal versus monetary stimulus may be drawn from distinct populations. This implies interactions between the two types of stimulus could be important for the overall effects of countercyclical policy on economic recovery.

The rest of the paper is structured as follows: Section 2 provides background on the 1994 stimulus tax policy. Section 3 describes how we link households' financial transactions data to housing prices and determine the tax rebate amounts they received. Section 4 presents our triple differences empirical strategy. Section 5 documents various sources of heterogeneity in MPCs out of the tax rebate. Section 6 concludes.

2 Background: 1994 Income Tax Rebate

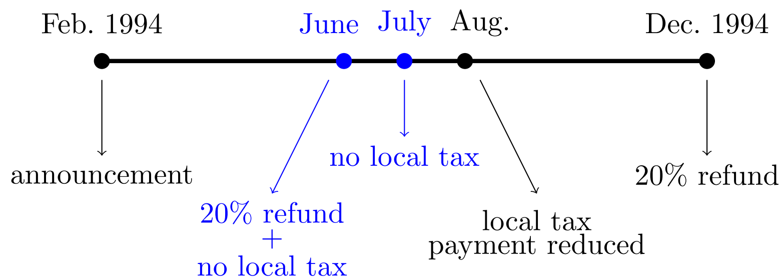
The collapse in land values and the stock market that began in 1990 prompted the ruling Liberal Democratic Party (LDP) to announce a series of tax cuts totalling 5.5 trillion yen, or approximately 55 billion USD, as part of the Outline of Tax Reform on February 10, 1994 (Watanabe et al. 1999). This temporary reform was split into four major sub-events:

(i) A June refund to each household equal to 20% of the national income tax liability due between January and June 1994. This portion of the refund was capped at 1,000,000 JPY.

(ii) A suspension of local income tax payments in June and July 1994.

(iii) An overall 20% reduction in annual local income tax liability effective between August 1994 and May 1995. Since local income tax liability is set according to a June to May calendar and

FIGURE 2. Timeline of the 1994 Income Tax Cut and Rebate



no local tax payments were due in June and July, this part of the reform applied retroactively.[4]

(iv) Another refund in December to each household equal to 20% of the national income tax liability due between July and December 1994.

The bulk of the 55 billion USD in tax cut funds in 1994 was concentrated in the 37 billion USD distributed between the two events in June and July, with the remaining 18 billion USD in funds allocated towards lowering local tax payments in August 1994 through May 1995 and towards the December refunds.[5]

Figure 2 summarizes all events included in the 1994 tax reform. We highlight in blue the two events we examine in this paper: the 20% income tax refund in June and the payroll tax holiday which spanned June and July. We do not analyze the December 1994 episode which provided an additional 20% income tax refund to households. The December refund overlapped with a major tax reform implemented on January 1, 1995 which drastically cut national income taxes (Ishi 2001).[6] Because December refunds were disbursed through a decrease in withholding, for households which normally receive employment income towards the end of the month the expenditure response to this payment would have likely been concentrated in January. We thus cannot separate the effects of the December refund from the effects of the 1995 tax reform.

In the next section, we compute the tax payments in June and July that each household in our sample would have been eligible to receive based on program rules and their observed history of national and local income tax payments. We find the average household received \$900 from the combined June-July payments, or \$300 per person. Scaling up these estimates using our survey

[4]To illustrate, suppose X is the normal monthly local income tax payment due. Then for a household which was below the 200,000 yen cap on the total local rebate amount, the 20% reduction translated to a cut of $2.4X$. The tax holiday in June and July accounted for $2X$, and the remaining $0.4X$ proportion was then split evenly over the remaining 10 months from August 1994 to May 1995.

[5]In Appendix A, we provide more background on the Japanese income tax system and examples of how the various components of the 1994 tax reform interacted to affect a household's annual income tax liability.

[6]Aggregating the results from a simple narrative event study design, Watanabe et al. (1999) estimate that the 1995 permanent income tax cuts contributed between 0.11 and 0.63 p.p. to annual consumption growth, compared to 0.01 to 0.09 p.p. for the 1994 temporary tax rebates.

population weights, we account for 30 billion USD remitted to two or more person households, or 0.7% of 1994 GDP. The policy experiment we analyze was therefore on the same scale as the 2001 U.S. rebate in terms of aggregate payments, and on the same scale as the 2008 U.S. rebate as a fraction of contemporaneous annual GDP.

3 Data

This section describes our construction of a dataset linking household expenditures and financial transaction information to the 1994 stimulus tax rebate and to regional housing price shocks during the boom period (1985-1990). We refer readers to LaPoint (2020) for more details on the estimation of historical local real estate price indices for Japan.

3.1 Japanese Family Income and Expenditure Survey

The main dataset we use in this paper is the Japanese Family Income and Expenditure Survey (FIES). This is a rolling panel of households, each interviewed for six consecutive months before being replaced by a new respondent household. The roughly 8,000 households that are interviewed each month make daily entries of all expenditures and income in a diary. Interviewers collect these diaries twice per month, sort the expenditures into over 600 categories, and then aggregate the resulting data to monthly observations made available to researchers. During the first interview, households are asked to report annual income in the preceding year and demographic information pertaining to household size, homeownership, and employment status of the household head.[7]

The FIES is similar in structure to the Diary Survey of the Consumer Expenditure Survey (CE) in the U.S., with the main distinction that the FIES provides additional information on household financial transactions. These variables include flows such as mortgage repayment, changes in principal on loans, and securities bought and sold within the month. We make heavy use of financial transaction variables to highlight mechanisms for heterogeneity in consumption responses, including by constructing measures of liquidity constraints and using asset and liability flows as outcomes.[8]

Given that we have a relatively small number of households ($N \approx 1,400$) for which we can compute the June-July 1994 tax rebate amounts, we apply a parsimonious set of sampling restrictions. Since our empirical approach relies on comparing consumption in periods when a rebate was received to consumption in periods in which no rebate was implemented, it is important

[7]Single households and those where the household head is employed in agriculture are excluded from the survey. Interviewers identify the household head among members of the household based on self-reported demographic information. Typically, the household head is the individual with the highest annual income.

[8]Prior to 2000, balance sheet stock variables such as mortgage debt outstanding are only available for the quarter of our sample that rotates into the survey in August, September, or October. For this reason, to maximize statistical power we rely on measures of liquidity constraints based on observable demographics and financial transactions.

that we exclude from our sample households with non-standard expenditure patterns. To this end, we check that our results are robust to dropping households if they experience large shocks within the panel such as a change in homeownership status, gender, job status, or a change in the age of the household head equal to more than one year.[9] We also exclude households headed by very young (under age 20) or very old (over age 80) persons, and those which exit the survey before the end of the sixth interview month.

Our empirical strategy conditions on eligibility for receiving tax rebates rather than comparing the response of recipients to non-recipients around policy implementation. We adopt this approach because whether a household receives a rebate is perfectly collinear with whether they recently earned a salary, and thus endogenously related to spending via extensive margin labor supply decisions. Additionally, we cannot identify when self-employed individuals received tax rebates from the 1994 policy, as payments to the self-employed were contingent on annual income tax return filing.[10] For these reasons we drop self-employed households and those which reported either zero labor income during any month of the panel or no annual income in the preceding year.[11]

3.2 Tax Rebates in the FIES

Given that households in the FIES were not required to report the receipt of income tax rebates in 1994, we use the program rules and households' self-reported income tax payments within the panel to compute payment amounts. As described in Section 2, rebate payments featured national and local components.

The national rebate amount, distributed in June 1994, refunded 20% of the national income tax due through withholding from January through June 1994. This means the national tax rebate amount can be backed out via the following accounting identity:

$$Tax6_i^{cf} - Tax6_i = 0.2 \times (Tax1_i + Tax2_i + Tax3_i + Tax4_i + Tax5_i + Tax6_i^{cf}) \tag{3.1}$$

where $Tax6^{cf}$ is the counterfactual national income tax payment a household i would have made in June 1994 had there been no tax rebate, and the other tax variables indicate the observed national income tax payments made in each of the first six months of 1994. Rearranging equation (3.1) to

[9] This would, for instance, exclude nearly all households in which the household head changed during the panel due to death or incapacitation.

[10] In particular, for workers employed by a third-party the tax rebate is automatically determined by the income tax withheld and remitted to the government by the employer. Self-employed individuals and freelancers are required to file annual tax returns at their local tax office between February and March of each year. This means that most self-employed would not have received any payments associated with the national component of the 1994 tax rebate until 1995, and the exact timing would have been specific to the individual taxpayer.

[11] We code households as self-employed when the interviewer classifies the head under any of the following categories: self-employed, corporate manager, freelancer, other, not working, family business.

solve for $Tax6_i^{cf}$, the rebate amount can then be expressed as a function of observed payments:

$$Tax6_i^{cf} - Tax6_i = 0.25 \times (Tax1_i + Tax2_i + Tax3_i + Tax4_i + Tax5_i + Tax6_i) \tag{3.2}$$

Hence, we set the national income tax rebate for each household equal to one-quarter of the sequence of six payments observed from January through June 1994.[12]

For the portion of the stimulus program which suspended local tax payments in June and July 1994, we cannot use accounting identities to compute the payment reduction each household received. Since local tax payments in the FIES include housing and car property tax payments which, depending on the municipality, are due in either June or July, the household's history of local tax payments is an imperfect proxy for the tax cut amount received. However, housing and car taxes are not due in August and September, so we impute the local tax cut amounts in June and July using the average of local tax payments made by the household in August and September.[13]

3.3 Constructing Housing Price Shocks

We aggregate data on individual appraisal records for over 150,000 residential use properties which are publicly available from the Ministry of Land, Infrastructure, Transport and Tourism (MLIT). These are the same data used in LaPoint (2020), who instead focuses on links between non-residential real estate price fluctuations and corporate balance sheets.

A key feature of these appraisal records is that the same set of properties gets surveyed each year by a pair of two real estate professionals. This allows us to obtain quality-adjusted local housing price indices by running regressions of prices on time dummies and property fixed effects.[14] For each prefecture s we subset to plots located in that area and compute a land price index by estimating the following plot-level regression:

$$\log p_{i,t}^s = \delta_t^s + \eta_i^s + \epsilon_{i,t}^s \tag{3.3}$$

$$P_t^s = \exp(\delta_t^s) \tag{3.4}$$

where i indexes an individual land plot, and the plot fixed effects η_i^s control for all time-invariant observed or unobserved characteristics of the land plot and any buildings on top of the land. P_t^s forms our prefectural-level price index, and we compute ΔP_{85-90}^s as our shock to local housing

[12] We note that these accounting procedures for imputing the 1994 tax rebate amounts are similar to the strategy outlined in an older working paper by Hori & Shimizutani (2005).

[13] We offer more details on the national and local tax systems in Appendix A.

[14] The panel dimension of these data means that, unlike the popular repeat sales methods of Case & Shiller (1987; 1989), we do not need to throw away a large number of records to identify the property fixed effects. See Section 2 and Appendix A of LaPoint (2020) for more details on these data and comparisons to other indexing methods.

growth rates during the boom period (1985-1990).[15]

We construct housing price shocks at the prefecture level, rather than at a finer geographic level, for two main reasons. First, we do not observe whether a household in our sample moved to their current location after the collapse in 1990. It is thus possible that the housing price shock we assign to households is a placebo, which would complicate the interpretation of our estimates of regional heterogeneity in consumption responses to the tax rebate. However, while it is possible that many households moved across municipal boundaries – for instance, from Central Tokyo to a suburb – it is less likely that households moved to another prefecture after the onset of the recession.[16]

To the extent that moves across prefectures do occur, it is even less likely that they involve two prefectures at opposite ends of the distribution of housing price changes. As the top panel of Figure 1 demonstrates, the 1980s housing cycle was characterized by disjoint geographic clusters of high and low price growth areas. The top quartile of prefectures by housing price growth in the late 1980s consists of two such clusters: one of five neighboring prefectures centered on Osaka, and another group of seven contiguous prefectures centered on Tokyo. Further, there are no cases where two prefectures at the lowest and highest quartiles of the distribution of housing price growth rates border each other. Hence, only households which moved more than several hundred miles between 1990 and 1994 will receive a placebo shock in our research design.

Concerns related to the geography of the expenditure survey sampling frame also lead us to aggregate housing prices at the prefecture level. The set of Census city codes represented in the FIES is not fixed over time due to municipal mergers beginning in 1990 and the fact that households outside the largest "certainty" cities are sampled in a quasi-random fashion each year. This means there will be no well-defined rankings from less to more exposed areas in the FIES, unless we restrict to households who reside within a set of cities which form a balanced panel. Unfortunately, this would lead us to drop a large fraction of geographic areas which faced minimal exposure to the housing price cycle.[17]

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[15] Appraised properties are selected to be representative of a neighborhood-use category in each given year and are switched out of the survey for another property if their characteristics change dramatically. This means that including a potentially time-varying vector of controls in equation (3.3) would have little impact on our measurement of the time fixed effects, as these would be absorbed by the individual fixed effects for most plots.

[16] The interpretation of these placebo shocks depends on whether households moved from high to low housing price growth cities (or vice versa) during the early 1990s and the liquidity constraints faced by the movers. Suppose, for example, liquidity constrained households move from high price growth to low price growth cities (e.g. they sell their house at a loss and downsize to a cheaper house by moving). In this case we will assign the low price growth shock to these households, even though this does not accurately reflect their local exposure to the nationwide collapse of housing prices. If we were to find that households in the less exposed areas exhibit higher MPCs out of the income tax rebate, we would be unable to disentangle the role of pre-existing liquidity constraints and local exposure to the housing market downturn in producing this result.

[17] The U-shaped relationship between ΔP_{85-90} and MPCs becomes less pronounced when we construct the housing price shock using a balanced panel of cities. Replicating our baseline results using city-level housing price movements, we find that households in cities with the lowest tercile of housing price growth spent 18% of the rebate and those in the highest tercile spent 34%. When sorting by liquidity measures and housing price growth, we find similar results to our baseline analysis using the prefecture-level variation in prices.

We use price growth during the boom period 1985-1990 rather than growth during the bust due to the staggered nature of housing price falls across areas in Japan. To circumvent endogeneity concerns, such as reverse causality between household expenditures and housing prices, we would then need to restrict to a short window after asset prices collapsed, such as 1990-1993. However prices continued to grow moderately in some areas following the 1990 crash of the Nikkei 225, particularly in regions that experienced an overall modest housing cycle. Further, prices in each prefecture do not bottom out until the early 2000s. For these reasons, we view price growth during the boom period as a better summary measure of local exposure to the housing price cycle.[18]

4 Empirical Strategy

We start by estimating regressions which yield the average response to the 1994 June-July tax rebate. The baseline specification is essentially a difference-in-differences (DD) model that estimates an average MPC by comparing recipient households' expenditures before and after the payment month of June, and then comparing these differences across recipient households and otherwise similar non-recipient households observed in adjacent years.

In particular, we estimate specifications of the form:

$$\Delta \log C_{i,t} = \sum_{j=0}^{k} \beta_j \cdot \frac{TaxCut_{i,t-j}}{MonthlyIncome_i} + X'_{i,t} \cdot \gamma + \epsilon_{i,t} \tag{4.1}$$

where $C_{i,t}$ is a category of expenditures made by household i in month-year t, and $X_{i,t}$ is a vector of controls. The variable $TaxCut_{i,t}$ refers to the amount of tax payment received from the stimulus program in t. Our coefficients of interest are β_j, which capture the average expenditure response of households to tax rebate payments at j months since the first payments were received in June 1994. Following a large literature which aims to test the permanent income hypothesis by estimating versions of a log-linearized Euler equation (e.g. Hsieh 2003; Kueng 2015, 2018), as we do in equation (4.1), we normalize tax payments by a measure of permanent income, $MonthlyIncome_i$. Here we use average monthly income as our proxy for permanent income, which is defined as annual household pre-tax income in the year preceding the survey divided by twelve.[19]

Following Aladangady (2017), to convert these estimates to an MPC we also run versions of

[18] We report the full ranking of prefectures by different definitions of the housing price shock in Appendix B.3. The ranking is relatively invariant to using residential or non-residential prices during the boom or bust, and to other sampling restrictions on the types of appraised properties included in the index construction.

[19] Our data are not subject to the critique raised in Kueng (2015) that income-based measures of permanent income from survey data may be heavily biased downward, thus attenuating estimates of the expenditure response. The diary structure of the FIES leads households to under-report irregular or large durable purchases such as vehicles, leading to a downward bias of total expenditures. In contrast, households are asked annual income in the preceding year at the initial interview, and this information is cross-referenced with tax records. In Appendix B.5, we provide evidence of "bunching" in the distribution of total expenditures that is not present in the distribution of our preferred measure of permanent income.

equation (4.1) where we instead scale $TaxCut_{i,t}$ by the sampling-weighted average income to expenditure ratio of household i's group (i.e. non-tax cut year households or recipient households). The transformed model can then be described by:

$$\Delta \log C_{i,t} = \sum_{j=0}^{k} \beta_j \cdot TaxCut^{MPC}_{i,t-j} + X'_{i,t} \cdot \gamma + \epsilon_{i,t} \tag{4.2}$$

$$TaxCut^{MPC}_{i,t} = \frac{TaxCut_{i,t}}{MonthlyIncome_i} \times \sum_{i \in g}^{N} \omega_i \cdot \frac{1}{ExpRatio_i} \tag{4.3}$$

where ω_i are the sampling weights attached to household i in group g, and $ExpRatio_i$ is the average ratio of category expenditures to monthly income over the interview period for household i. Rescaling the rebate amount *ex ante* allows us to directly interpret the estimated coefficients $\hat{\beta}_j$ as MPCs. This helps overcome the well-known bias that comes from converting elastcities to MPCs *ex post* by multiplying the estimates by an average expenditure to income ratio (e.g. Hall 2009).

In our baseline results, the vector of controls $X_{i,t}$ includes a full set of month-year dummies, interview dummies, a quadratic in age of the household head, and a dummy equal to one if the number of household members changed within the preceding month. Interview dummies control for underreporting that comes from the "survey fatigue" phenomenon discussed in Stephens (2003; 2006). In all specifications, we also include in $X_{i,t}$ month-to-month changes in bonus income, scaled by average monthly income. Accounting for bonus receipt is especially important given that full-time employees generally receive a bonus payment (typically equal to two months of salary) in either June or July of each year, which are the rebate implementation months (Hori & Shimizutani 2009). Failing to control for bonus payments would thus lead us to conflate expenditure responses to the tax rebate with responses to large anticipated payments from the employer.

While for the average household the payment from the local tax holiday in July was lower than the June national tax rebate payment, we expect expenditure responses to be concentrated in July. Payments in both months were remitted to households through their regularly scheduled monthly salary payment from an employer. Since many households receive salary payments towards the end of a month, this likely means much of the expenditure response to the initial June payment will not show up in the data until the following month. Hence, β_1 will capture the expenditure response to a combination of the July payment and the portion of household spending responses to the June payment that was not realized in the previous month. Indeed, in our main results we find the entire expenditure response was concentrated in July 1994.

Our main specification accounts for the fact that the average response captured by equation (4.1) masks considerable heterogeneity in expenditure responses based on households' exposure to the housing price cycle that preceded the recession. We sort households based on their current prefecture of residence into bins ℓ of the housing price growth shock ΔP^s_{85-90} constructed in Section

3.3 and estimate the augmented model:

$$\Delta \log C_{i,t} = \mathbb{1}\{i \in \ell\} + \sum_{j=0}^{k} \alpha_j \cdot \frac{TaxCut_{i,t-j}}{MonthlyIncome_i}$$
$$\sum_{\ell=1}^{n} \sum_{j=0}^{k} \beta_{\ell,j} \cdot \frac{TaxCut_{i,t-j}}{MonthlyIncome_i} \times \mathbb{1}\{i \in \ell\} + X'_{i,t} \cdot \gamma + \epsilon_{i,t} \tag{4.4}$$

where $\mathbb{1}\{i \in \ell\}$ is a dummy equal to one if households reside in a prefecture that places them in quantile ℓ of the housing price growth shock ΔP^s_{85-90}. The coefficients $\beta_{\ell,j}$ in equation (4.4) capture the average expenditure response of recipient households located in the cluster of prefectures defined by quantile ℓ at j months since the rebate implementation.

This model compares expenditures of households during and outside the tax cut period to otherwise similar households within the same months in non-tax cut years, as in equation (4.1). The added interactions of $TaxCut$ with $\mathbb{1}\{i \in \ell\}$ isolate the differences between the reference group of households in the least affected areas in non-tax cut years and each of the subgroups of rebate recipients which differ in their exposure to local housing market volatility. Our strategy is similar in spirit to the one pursued by Guren et al. (2020), who use local housing price comovements with regional cycles to identify housing wealth effects. To maintain statistical power across the different subgroups, in our main results we sort recipient households into $n = 3$ bins of the housing price shock.[20] Splitting prefectures by terciles of the housing price shock implies that our concept of a less-affected area corresponds to a prefecture where prices grew cumulatively by less than 12%, or essentially 0% in real terms, during the 1980s.

In all specifications, we pool households from survey waves in 1992-1993 and 1995-1996 which rotated into the survey during the same months as our rebate recipients to form our control group.[21] The identifying assumption underlying both specifications is that there was no other shock in June-July 1994 affecting the expenditure patterns of tax cut recipients relative to their counterparts in the same months in non-tax cut years. Table 1 provides summary statistics for our panels of recipient households and the control group of households from other years, along with t-tests of differences in means. Households in the survey during the stimulus tend to be slightly younger and have more working members. Annual income in the preceding year is also higher among recipients,

[20] We also include dummies indicating the liquidity group of each household when we examine heterogeneous responses by financial transaction measures in Section 5.3. This allows liquidity levels to have a direct effect on expenditure changes even for the households we observe in non-tax cut periods.

[21] Our point estimates are virtually identical when we expand the control group to include all cohorts in 1992-1993 and 1995-1996. The results also qualitatively hold when we restrict the control group to cohorts surveyed within a smaller time frame around the policy, such as 1993 and 1995. Given that the full panel length is only six months, we always exclude from the control groups households that rotated into the survey between January-March or May-December 1994. Such households would have reported expenditures during at least one of the tax cut months, but we cannot precisely impute the local income tax component of $TaxCut$ for these households, as we need to observe several months of normal income tax payments.

TABLE 1. Characteristics of Recipient and Control Households

	Recipient HH		Other HH		
	Mean	s.d.	Mean	s.d.	Difference t-stat
Age of household head	44.38	9.85	44.82	10.34	−3.42
Male household head	0.96	0.20	0.95	0.21	2.25
Number of members	3.67	1.15	3.64	1.16	2.22
Number of working members	1.66	0.77	1.62	0.74	4.81
Homeowner	0.62	0.49	0.61	0.49	0.49
Mortgage holder	0.38	0.48	0.36	0.48	3.30
Debt service ratio	0.11	0.17	0.11	0.18	3.85
Yearly income	7,758	3,435	7,451	3,506	6.84
Total income (diary)	606.48	573.83	575.97	423.70	4.28
Bonus	103.19	312.78	97.31	301.49	1.46
Total expenditures	359.58	253.49	350.82	264.44	2.63
Non-durable expenditures	289.06	180.46	280.85	163.95	3.56
Strict non-durable expenditures	250.98	161.92	243.97	145.73	3.39
Total 1994 tax rebate	90.92	91.72	–	–	–
N	7,350		30,060		37,416

Notes: The table reports survey sampling weighted means, standard deviations, and t-stats for the test of difference in means for characteristics of households in the survey during the tax rebate episode ("Recipient HH") and those in other years, including 1992-1993 and 1995-1996 ("Other HH"). Monetary values are reported in units of thousands of real 2010 JPY. Strict non-durables include goods which are typically consumed within a quarter, following the definition in Lusardi (1996). Yearly income refers to annual pre-tax income in the year preceding the survey, while total income (diary) is average monthly pre-tax income reported during the survey. We define the debt service ratio (DSR) as the ratio of debt repayment to disposable income in Section 5.3.

as the control group includes survey waves from later on in the recession.[22]

The Japanese government also offered households combinations of national income tax rebates and local income tax holidays in June-July of 1995 and 1996.[23] Failing to account for rebates received by the control cohorts would lead us to underestimate the impact of the 1994 stimulus program. We thus include in $X_{i,t}$ rebate amounts and one-month lagged rebate amounts received by households rotating into the survey in 1995 and 1996. By construction these additional control variables are equal to zero for our treated households. We find that our MPC estimates are approximately 10% lower when we do not control for tax rebates received by the control cohorts. The attenuation towards zero is relatively small because the 1995 and 1996 payments were on average about 4% of monthly household income, compared to 8% of monthly income for the 1994 payments.[24]

5 Consumption Responses to the Tax Rebate

In this section we report our main results on heterogeneous consumption responses to the tax rebate and discuss implications for the overall effectiveness of the stimulus tax cut policy.

5.1 Baseline Results

Table 2 presents the baseline results from estimating equation (4.1). We consider spending in non-durables (ND), strict non-durables (SND) and total expenditures (TEXP) as our main consumption categories.[25] In the row labeled "July MPC," we report the coefficient on the first lag of $TaxCut$ from estimating the model in equations (4.2) and (4.3) where we rescale the rebate variable *ex ante*. We estimate an average MPC of 0.14 out of non-durables within the quarter of rebate enactment. In total, households spent 19.1% of every dollar on goods and services.

Expenditure responses are concentrated in July 1994, with no significant response of expenditures in June when households received the first payment, or in the non-rebate month of August. The lack of a response in June accords with the idea that many households receive salary payments at the end of the month, and so spending out of the initial payment spilled over into July. The small negative but insignificant coefficient on the second lag of $TaxCut$ indicates that households did not immediately undo July spending by drastically cutting back their spending in August.

[22]We report summary statistics for recipient and other households by each housing price exposure tercile in Appendix B.1.

[23]In particular, salaried employees were entitled to a 15% rebate of national income taxes paid through withholding between January and June, in addition to a local income tax holiday in June of those years. The calculation is analogous to the one implied by the accounting identities presented in Section 3.2.

[24]The stimulus payments in 1995 and 1996 were much smaller because the local income tax holiday only applied to one month (June), and the national income tax rebate portion was capped at 25,000 JPY, compared to a 1,000,000 JPY cap for the June 1994 rebate.

[25]Non-durables are defined as in Stephens & Unayama (2011) and include food, clothing, and miscellaneous services. Our definition of food excludes alcohol, as the latter is non-perishable and may be stored.

TABLE 2. Baseline Expenditure Responses to the 1994 Tax Rebate

	(1)	(2)	(3)	(4)	(5)	(6)
$TaxCut$	−0.092	−0.017	−0.111			
	(0.100)	(0.097)	(0.133)			
L.$TaxCut$		0.343***	0.264**	0.347***	0.310**	0.407***
		(0.116)	(0.123)	(0.117)	(0.124)	(0.142)
L2.$TaxCut$			−0.167			
			(0.144)			
July MPC		0.136***	0.105**	0.137***	0.106**	0.191***
		(0.046)	(0.049)	(0.046)	(0.042)	(0.067)
Category	ND	ND	ND	ND	SND	TEXP
Controls	✓	✓	✓	✓	✓	✓
N	24,944	24,944	18,708	24,944	24,944	24,944
Adj. R^2	0.052	0.052	0.065	0.052	0.049	0.041

Notes: Each column presents OLS results from estimating equation (4.1) using non-durables (ND), strict non-durables (SND), or total expenditures (TEXP) as the measure of consumption. $TaxCut$ corresponds to the response to the tax rebate in June 1994, L.$TaxCut$ is the July 1994 response, and L2.$TaxCut$ is the August 1994 response. The July MPC row provides the MPC estimate for the coefficient on L.$TaxCut^{MPC}$ in equation (4.2). Robust standard errors clustered by household in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

TABLE 3. WLS Estimates of Baseline Expenditure Responses

	(1)	(2)	(3)	(4)	(5)	(6)
$TaxCut$	-0.054	0.019	-0.069			
	(0.145)	(0.142)	(0.199)			
L.$TaxCut$		0.345^{**}	0.229^{*}	0.341^{**}	0.294^{*}	0.371^{**}
		(0.151)	(0.168)	(0.153)	(0.163)	(0.186)
L2.$TaxCut$			-0.103			
			(0.231)			
July MPC		0.134^{**}	0.116^{*}	0.132^{**}	0.099^{*}	0.169^{**}
		(0.058)	(0.065)	(0.059)	(0.055)	(0.085)
Category	ND	ND	ND	ND	SND	TEXP
Controls	✓	✓	✓	✓	✓	✓
N	24,944	24,944	18,708	24,944	24,944	24,944
Adj. R^2	0.047	0.048	0.059	0.048	0.047	0.035

Notes: Each column presents weighted least squares (WLS) results from estimating equation (4.1) using non-durables (ND), strict non-durables (SND), or total expenditures (TEXP) as the measure of consumption. The weights are survey sampling weights for each household. $TaxCut$ corresponds to the response to the tax rebate in June 1994, L.$TaxCut$ is the July 1994 response, and L2.$TaxCut$ is the August 1994 response. The July MPC row provides the MPC estimate for the coefficient on L.$TaxCut^{MPC}$ in equation (4.2). Robust standard errors clustered by household in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Table 3 shows analogous results when we instead run weighted least squares (WLS) with the survey sampling weights attached to each household. These weights account for the fact that certain households are less likely to be sampled and remain in the survey for the full six-month panel. Although the point estimates are similar across the weighted and unweighted specifications, for non-durables the standard errors are about 30% larger using the weights. This is due to the fact that non-middle aged, self-employed, and part-time households are under-represented in the FIES (Unayama 2018). Since all of these groups are far less likely to have received a substantial income tax rebate, WLS effectively up-weights a mass of zero or near-zero observations of $TaxCut$ in equation (4.1). For the remainder of the paper, we use these more conservative WLS estimates to make statements about consumption responses that apply to the general population.[26]

5.2 Heterogeneity by Housing Cycle Exposure

We now assess heterogeneity in spending responses based on households' exposure to the 1980s housing cycle. Figure 3 compares mean expenditures over April to October for our recipient households in 1994 to those of control households in non-tax cut years, sorting households by terciles of the housing price shock ΔP^s_{85-90}. We do this for non-durables and a more restrictive definition of consumption called strict non-durables (SND) which excludes semi-durable items like clothing and utensils (Lusardi 1996).[27]

A clear U-shaped pattern in spending responses emerges from Figure 3. In the less-affected areas ($\Delta P^s_{85-90} \leq 12\%$) there is a clear 12% spike in July non-durable expenditures for recipient households, relative to 7% for non-tax cut households in other years.[28] We find a similar gap for total expenditures in Panel B of the figure. Meanwhile, we find no statistically significant difference in responses between our treatment and control households in the middle-affected areas ($13\% \leq \Delta P^s_{85-90} \leq 34\%$) for both consumption definitions. In the most-affected areas which experienced very large growth in housing prices ranging from 35% to 183%, we find a small, marginally significant 1 p.p. difference in consumption growth between treatment and control households. Overall, the triple differences estimates implied by this figure suggest the June-July 1994 tax cut stimulated consumption by 5 to 6 p.p. more in the less-affected areas relative to the more-affected areas, and by about 1 p.p. more in the more-affected areas relative to the middle-affected areas.[29]

Table 4 documents this U-shape in MPCs by housing market exposure using estimates from the regression in equation (4.4). Column 1 shows tax rebate recipients in the less-affected areas

[26]We provide analogous results from unweighted specifications in Appendix B.2.

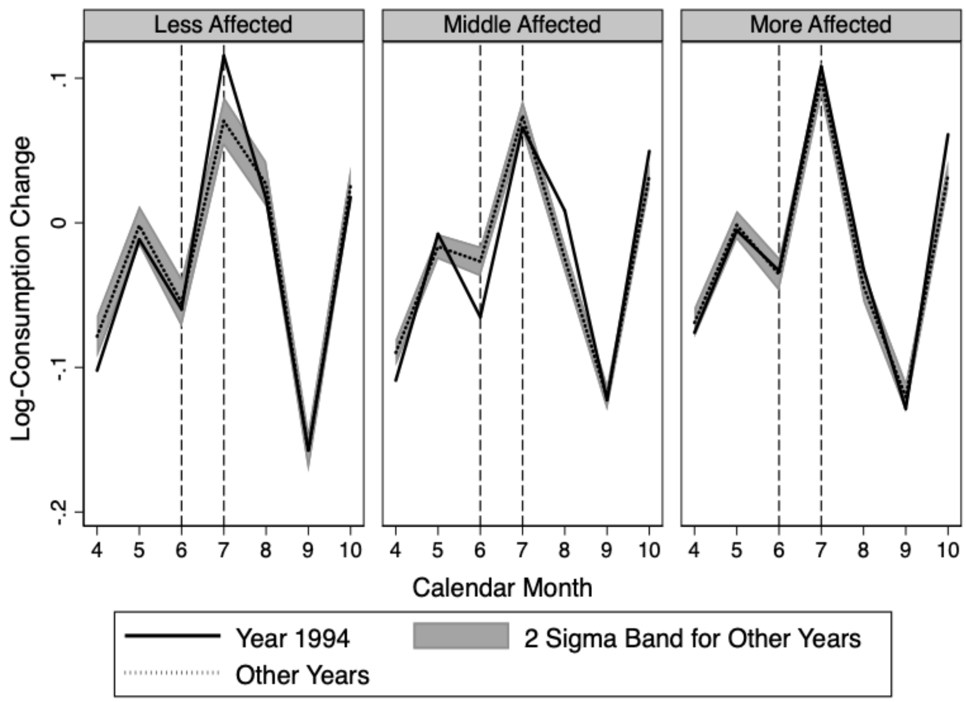
[27]Strict non-durables includes expenditures on goods and services which are typically consumed within a quarter. This amounts to our measure of non-durables minus expenditures on clothing, utensils, recreational goods, reading materials, and miscellaneous service payments.

[28]Recall that many households receive a summer bonus in June-July of each year, leading to a spike in expenditures in July even among the control groups in each panel of the figure.

[29]See Appendix B.7 for the triple differences figure using log changes in strict non-durable expenditures as the outcome variable.

FIGURE 3. Consumption Responses by Housing Price Exposure

A. Non-durable Spending by Housing Price Exposure



B. Total Spending by Housing Price Exposure

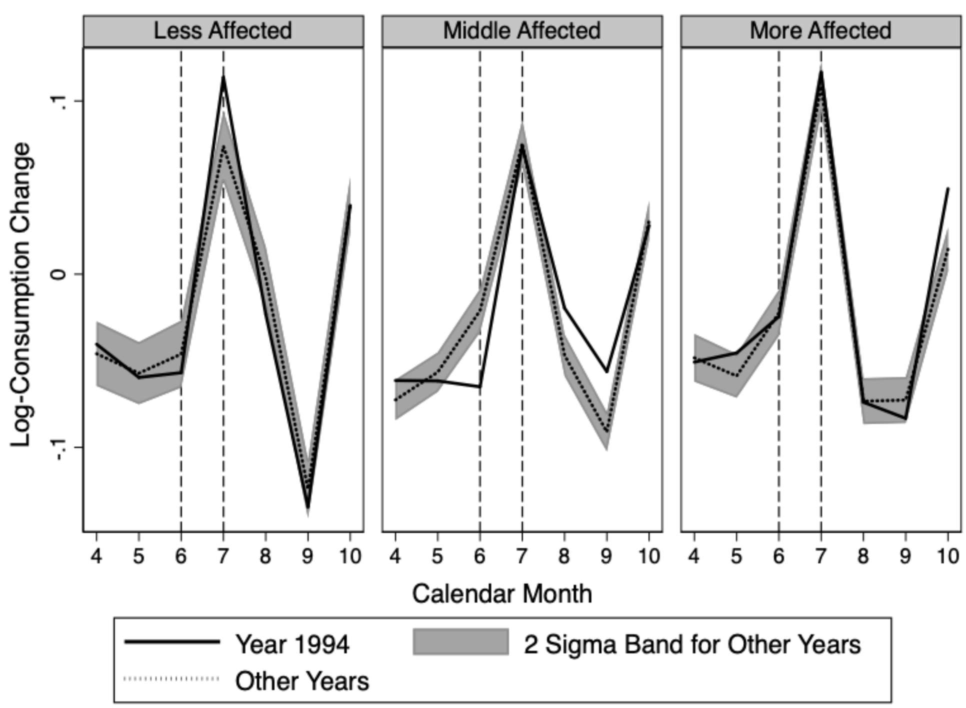


TABLE 4. MPC Estimates by Local Housing Market Exposure

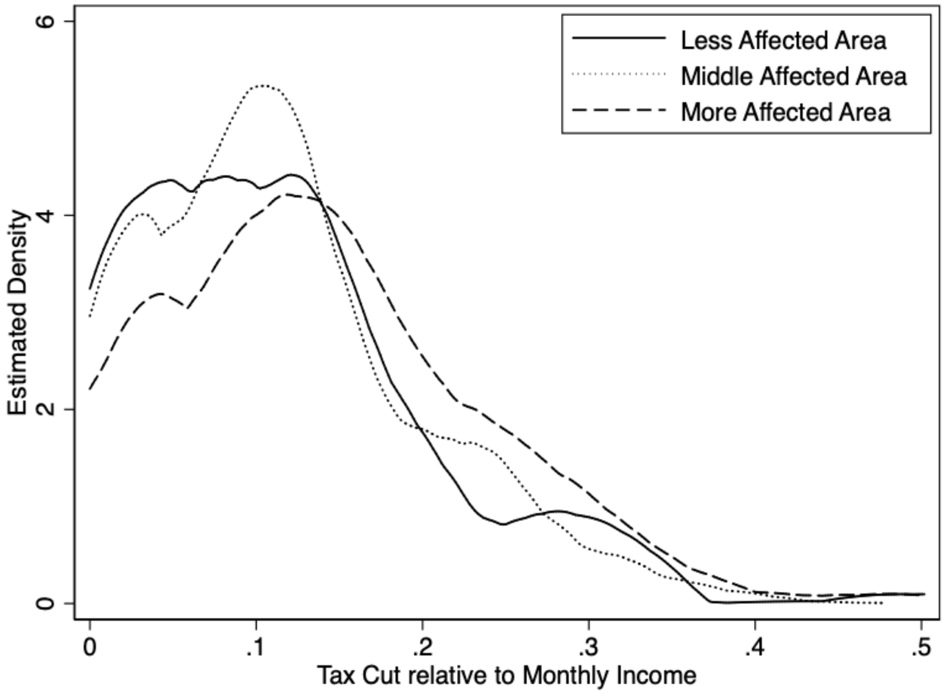
	(1)	(2)	(3)	(4)	(5)	(6)
Less: L.$TaxCut^{MPC}$	0.466**	0.394***	0.292**	0.447***		
	(0.186)	(0.151)	(0.143)	(0.173)		
Middle: L.$TaxCut^{MPC}$	−0.060	0.039	−0.003		0.125	
	(0.150)	(0.104)	(0.082)		(0.110)	
More: L.$TaxCut^{MPC}$	0.244**	0.148**	0.125*			0.083
	(0.097)	(0.069)	(0.068)			(0.075)
Category	TEXP	ND	SND	ND	ND	ND
Controls	✓	✓	✓	✓	✓	✓
p-value $H_0 : \beta_{1,1} = \beta_{2,1}$	0.02	0.05	0.07	–	–	–
p-value $H_0 : \beta_{1,1} = \beta_{3,1}$	0.27	0.13	0.28	–	–	–
p-value $H_0 : \beta_{1,1} = \beta_{2,1} = \beta_{3,1}$	0.05	0.14	0.15	–	–	–
N	24,944	24,944	24,944	4,664	11,280	9,000
Adj. R^2	0.036	0.048	0.047	0.072	0.047	0.052

Notes: The first three columns show MPC estimates from a WLS version of equation (4.4) using non-durables (ND), strict non-durables (SND), or total expenditures (TEXP) as the measure of consumption. The weights are survey sampling weights for each household. L.$TaxCut^{MPC}$ is the response to the tax rebate in July 1994. The last three columns instead correspond to MPC estimates from separately running (4.2) for each area subsample. Robust standard errors clustered by household in parentheses. The p-values for the first three columns correspond to F-tests for equality of the coefficients on the interaction terms in (4.4). $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

spent 47% of the rebate within three months of receipt, compared to 24% among recipients in the most-affected areas, and no response among those living in areas that experienced only a moderate pricing boom. We find similar patterns regardless of whether we examine non-durable or strict non-durable spending, with recipients in the less-affected areas spending 39% of the rebate on non-durables and those in the more-affected areas spending 15%. Even though our standard errors are fairly large when we use the survey sampling weights, we can reject at the 5% level the null that recipients' total spending responses are equal across all areas.

The U-shape in MPCs we document is not due to differences in payment size relative to permanent income. As shown in Figure 4, the distribution of tax rebate payments has a similar shape across terciles of housing price growth. If instead rebates were an outsized fraction of monthly income in one area relative to another, then any finding of heterogeneous expenditure responses with respect to the housing cycle could simply be reflecting non-linearities in the income tax schedule. The ratio of mean total June-July payments relative to average monthly income in the less-affected areas is

FIGURE 4. Distribution of Tax Rebates by Tercile of Housing Price Growth



Notes: The figure plots the estimated density of the variable $TaxCut$ summed across June and July 1994, relative to our preferred measure of permanent income: the household's average monthly pre-tax earnings in the year preceding the survey. We sort households into terciles of the housing price shock defined in Section 3.3.

0.12, compared to 0.13 in middle-affected, and 0.14 in more-affected areas.[30] To the extent that there are a few households which receive very large rebates relative to permanent income in the more affected areas, we estimate least absolute deviation regressions in Appendix B.6 to show that our results on heterogeneity in MPCs are robust to outliers.

5.3 Heterogeneity by Liquidity Measures

We now delve deeper into the sources of heterogeneity in MPCs by liquidity measures and by liquidity measures within terciles of our measure of exposure to housing market risk.

[30] The Kolmogorov-Smirnov test yields a p-value of 0.054 against the null of no difference between the rebate to monthly income ratio across the less affected and middle affected areas, a p-value of 0.001 comparing the less affected and more affected areas, and a p-value of 0.167 comparing the middle and more affected areas. Hence, while we find clear differences in the size of rebates relative to permanent income in the less affected areas relative to the more affected areas, this supports the near-rationality argument we advance in this paper.

5.3.1 Results by Liquidity Measures

We start by defining five main measures of household liquidity. A robust set of research argues that younger and low-income households are more likely to face liquidity constraints (e.g. Zeldes 1989). Due to consumption commitments, homeowners with mortgages are also potentially less capable of smoothing consumption out of cash flows such as the tax rebate we analyze (Chetty & Szeidl 2007). We therefore consider age, age-adjusted income, and mortgage holding as our first three measures.

Our fourth measure of liquidity is informed by the finding in Johnson & Li (2010) that debt service ratios (DSRs), or debt payments relative to disposable income, predict whether households are able to successfully apply for lines of credit. In our results, we split debt-holding households by above and below median DSR and compare them to the 26% of our sample which holds no debt.[31] We also consider households' homeownership status to separate the influence of mortgage debt from the potential wealth shock arising from the collapse in housing prices in the early 1990s.

Finally, we take seriously the notion from the theoretical buffer stock literature that for liquidity constrained households consumption and income closely track each other (Deaton 1991; Carroll 1997, 2001). We translate this behavior to our empirical setting by computing for each household the panel average of squared deviations between disposable income and expenditures on goods and services plus mortgage repayment. Hence, according to this measure, liquidity constraints are tighter for households with low levels of deviations between disposable income and spending.[32]

We report non-durable spending responses in July 1994 to the tax rebate according to each of these proxies for liquidity constraints in Table 5. In each column we estimate an extension of our baseline regression in (4.1) where we interact $TaxCut^{MPC}$ and its monthly lag with dummies indicating low, medium, or high levels of household liquidity. Spending responses are highly concentrated among the youngest rebate recipients, with an MPC in non-durables of 0.43 for households where the head is under 40 years old (column 1, Panel A), and economically insignificant and imprecisely estimated responses for households where the head is over 40.

Liquidity seems to play less of a role in driving responses to the rebate when we turn to our other measures. For age-adjusted income, high-income households exhibit larger MPCs (column 4), although these differences across liquidity levels are not statistically different due to the large standard errors. The half of households with no debt account for about half of the entire spending response (column 3), with no economically significant reaction among households with a positive

[31] More specifically, we compute the debt service ratio as the ratio of mean debt repayment (mortgage repayment + other debt repayment) over the panel to mean disposable income over the panel. Disposable income is defined as total income less the sum of income and local taxes and Social Security premia.

[32] We do not take the approach of attempting to classify households using the "hand-to-mouth" (HtM) concept introduced in Kaplan et al. (2014). To do so, we would need to classify households based on the liquidity of their assets, which are only observable for the roughly 25% of households which rotate into the survey in months that make it impossible for us to compute a tax rebate eligibility. However, the evidence in Hara et al. (2016) on the characteristics of hand-to-mouth households in Japan suggests high versus low age-adjusted income closely approximates a classification based on HtM to non-HtM.

TABLE 5. MPC Estimates by Liquidity Measures

	(1)	(2)	(3)	(4)
Panel A: Non-durables (ND)				
Lower liquidity: L.$TaxCut^{MPC}$	0.428***	0.039	0.093*	0.050
	(0.101)	(0.059)	(0.050)	(0.093)
Medium liquidity: L.$TaxCut^{MPC}$	0.039	0.127	0.106	0.096
	(0.070)	(0.105)	(0.088)	(0.080)
Higher liquidity: L.$TaxCut^{MPC}$	0.013	0.233***	0.242	0.136**
	(0.076)	(0.086)	(0.168)	(0.067)
Adj. R^2	0.049	0.048	0.048	0.048
Panel B: Total expenditures (TEXP)				
Lower liquidity: L.$TaxCut^{MPC}$	0.503***	0.045	0.093	0.101
	(0.125)	(0.083)	(0.067)	(0.134)
Medium liquidity: L.$TaxCut^{MPC}$	0.032	0.111	0.145	0.029
	(0.094)	(0.112)	(0.109)	(0.094)
Higher liquidity: L.$TaxCut^{MPC}$	0.003	0.282***	0.194	0.163**
	(0.078)	(0.103)	(0.167)	(0.081)
Adj. R^2	0.037	0.035	0.035	0.035
Liquidity measure:	Age	Homeownership	DSR	Age-adjusted income
Controls	✓	✓	✓	✓
N	24,944	24,944	24,944	24,944

Notes: Each column provides WLS point estimates for the coefficient on L.$TaxCut^{MPC}$ in a version of equation (4.2) where we interact $TaxCut^{MPC}$ and its lags with dummies indicating the liquidity group of the household. Panel A uses total expenditures (TEXP), while Panel B uses non-durables (ND) as the expenditure category outcome. For age, low liquidity refers to the bottom tercile of the (age < 40), medium liquidity refers to the second tercile ($40 \geq$ age < 50), and high liquidity refers to the top tercile (age ≥ 50). For homeownership, low liquidity refers to renters, middle to homeowners with a mortgage, and high to homeowners with no mortgage. For debt service ratio (DSR), low refers to households with a below median DSR, middle to those with an above median DSR, and high refers to those with no outstanding debt. Finally, age-adjusted income sorts households according to income residualized on a polynomial in age. Robust standard errors clustered by household in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

DSR. We obtain large point estimates for the July spending response for the one-third of households which own their homes and have no mortgage debt, with a statistically significant MPC of 0.28 (column 2, Panel B). Taken together, we fail to find results which uniformly indicate that more liquidity constrained households exhibit higher MPCs out of the stimulus tax cut.

5.3.2 Results by Liquidity Measures and Housing Price Shocks

To investigate why spending responses appear to be concentrated among liquidity-constrained households according to some measures but unconstrained households according to other measures, we now sort households according to both exposure to housing price risk and liquidity. In Table 6 we present results from regressions akin to equation (4.4) where each column represents a regression over households within one of the terciles of ΔP^{s}_{85-90} and we interact rebate payments with an indicator for whether households belong to a group within each measure of liquidity. These regressions also include liquidity group dummies which allow liquidity constraints to have a direct effect on expenditure responses.

Several striking patterns emerge when we sort along both dimensions of household balance sheet exposure in this way. For each liquidity measure within the less-affected areas we find no statistically significant heterogeneity in spending responses, although we still obtain larger point estimates for younger, renter households with no debt. Consistent with the triple differences results in Figure 3 we find limited evidence of a significant spending response among households in the middle-affected areas. In the most-affected areas we see more prominent heterogeneity according to age, debt service, and homeowner status, with responses again concentrated among younger, high-income households with zero debt who rent. Including all goods expenditures, MPCs within the most-affected areas range from 0.59 for households under age 40 to a statistically insignificant 0.05 for households over 50 years old.

5.3.3 Mortgage Repayment Responses

Our results point to lower average MPCs of about 14% out of the 1994 rebate program, compared to the 20-30% average MPC estimates for stimulus programs in the U.S., which paid out similar aggregate amounts to eligible households in 2001 and 2008 (Johnson et al. 2006; Parker et al. 2013). Instead, the average spending response we estimate is almost identical in magnitude to the 13% reported in Sahm et al. (2012), who study the 2009 U.S. payroll tax holiday.[33] Several papers in the literature have argued that low MPCs can be rationalized by a large fraction of households who use stimulus payments to pay down debt in an attempt to maintain a fixed level of savings (Agarwal et al. 2007; Agarwal & Qian 2014; Sahm et al. 2015).

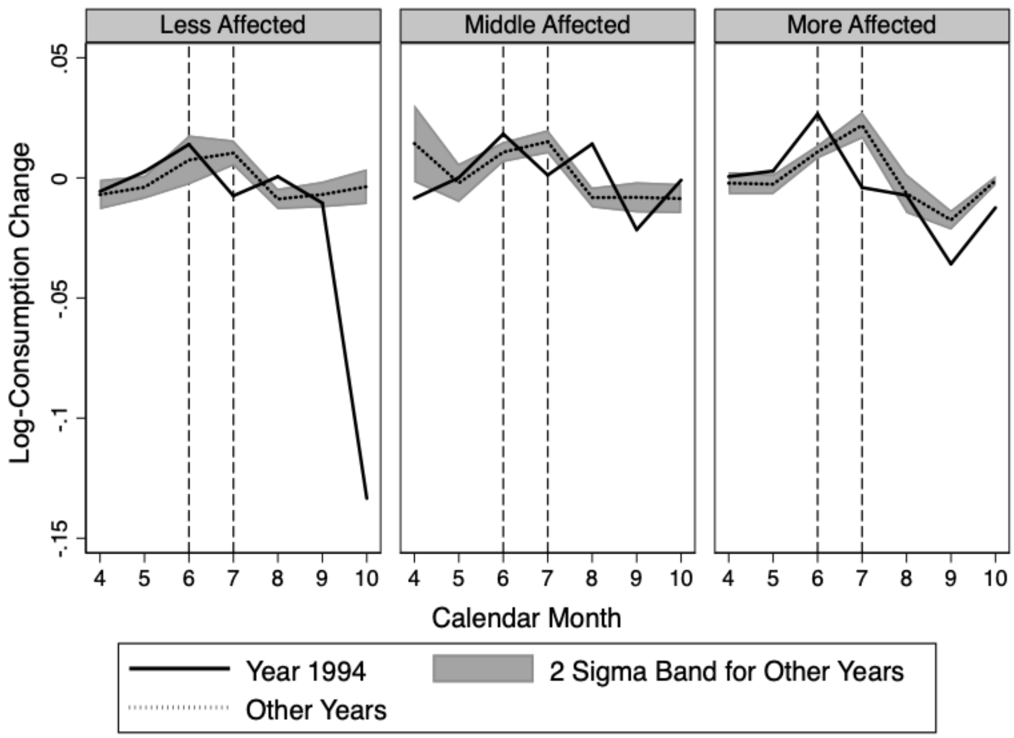
[33] The stimulus program we study contains both a payroll tax holiday component through the suspension of local income tax payments in June-July 1994, and a rebate component through the national income tax system.

TABLE 6. MPC Estimates by Liquidity Measures within a Housing Market Group

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Age						
Younger: L.$TaxCut^{MPC}$	0.584**	0.730***	0.380**	0.680**	0.966***	0.593**
Middle-aged: L.$TaxCut^{MPC}$	0.819*	0.027	−0.017	0.747	−0.212	0.087
Older: L.$TaxCut^{MPC}$	0.205	−0.093	0.031	0.274	−0.213	0.053
p-value $H_0 : \beta_{1,1} = \beta_{2,1} = \beta_{3,1}$	0.223	0.014	0.058	0.421	0.006	0.085
Panel B: Homeownership						
Renter: L.$TaxCut^{MPC}$	0.534**	0.213	0.255*	0.384	0.448**	0.409*
HO w/mortgage: L.$TaxCut^{MPC}$	0.239	0.125	−0.036	0.406	−0.048	0.057
HO: L.$TaxCut^{MPC}$	0.516*	0.064	0.120	0.610*	−0.078	0.166
p-value $H_0 : \beta_{1,1} = \beta_{2,1} = \beta_{3,1}$	0.586	0.873	0.155	0.828	0.152	0.379
Panel C: Debt service ratio						
High DSR: L.$TaxCut^{MPC}$	0.234	0.122	0.066	0.345	0.047	0.105
Low DSR: L.$TaxCut^{MPC}$	0.582*	0.142	0.043	0.674*	0.026	0.246
No debt: L.$TaxCut^{MPC}$	0.541***	0.063	0.270	0.372**	0.031	0.277
p-value $H_0 : \beta_{1,1} = \beta_{2,1} = \beta_{3,1}$	0.344	0.947	0.707	0.699	0.997	0.723
Panel D: Age-adjusted income						
Low: L.$TaxCut^{MPC}$	0.355	−0.167	0.055	−0.062	−0.085	0.183
Middle: L.$TaxCut^{MPC}$	0.588**	0.217	−0.024	0.670**	−0.056	0.008
High: L.$TaxCut^{MPC}$	0.334	0.113	0.105	0.437*	0.077	0.186*
p-value $H_0 : \beta_{1,1} = \beta_{2,1} = \beta_{3,1}$	0.675	0.295	0.716	0.277	0.843	0.683
ΔP^s_{85-90} exposure	Less	Middle	More	Less	Middle	More
Category	ND	ND	ND	TEXP	TEXP	TEXP
Controls	✓	✓	✓	✓	✓	✓
N	4,664	11,280	9,000	4,664	11,280	9,000

Notes: Each column provides WLS point estimates for the coefficient on L.$TaxCut^{MPC}$ in a version of equation (4.4) where we interact $TaxCut^{MPC}$ and its lags with dummies indicating the liquidity group of the household and restrict to households located in areas with different exposure to the housing price cycle based on ΔP^s_{85-90}. For Panel A, sorting is by terciles of age of the household head. For Panel B, we sort by renters, homeowners with a mortgage, and homeowners with no mortgage. In Panel C, we sort by households with a below median DSR, those with an above median DSR, and those with no outstanding debt. Finally, Panel D sorts households according to income residualized on a polynomial in age. Robust standard errors clustered by household. The p-values correspond to F-tests for equality of the coefficients on the interaction terms. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

FIGURE 5. Mortgage Repayment Responses by Housing Price Exposure



Can the patterns we document be explained by differences in mortgage lending rates and propensities to engage in balance sheet repair across regions more affected by the housing cycle? To answer this question, we replicate the triple-differences analysis of Figure 3 by replacing month-to-month changes in goods expenditures with mortgage repayments as the outcome variable. We focus on mortgage debt, since during this time period non-mortgage sources of debt, such as payday loans, credit card, and education debt together constitute only 16% of overall household debt; in fact, only 27% of households have any outstanding non-mortgage debt.[34]

Figure 5 demonstrates how mortgage repayments evolve around the June-July rebate episode across treatment and control households in regions with different exposure to the aggregate housing cycle. Two patterns are immediately noticeable. First, there is a slight bump in June 1994 mortgage repayments for recipients relative to the control groups equal to 1 p.p. in the less-affected areas and 2 p.p. in the more-affected areas. Second, any increase in repayments in June is completely offset

[34]We perform these tabulations for the subset of households who enter the FIES in August, September, or October prior to 1994. Prior to 2000, households entering in those months participated in an additional module, called the Family Savings Survey (FSS), which posed questions about asset holdings and debt outstanding. Unfortunately, households for which we can compute June-July tax rebate amounts entered the survey in March or April, so we cannot link the households in our treatment group to their assets and outstanding debt. For this reason, we rely on demographic proxies for liquidity constraints such as age, and the debt service ratio, which uses debt repayment variables that are observable for all households regardless of their monthly rotation group.

by negative changes in repayments in the quarter after July rebate receipt. We suspect overall repayment responses are negligible due to the fact that prepayments for government-sponsored mortgages are only permissible above a threshold amount equal to 1 million JPY ($\approx$ 10,000 USD), of which the average household rebate amount covers only 9%. Further, outside of 1994, mortgage repayments peak in July when most salaried workers receive summer bonuses. Households frequently opt into biannual amortization schedules which coincide with bonus months in June-July or December.

To sum up, we find little evidence that households used the tax rebates to pay down mortgage debt. A small number of households implicitly borrow against their anticipated rebate and tax holiday receipts in July by engaging in early mortgage repayments in June. This provides further evidence against the importance of liquidity constraints for the consumption non-smoothing behavior we uncover. Liquidity constraints are not a compelling source of regional heterogeneity in spending responses, as credit markets are universally under-developed across Japan during this time period. Only 52% of households carried any debt, and this fraction varies minimally across areas, with a standard deviation of 5 p.p. across prefectures.

5.4 Discussion and Aggregate Implications

We have established two facts about heterogeneity in MPCs out of the 1994 tax rebate. First, there is a U-shape in non-durable consumption responses; households in low housing price growth areas consumed 47% of their payments within a quarter, compared to 24% in high housing price growth areas and no response in only moderately affected areas. Second, the high MPC observed in the less-affected areas is not driven by liquidity constrained households, whereas the more muted MPC in the more-affected areas originates from a distinctive set of "winner" households which had minimal balance sheet exposure to the negative wealth shock from the collapse in housing prices.

We view these results as broadly consistent with a near-rationality hypothesis, whereby households will spend a larger fraction of the tax rebate if the size of the payment is small relative to their permanent income (Kueng 2015, 2018). The notion of near-rationality carries this empirical prediction because welfare losses from not smoothing consumption are of second-order magnitude when the permanent income shock is relatively small. Conditional on any spillovers to labor markets, the impact of a temporary collapse in housing prices on permanent income is negligible for households who are not homeowners or homeowners who carry no debt. The housing boom-bust also has more limited consequences for the permanent income of the young, who have much of their working life ahead of them. Near-rationality helps explain the lack of heterogeneous spending responses within areas where the housing cycle was mild, since regardless of homeownership status, mortgage debt or age, the implied shock to permanent income would have been limited.[35]

[35] In Appendix C we present a simple overlapping generations (OLG) model with households who reach homeownership age at different periods relative to the boom-bust cycle which helps illustrate the near-rationality argument.

The 30 billion USD distributed towards the June-July rebates we study here represents about 5% of 1994Q3 total personal consumption expenditures (PCE), or 16% of non-durable PCE.[36] Abstracting from possible general equilibrium multipliers arising from regional spillovers, our baseline MPC estimates from Table 3 imply the rebate program raised total PCE by about 0.9% and non-durable PCE by 1.7% in 1994Q3. Our estimates are comparable to the 0.8% (total PCE) and 2.9% (non-durable PCE) estimates reported in Johnson et al. (2006), who perform a similar back-of-the-envelope calculation for the 2001 U.S. stimulus rebates. Unfortunately, due the short six-month length of the household panel, we cannot examine whether the rebate stimulated consumption in subsequent quarters.[37]

But how large are differences in expenditure responses across areas relative to the aggregate size of the rebate program? Suppose the prefectures which experienced no real housing price growth account for 31.5% total non-durable expenditures, as in our household microdata during the tax cut months. Our estimate of an MPC in non-durables of 0.39 (from Table 4) among households in these least-affected areas where tax rebate outlays totalled only 4 billion USD then translates to a response equal to 0.27% of non-durable PCE arising from these prefectures.[38] By a similar calculation, an MPC in non-durables of 0.15 among households in the most-affected prefectures where payments totalled 19 billion USD implies a response of 0.50% of non-durable PCE. If the average MPC is linear in the size of the payment, an extreme policy where all June-July payments were sent to the "winners" in the least-affected prefectures would have stimulated aggregate non-durable PCE by 2.0%. This is a 17.6% larger aggregate non-durable PCE response than the one we estimate using the average MPC across all areas. How payments target households based on observable tags such as exposure to a housing price cycle can thus have potentially large consequences for the overall effectiveness of stimulus programs.

6 Conclusion

We study a large income tax rebate during Japan's Lost Decade to document new sources of heterogeneity in marginal propensities to consume (MPC) out of fiscal policy. Outlays during this episode were of a similar magnitude in terms of aggregate spending and per capita payments to the 2001 U.S. rebates, and represented the same fraction (0.7%) of GDP as the 2008 U.S. rebates. Using regional variation in prices induced by the 1980s housing boom-bust cycle, we document a U-shaped pattern in households' MPCs with respect to local housing price growth. Recipients residing in areas

[36]The quarterly PCE series we reference here can be found at https://www.esri.cao.go.jp/en/sna/data/kakuhou/files/2009/23annual_report_e.html, under item 12, "Composition of Final Consumption Expenditure of Households classified by Type," for Supporting Tables in the Flow section of the National Accounts for 2009.

[37]Part of the expenditure response to the June-July rebate may have been reflected in 1994Q2 PCE numbers, meaning that these simple calculations offer a lower bound on the stimulating effect of the policy.

[38]Households in the least-affected prefectures account for 31.5% of total non-durable expenditures in our data. Given total 1994Q3 non-durable PCE of 185 billion USD, we calculate the aggregate effect from these households as $0.315 \times 0.39 \times 4/185 = 0.0027$.

with the smallest housing price gains spent 47% of the rebate within a quarter, compared to 24% among those living in areas experiencing the largest price gains. Delving deeper into the sources of this heterogeneity, we find that while expenditure responses were fairly uniform across households within the least-exposed regions, MPCs were higher for younger, renter households with no debt within the most-exposed regions.

Our findings support the idea that fiscal stimulus payments operating through the income tax system disproportionately disburse payments to low MPC, or "loser" households which are more exposed to housing market risk. Moreover, our work suggests winners with respect to fiscal stimulus policy may be distinct from the predominantly young homeowners with fixed-rate mortgages who are winners under expansionary monetary policy. We thus view studying interactive effects between countercyclical fiscal and monetary policy as a fruitful avenue for future research.

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A Details on the Japanese Income Tax System

In this appendix we provide additional background information on the structure of national and local income taxes in Japan. Our summary is based on Ishi (2001) and the Comprehensive Handbook of Japanese Taxes published by the Ministry of Finance (2006).[39] We also work through an example which illustrates how the June-July 1994 tax rebate amounts were determined.

A.1 National and Local Income Tax Liability

Monthly national income tax withholding in Japan is determined by income thresholds and exemptions made for dependents. For salaried individuals, any exemptions are declared prior to the start of the tax year in January. The employer withholds the appropriate amount from the individual's monthly salary given these exemptions and the employee's implied tax bracket.[40]

Local income tax withholding (sometimes called the "inhabitants" tax) is the sum of two components: one is a flat per-taxpayer fee, which is typically very small relative to overall tax liability, and the other is based on earnings from an employer. The local income tax schedule encompasses income taxes levied at the prefecture level and those levied at the municipality level. Both the flat fee and the variable rate component of local income tax liability will vary depending on the taxpayer's registered address. Employers withhold tax payments due according to the combined prefectural and municipal schedules and remit these local payments to the prefectural tax authority, before the latter remits the revenues back to the municipal government.

One key feature of this income tax system is that, except for exceptional cases where the taxpayer owns a business or earns income from self-employment, total earned income tax payments for salaried individuals are determined in advance based on the previous year's annual (January through December) income. For the inhabitants' tax, payments are fixed between June and May and based on the previous year's (January through December) income. Since the 1994 stimulus rebate amounts we study in this paper were entirely determined by the sequence of counterfactual income tax payments that would have been due in 1994, this means that for salaried taxpayers our $TaxCut$ variable is completely predetermined and therefore not responding to contemporaneous changes in economic conditions.

We provide a detailed example below that contrasts how the national and local components of the income tax schedule interacted with the 1994 stimulus program for a salaried taxpayer versus a self-employed individual who is responsible for filing taxes at the end of the calendar year.

[39] The full English translation of the handbook can be downloaded here: https://www.mof.go.jp/english/tax_policy/publication/comprehensive_handbook_2006e/index.htm.

[40] There are other potential exemptions, which include expenses for commuting, transferring locations within the firm, expenses related to on-the-job training or certifications. All of these are declared by the employee and certified by the employer to determine the appropriate withholding amounts.

A.2 Example: Tax Rebate Calculation

Consider two archetypal taxpayers A and B, each of whom would have, in the absence of the stimulus program, had an annual local income tax liability of 250,000 JPY ($\approx$ 2,500 USD) for the 1994 local fiscal year running from June 1, 1994 to May 31, 1995. Suppose the counterfactual total national income tax liability for each taxpayer would have been 500,000 JPY between January 1994 through December 1994, with each taxpayer making a total of 250,000 JPY in payments between January 1994 and June 1994.

The only difference between the two taxpayers is that A is a salaried employee, while taxpayer B is self-employed. We start by computing the stream of A and B's payments throughout the fiscal year under the counterfactual scenario where there was no stimulus program, and under the actual program rules.[41]

Taxpayer A's Counterfactual and Actual Local Income Tax Payments

	6/1994	7/1994	8/1994	9/1994 – 5/1995	Total payment
With no stimulus:	21,200	20,800	20,800	20,800	250,000
With stimulus:	0	0	20,400	20,000	200,400

Taxpayer B's Counterfactual and Actual Local Income Tax Payments

	FY 1994Q1	FY 1994Q2	FY 1994Q3	FY 1994Q4	Total payment
With no stimulus:	64,000	62,000	62,000	62,000	250,000
With stimulus:	14,400	62,000	62,000	62,000	200,400

Under the stimulus tax program each taxpayer receives a rebate over the local fiscal year equal to 20% of the portion of their local tax liability that depends on income. In this particular case, the flat rate component of the tax liability was a minuscule 2,200 JPY ($\approx$ 22 USD). Hence the total local portion of the rebate was equal to $0.2 \times (250{,}000 - 2{,}200) = 49{,}600$ JPY, where the payments were rounded to the nearest 100 yen. For taxpayer A, 42,000 of this amount was disbursed via a withholding tax holiday in June and July 1994, and the remaining 7,600 was disbursed over the subsequent 10 months, with a 400 yen reduction in August 1994 and an 800 yen reduction in every month thereafter.

[41] This example draws from our reading of Japanese-language fliers from the period which were designed to advertise the stimulus program to eligible taxpayers. One such flier provides the stream of payments assumed in this example to show how each of the sub-parts of the reform affected a household's income tax liability in 1994.

Computing the stream of payments is more straightforward for the self-employed taxpayer B, who we suppose files taxes on a quarterly basis and thus simply receives the entire rebate as a lump-sum tax break at the end of FY 1994Q1, or August 1994. Turning to the national component of the stimulus program, salaried taxpayers like taxpayer A simply received a 20% rebate on national income tax payments that would have been due through withholding by the employer in June 1994. In contrast, the self-employed only receive their 20% rebate on payments made between January 1994 and June 1994 once they have filed their end-of-year tax return and any adjustments (e.g. for dependents) have been made.

Putting everything together, for taxpayer A the total rebate amount across the local and national tax schedules received in June-July 1994 was 49,600 + 0.2 × 250,000 = 99,600 JPY (≈ 1,000 USD). And for taxpayer B the total rebate amount from the local tax schedule consisted of only the 49,600 JPY (≈ 500 USD) remitted in August 1994.

B Robustness of the Results

B.1 Sample Balance by Housing Price Exposure

In this sub-appendix we present additional summary statistics to examine the extent to which our treatment households who received a tax rebate and control group of households in non-tax rebate periods are balanced on observables *within* each tercile of the housing price shock. Overall, with the exception of age, which we directly control for in our main specifications, we do not find any systematic differences between the treatment and control groups across areas. This supports the parallel trends assumption underlying our triple differences design.

Table B.1. Characteristics of Recipient and Control Households in Less-Affected Areas

	Recipient HH		Other HH		
	Mean	s.d.	Mean	s.d.	Difference t-stat
Age of household head	43.22	9.54	44.38	9.93	−4.41
Male household head	0.96	0.19	0.95	0.22	2.38
Number of members	3.73	1.16	3.58	1.14	4.73
Number of working members	1.60	0.71	1.61	0.69	−0.65
Homeowner	0.58	0.49	0.59	0.49	−1.04
Mortgage holder	0.31	0.46	0.38	0.48	−5.21
Yearly income	7,051.20	3,245.83	7,083.62	3,239.68	−0.36
Total income (diary)	600.97	952.66	579.19	478.26	0.85
Bonus	94.61	269.29	104.33	305.33	−1.31
Total expenditures	338.22	218.21	344.42	259.77	−1.03
Non-durable expenditures	270.31	160.12	275.31	172.00	−1.13
Strict non-durable expenditures	234.50	140.27	238.46	153.46	−1.03
Total tax rebates	68.80	83.80	0.00	0.00	30.57
N	1,386		33,108		34,494

Table B.2. Characteristics of Recipient and Control Households in Middle-Affected Areas

	Recipient HH		Other HH		
	Mean	s.d.	Mean	s.d.	Difference t-stat
Age of household head	44.62	9.52	44.82	10.28	−1.18
Male household head	0.95	0.21	0.95	0.21	0.29
Number of members	3.66	1.12	3.61	1.18	2.50
Number of working members	1.70	0.78	1.63	0.74	5.36
Homeowner	0.63	0.48	0.61	0.49	2.38
Mortgage holder	0.39	0.49	0.61	0.49	3.11
Yearly income	7,740.81	3,329.21	7,391.90	3,397.88	6.03
Total income (diary)	602.29	426.09	590.52	471.17	1.58
Bonus	99.57	295.79	104.81	312.38	−1.02
Total expenditures	362.56	270.86	349.89	266.82	2.70
Non-durable expenditures	290.19	197.01	280.31	174.24	2.90
Strict non-durable expenditures	251.63	178.24	242.95	155.78	2.82
Total tax rebates	82.44	92.84	0.00	0.00	52.25
N	3,462		81,570		85,032

Table B.3. Characteristics of Recipient and Control Households in More-Affected Areas

	Recipient HH		Other HH		
	Mean	s.d.	Mean	s.d.	Difference t-stat
Age of household head	44.71	10.40	45.47	10.34	−3.60
Male household head	0.96	0.19	0.96	0.20	1.07
Number of members	3.66	0.18	3.63	1.15	1.02
Number of working members	1.65	0.78	1.65	0.77	−0.03
Homeowner	0.61	0.49	0.62	0.48	−0.88
Mortgage holder	0.40	0.49	0.37	0.48	2.84
Yearly income	8,166.65	3,614.98	8,187.80	3,743.55	−0.29
Total income (diary)	614.71	462.30	622.47	497.21	−0.82
Bonus	112.94	355.21	109.89	350.90	0.42
Total expenditures	366.64	245.81	374.73	277.96	−1.61
Non-durable expenditures	297.46	165.83	300.57	179.64	−0.92
Strict non-durable expenditures	258.91	148.24	259.83	159.17	−0.30
Total tax rebates	93.87	103.78	0.00	0.00	45.25
N	2,502		63,852		66,354

B.2 Unweighted Estimates of Heterogeneous Expenditure Responses

As discussed in Section 5.1, we use the survey sampling weights to obtain our baseline estimates. In our setting, the decision of whether or not to weight represents a tradeoff between representativeness of the results and precision, as the standard errors are around 30% larger for the WLS estimates. Our results on heterogeneity in the MPC by liquidity measures and housing cycle exposure are qualitatively unchanged when we estimate our specifications by OLS. However, the difference in MPCs between the households in the less-affected and more-affected areas is noticeably smaller when we do not weight observations. Households in the less-affected areas spent 36% of the rebates in July while those in the more-affected areas spent 26%, compared to 47% and 24%, respectively, in our baseline WLS specifications.

Table B.4. MPC Estimates by Local Housing Market Exposure (Unweighted)

	(1)	(2)	(3)	(4)	(5)	(6)
Less: L.$TaxCut^{MPC}$	0.363**	0.292**	0.236**	0.225*		
	(0.173)	(0.121)	(0.110)	(0.131)		
Middle: L.$TaxCut^{MPC}$	0.070	0.069	0.041		0.104	
	(0.101)	(0.071)	(0.064)		(0.074)	
More: L.$TaxCut^{MPC}$	0.262***	0.155**	0.130**			0.134**
	(0.085)	(0.061)	(0.057)			(0.065)
Category	TEXP	ND	SND	ND	ND	ND
Controls	✓	✓	✓	✓	✓	✓
p-value $H_0 : \beta_{1,1} = \beta_{2,1}$	0.13	0.10	0.11	–	–	–
p-value $H_0 : \beta_{1,1} = \beta_{3,1}$	0.59	0.30	0.38	–	–	–
p-value $H_0 : \beta_{1,1} = \beta_{2,1} = \beta_{3,1}$	0.19	0.24	0.24	–	–	–
N	24,944	24,944	24,944	4,664	11,280	9,000
Adj. R^2	0.041	0.053	0.050	0.071	0.055	0.048

Notes: The first three columns show MPC estimates from estimating equation (4.4) by OLS using non-durables (ND), strict non-durables (SND), or total expenditures (TEXP) as the measure of consumption. L.$TaxCut^{MPC}$ is the response to the tax rebate in July 1994. The last three columns instead correspond to MPC estimates from separately running (4.2) for each area subsample. Robust standard errors clustered by household in parentheses. These specifications include all survey panels, not just the March and April rotation groups. The p-values for the first three columns correspond to F-tests for equality of the coefficients on the interaction terms in (4.4). $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

B.3 Ranking of Prefectures by Definition of Housing Price Shock

As discussed in Section 3.3, we define the housing price shock ΔP^s_{85-90} as the cumulative growth in the prefecture-level housing price index over the period 1985 to 1990. An alternative would be to use the magnitude of the bust in a short window of time before the tax rebate episode, such as 1990-1993. This distinction turns out to be unimportant for our research design, as boom and bust growth rates have a correlation of -0.91 across prefectures.

Another issue relates to the sampling of plots used to create local housing price indices. Roughly 25% of plots in our sample are located in peripheral areas which are outside city planning zones (CPZs), meaning that these plots are not subject to national land use laws which impose limits on the height and size of buildings. This is a potentially important distinction given the evidence in LaPoint (2020) that land use regulations explain a large fraction of the geographic dispersion in land values during the 1980s real estate cycle, with more *ex ante* land use constrained areas experiencing a larger local cycle. Our main definition of ΔP^s_{85-90} uses the full set of residential properties, which leads to a conservative estimate of the magnitude of local housing price cycles.

Table B.5. MPC Estimates by Liquidity Measures (Unweighted)

	(1)	(2)	(3)	(4)
Panel A: Non-durables (ND)				
Lower liquidity: L.$TaxCut^{MPC}$	0.324***	0.089*	0.075*	0.033
	(0.078)	(0.047)	(0.042)	(0.090)
Medium liquidity: L.$TaxCut^{MPC}$	0.032	0.083	0.149**	0.112*
	(0.060)	(0.083)	(0.069)	(0.064)
Higher liquidity: L.$TaxCut^{MPC}$	0.064	0.201***	0.193	0.141***
	(0.061)	(0.074)	(0.118)	(0.053)
Adj. R^2	0.053	0.053	0.053	0.053
Panel B: Total expenditures (TEXP)				
Lower liquidity: L.$TaxCut^{MPC}$	0.342***	0.140*	0.111*	0.002
	(0.081)	(0.074)	(0.062)	(0.106)
Medium liquidity: L.$TaxCut^{MPC}$	0.037	0.107	0.153**	0.110
	(0.079)	(0.089)	(0.076)	(0.086)
Higher liquidity: L.$TaxCut^{MPC}$	0.098	0.175**	0.212	0.182***
	(0.075)	(0.074)	(0.137)	(0.062)
Adj. R^2	0.041	0.041	0.041	0.041
Liquidity measure:	Age	Homeownership	DSR	Age-adjusted income
Controls	✓	✓	✓	✓
N	24,944	24,944	24,944	24,944

Notes: Each column provides WLS point estimates for the coefficient on L.$TaxCut^{MPC}$ in a version of equation (4.2) where we interact $TaxCut^{MPC}$ and its lags with dummies indicating the liquidity group of the household. Panel A uses total expenditures (TEXP), while Panel B uses non-durables (ND) as the expenditure category outcome. For age, low liquidity refers to the bottom tercile of the (age < 40), medium liquidity refers to the second tercile ($40 \geq$ age < 50), and high liquidity refers to the top tercile (age ≥ 50). For homeownership, low liquidity refers to renters, middle to homeowners with a mortgage, and high to homeowners with no mortgage. For debt service ratio (DSR), low refers to households with a below median DSR, middle to those with an above median DSR, and high refers to those with no outstanding debt. Finally, age-adjusted income sorts households according to income residualized on a polynomial in age. Robust standard errors clustered by household in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Table B.6. MPC Estimates by Liquidity Measures within a Housing Market Group (Unweighted)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Age						
Younger: L.$TaxCut^{MPC}$	0.281	0.399**	0.408***	0.454	0.489***	0.499***
Middle-aged: L.$TaxCut^{MPC}$	0.392	−0.006	−0.006	0.375	−0.061	0.091
Older: L.$TaxCut^{MPC}$	0.114	0.033	0.089	0.202	0.025	0.191
p-value $H_0 : \beta_{1,1} = \beta_{2,1} = \beta_{3,1}$	0.604	0.063	0.019	0.809	0.051	0.159
Panel B: Homeownership						
Renter: L.$TaxCut^{MPC}$	0.333	0.188	0.248**	0.241	0.268	0.266
HO w/mortgage: L.$TaxCut^{MPC}$	0.231	0.129*	0.023	0.604*	0.058	0.180
HO: L.$TaxCut^{MPC}$	0.135	0.003	0.203	0.103	0.048	0.285**
p-value $H_0 : \beta_{1,1} = \beta_{2,1} = \beta_{3,1}$	0.829	0.644	0.207	0.430	0.580	0.828
Panel C: Debt service ratio						
High DSR: L.$TaxCut^{MPC}$	0.120	0.040	0.112	0.343	0.036	0.190*
Low DSR: L.$TaxCut^{MPC}$	0.306	0.140	0.154	0.332	0.084	0.342**
No debt: L.$TaxCut^{MPC}$	0.241	0.219	0.177	0.119	0.456	0.128
p-value $H_0 : \beta_{1,1} = \beta_{2,1} = \beta_{3,1}$	0.731	0.595	0.915	0.752	0.433	0.666
Panel D: Age-adjusted income						
Low: L.$TaxCut^{MPC}$	0.364	−0.347	0.145	−0.269	−0.427	0.215
Middle: L.$TaxCut^{MPC}$	0.426*	0.139	−0.040	0.826*	0.002	−0.028
High: L.$TaxCut^{MPC}$	0.084	0.118	0.171**	0.090	0.169	0.288***
p-value $H_0 : \beta_{1,1} = \beta_{2,1} = \beta_{3,1}$	0.404	0.224	0.325	0.171	0.215	0.286
ΔP^s_{85-90} exposure	Less	Middle	More	Less	Middle	More
Category	ND	ND	ND	TEXP	TEXP	TEXP
Controls	✓	✓	✓	✓	✓	✓
N	4,664	11,280	9,000	4,664	11,280	9,000

Notes: Each column provides OLS point estimates for the coefficient on L.$TaxCut^{MPC}$ in a version of equation (4.4) where we interact $TaxCut^{MPC}$ and its lags with dummies indicating the liquidity group of the household and restrict to households located in areas with different exposure to the housing price cycle based on ΔP^s_{85-90}. For Panel A, sorting is by terciles of age of the household head. For Panel B, we sort by renters, homeowners with a mortgage, and homeowners with no mortgage. In Panel C, we sort by households with a below median DSR, those with an above median DSR, and those with no outstanding debt. Finally, Panel D sorts households according to income residualized on a polynomial in age. Robust standard errors clustered by household. The p-values correspond to F-tests for equality of the coefficients on the interaction terms. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Generally, we find the ranking of prefectures is very stable across each of the definitions (boom vs. bust and CPZ vs. non-CPZ), with few prefectures switching between terciles of the shock.

B.4 IV Method to Address Measurement Error

Although we find statistically significant MPCs out of the stimulus tax rebate in our baseline specifications, our estimates feature large standard errors. One concern is that our measure of the tax rebate variable $TaxCut$ is subject to measurement error since, as outlined in Section 3.2, we impute payments using a household's history of reported national and local income tax payments. As noted in Stephens & Unayama (2015; 2019) and Koijen et al. (2015), among others, self-reported measures of income and consumption may be subject to substantial misreporting.

Here we show that our main finding of a U-shape in the MPC out of the tax rebate holds when we use our $TaxCut$ variable as an instrument for month-on-month changes in household disposable income. The idea behind this approach is that our baseline estimates identify the reduced-form effect of the imputable portion of the tax rebate, whereas what we really care about is the effect of the anticipated shock to disposable income on expenditures. Using our imputed tax rebate as an instrument allows us to identify the MPC of "complier" households for which the rebate had a measurable effect on disposable income in July 1994.

In particular, in columns 1 through 3 of Table B.8, we estimate the following model:

$$\Delta \log C_{i,t} = \beta^{2nd} \cdot \Delta DispIncome^{MPC}_{i,t-1} + X'_{i,t} \cdot \gamma + \epsilon_{i,t} \tag{B.1}$$

$$\Delta DispIncome^{MPC}_{i,t-1} = \beta^{1st} \cdot TaxCut^{MPC}_{i,t-1} + X'_{i,t} \cdot \psi + \eta_{i,t} \tag{B.2}$$

$$cov\Big(TaxCut^{MPC}_{i,t-1}, \epsilon_{i,t}\Big) = 0 \tag{B.3}$$

where $TaxCut^{MPC}$ is defined as in equation (4.3). The exclusion restriction in (B.3) says that $TaxCut^{MPC}$ only influences expenditures through its effect on disposable income, which is defined as total income less the sum of national and local income taxes and Social Security premia. This assumption is likely to be valid given that the June-July tax rebate was not accompanied by any other reform, and that the rebate amount was tied to tax liability based on the preceding year's income (see Appendix A for details).

We also provide estimates in columns 4 through 6 of Table B.8 for a just-identified instrumental variables version of our main specification in (4.4), which captures heterogeneous spending responses by housing cycle exposure. To do so, we instrument a vector $D_{i,t-1}$ of variables pertaining to first differences of household disposable income with a vector $Z_{i,t-1}$ which contains analogous variables related to the tax rebate these same households received. More concretely, this model can be described by the following equations:

Table B.7. Ranking of Prefectures by Definition of Housing Price Shock

Rank:	All plots + boom	CPZ plots + boom	All plots + bust	CPZ plots + bust
More-affected				
Osaka	1	1	1	1
Chiba	2	2	4	4
Tokyo	3	4	2	3
Kyoto	4	3	3	2
Saitama	5	7	7	7
Nara	6	5	5	5
Kanagawa	7	9	8	9
Hyogo	8	6	6	6
Shiga	9	8	9	8
Aichi	10	11	10	11
Shizuoka	11	12	12	12
Yamanashi	12	10	40	25
Gunma	13	13	33	27
Tochigi	14	15	45	40
Okayama	15	17	46	44
Gifu	16	16	32	33
Middle-affected				
Wakayama	17	14	11	10
Ibaraki	18	20	18	17
Mie	19	18	39	35
Hiroshima	20	19	13	13
Kagawa	21	21	21	14
Miyagi	22	22	15	15
Ishikawa	23	23	25	30
Fukui	24	25	47	45
Fukuoka	25	27	28	26
Nagano	26	26	44	39
Okinawa	27	28	38	36
Fukushima	28	24	31	34
Toyama	29	32	30	29
Niigata	30	29	37	38
Ehime	31	30	20	19
Hokkaido	32	31	14	16
Least-affected				
Kumamoto	33	34	29	31
Yamaguchi	34	36	42	41
Tottori	35	33	41	47
Tokushima	36	35	43	46
Oita	37	47	34	37
Nagasaki	38	38	35	43
Shimane	39	40	27	23
Saga	40	39	36	42
Miyazaki	41	38	22	21
Yamagata	42	42	26	25
Iwate	43	43	23	24
Akita	44	44	24	28
Kagoshima	45	45	17	20
Kochi	46	47	19	22
Aomori	47	46	16	18

$$\Delta \log C_{i,t} = D'_{i,t-1} \cdot \beta^{2nd} + X'_{i,t} \cdot \gamma + \epsilon_{i,t} \tag{B.4}$$

$$D_{i,t-1} = \beta^{1st} \odot Z_{i,t-1} + X'_{i,t} \cdot \psi + \eta_{i,t} \tag{B.5}$$

$$cov\Big(Z_{i,t-1}, \epsilon_{i,t}\Big) = 0 \tag{B.6}$$

where the vectors $D_{i,t-1}$ and $Z_{i,t-1}$ are defined as:

$$D'_{i,t-1} := \Big\{\Delta DispIncome^{MPC}_{i,t-1}, \Delta DispIncome^{MPC}_{i,t-1} \times \mathbb{1}\{i \in \ell\} \text{ for } \ell = 1,2,3\Big\} \tag{B.7}$$

$$Z'_{i,t-1} := \Big\{TaxCut^{MPC}_{i,t-1}, TaxCut^{MPC}_{i,t-1} \times \mathbb{1}\{i \in \ell\} \text{ for } \ell = 1,2,3\Big\} \tag{B.8}$$

Table B.8. MPC out of Disposable Income Using Tax Rebates as an IV

	(1)	(2)	(3)	(4)	(5)	(6)
L.$\Delta DispIncome^{MPC}$	0.157*	0.146**	0.129*			
	(0.086)	(0.071)	(0.075)			
Less: L.$\Delta DispIncome^{MPC}$				0.142**	0.122**	0.091**
				(0.062)	(0.050)	(0.044)
Middle: L.$\Delta DispIncome^{MPC}$				0.042	0.049	0.32
				(0.049)	(0.035)	(0.028)
More: L.$\Delta DispIncome^{MPC}$				0.081**	0.060**	0.047*
				(0.038)	(0.027)	(0.025)
Category	TEXP	ND	SND	TEXP	ND	SND
Estimation	IV	IV	IV	IV	IV	IV
Controls	✓	✓	✓	✓	✓	✓
p-value $H_0 : \beta_{1,1} = \beta_{2,1}$	–	–	–	0.036	0.062	0.081
p-value $H_0 : \beta_{1,1} = \beta_{3,1}$	–	–	–	0.192	0.110	0.204
p-value $H_0 : \beta_{1,1} = \beta_{2,1} = \beta_{3,1}$	–	–	–	0.086	0.174	0.182
First stage F-test (cluster-robust)	80.67	80.67	80.67	21.27	21.27	21.27
First stage F-test (Cragg-Donald)	308.15	308.15	308.15	82.54	82.54	82.54
N	123,920	123,920	123,920	123,920	123,920	123,920
Adj. R^2	0.104	0.090	0.066	0.054	0.086	0.077

Notes: The first three columns show MPC estimates from estimating equation (B.1) using disposable income as an IV, where L.$\Delta DispIncome^{MPC}$ is the response to the tax rebate in July 1994. The last three columns show MPC estimates from estimating equation (B.4) using the interactions of $L.TaxCut$ with terciles of the housing price shock. We use the survey sampling weights for each household to be consistent with our baseline estimates. The p-values for the last three columns correspond to F-tests for equality of the coefficients on the interaction terms in (B.4). Standard errors in parentheses and first stage F-stats are clustered by household. $***p < 0.01, **p < 0.05, *p < 0.1$

The first three columns of Table B.8 show that our estimates of the MPC out of disposable income when we use the July tax rebates as an instrument are similar to our baseline estimates in Table 3. The average household spent 15% of the change in their July income due to the rebate on non-durables, compared to 11% when we instead estimate the reduced form regression of spending on the tax rebate. With a cluster-robust first stage F-stat of 80.67, the model in (B.1) is not subject to a weak IV problem. Looking at heterogeneity based on exposure to the housing cycle in the last three columns, for each of our three expenditure measures, we find an MPC out of disposable income due to the rebate that is almost twice as large in the less affected areas compared to the response among households in the more affected areas.

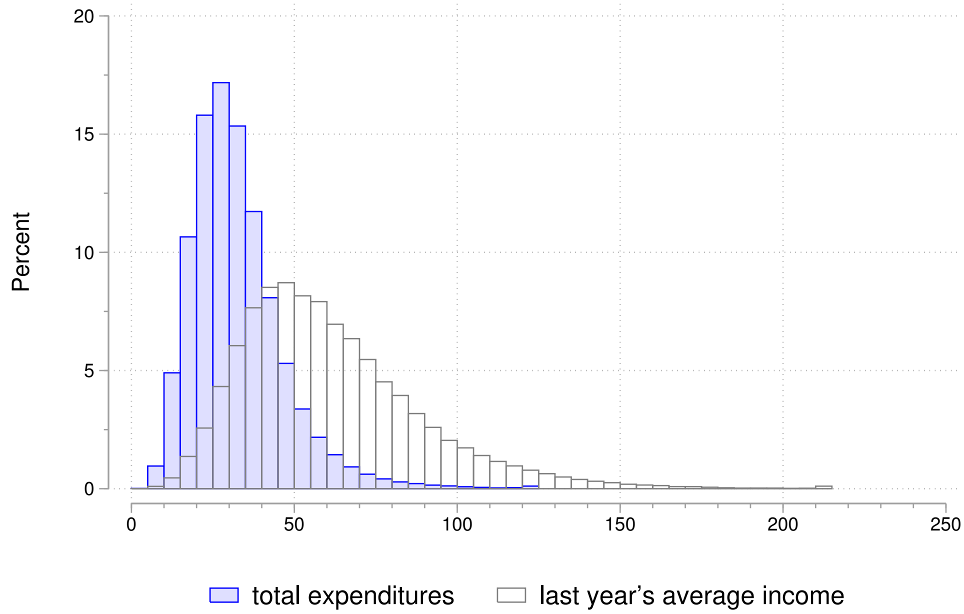
B.5 Choice of Permanent Income Proxy

Kueng (2015; 2018) raises the issue that in expenditure surveys such as the Consumer Expenditure Survey (CE) in the U.S., estimating reduced-form versions of the consumption Euler equation may be subject to substantial measurement error arising from the choice of proxy for permanent income used to normalize cash flows. While in the CE there is substantial under-reporting of income leading to "bunching" at near-zero levels of household income, we show in Figure B.1 that in the FIES there is no such bunching. Rather, due to the diary nature of the survey, there is substantial under-reporting of durable goods purchases, which leads to bunching at low levels of total expenditures over the panel (Unayama 2018).

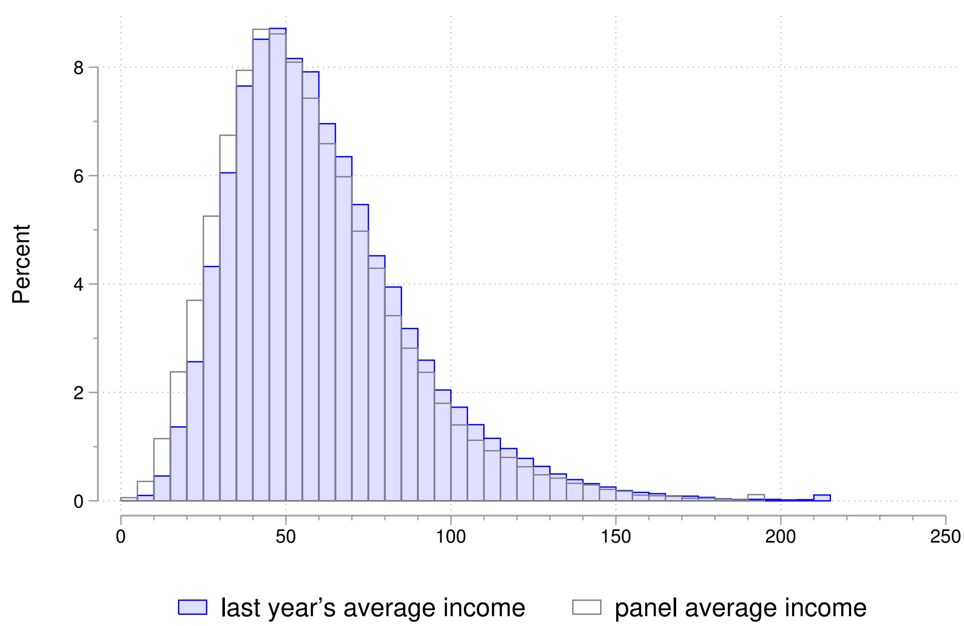
Throughout the paper, we use average monthly income based on total household income in the year prior to the survey divided by 12, which is a value confirmed by the interviewer and therefore not subject to imperfect recall by the household. We obtain similar results when we instead use average monthly income reported over the panel, since the distributions are quite similar for these measures, as shown in Panel B of Figure B.1. Our estimates are attenuated towards zero and much less precise when we instead normalize the tax cut variable by total expenditures.

FIGURE B.1. Distribution of Permanent Income Proxies

A. Total Expenditures vs. Last Year's Average Monthly Income



B. Last Year's Average Monthly Income vs. Panel Average Income



Notes: The figure compares the distributions of three measures of permanent income proposed in the literature on estimating versions of the consumption Euler equation: average total expenditures over the panel, average monthly pre-tax income in the preceding year, and average monthly pre-tax income over the panel. The x-axis units are 10,000 JPY ($\approx$ 100 USD) in real 2010 currency, so 50 corresponds to 500,000 yen or roughly 5,000 dollars in monthly income or expenditures.

B.6 Robustness to Least Absolute Deviation Regressions

In this sub-appendix we show the robustness of our main results to running least absolute deviation versions of our baseline regressions in equation (4.2) and (4.4). These regressions estimate the conditional median response of expenditures to the June-July tax rebate receipt. We perform this exercise to demonstrate that our finding of heterogeneity in MPCs by household exposure to the housing cycle is not driven by a small number of disproportionately large tax rebates relative to household income. In Table B.9 we find a median expenditure response in the pooled sample that is similar to the average response, with a July MPC for non-durables of 0.09 compared to 0.11 in Table 3. When we examine heterogeneity by terciles of the housing price shock, we find a similar U-shape in MPCs for total expenditures and strict non-durables, with overall lower MPCs within each subgroup of households (Table B.10). However, due to the larger standard errors, we cannot reject the null of no difference in the MPCs across each of the subgroup pairings.

Table B.9. LAD Estimates of Baseline Expenditure Responses

	(1)	(2)	(3)	(4)	(5)	(6)
$TaxCut^{MPC}$	−0.027	−0.007	−0.047			
	(0.040)	(0.046)	(0.040)			
L.$TaxCut^{MPC}$		0.092**	0.077**	0.092**	0.070***	0.136***
		(0.036)	(0.035)	(0.037)	(0.022)	(0.049)
L2.$TaxCut^{MPC}$			−0.046			
			(0.058)			
Category	ND	ND	ND	ND	SND	TEXP
Controls	✓	✓	✓	✓	✓	✓
N	154,900	123,920	92,940	123,920	123,920	123,920
Adj. R^2	0.146	0.149	0.150	0.149	0.128	0.101

Notes: Each column presents MPC estimates from an LAD version of equation (4.2) using non-durables (ND), strict non-durables (SND), or total expenditures (TEXP) as the measure of consumption. We weight observations using the survey sampling weights for each household. $TaxCut^{MPC}$ corresponds to the response to the tax rebate in June 1994, L.$TaxCut^{MPC}$ is the July 1994 response, and L2.$TaxCut^{MPC}$ is the August 1994 response. These specifications include all survey panels, not just the March and April rotation groups. Robust standard errors clustered by household in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

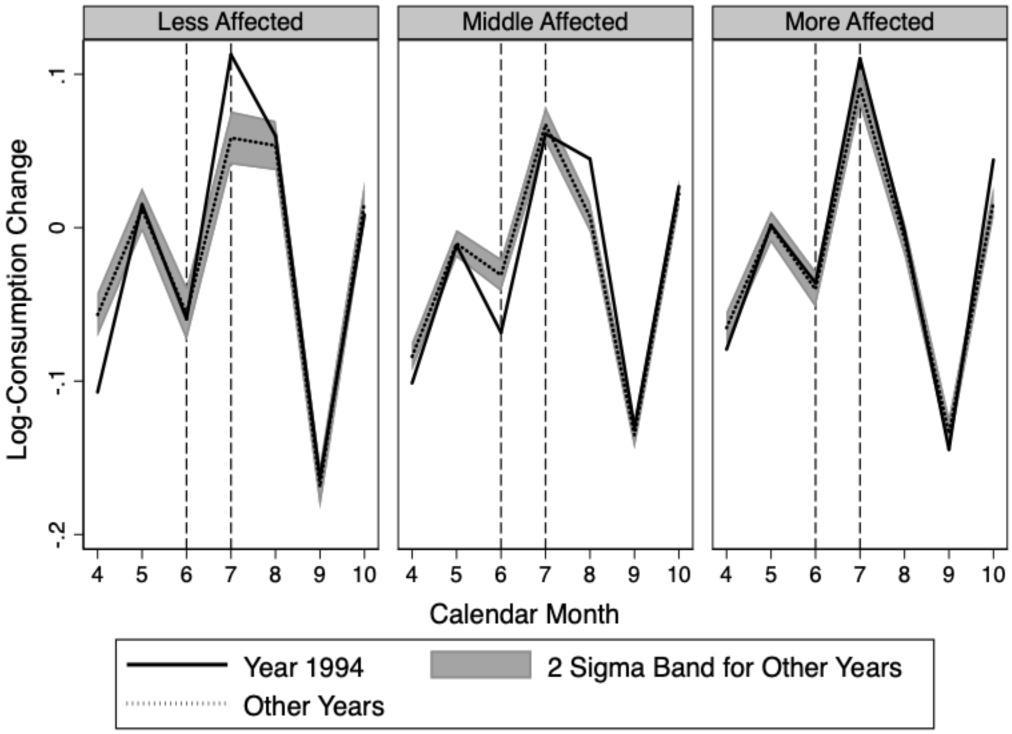
Table B.10. LAD Estimates of MPCs by Local Housing Market Exposure

	(1)	(2)	(3)	(4)	(5)	(6)
Less: L.$TaxCut^{MPC}$	0.226	0.265**	0.168**	0.242***		
	(0.148)	(0.126)	(0.077)	(0.089)		
Middle: L.$TaxCut^{MPC}$	0.054	0.082**	0.040		0.091**	
	(0.061)	(0.039)	(0.039)		(0.042)	
More: L.$TaxCut^{MPC}$	0.190**	0.083	0.079*			0.043
	(0.087)	(0.063)	(0.041)			(0.066)
Category	TEXP	ND	SND	ND	ND	ND
Controls	✓	✓	✓	✓	✓	✓
p-value $H_0 : \beta_{1,1} = \beta_{2,1}$	0.301	0.137	0.137	–	–	–
p-value $H_0 : \beta_{1,1} = \beta_{3,1}$	0.833	0.196	0.296	–	–	–
p-value $H_0 : \beta_{1,1} = \beta_{2,1} = \beta_{3,1}$	0.303	0.331	0.327	–	–	–
N	123,920	123,920	123,920	22,996	56,688	44,236
Adj. R^2	0.101	0.149	0.128	0.172	0.155	0.129

Notes: Each column presents MPC estimates from an LAD version of equation (4.4) using non-durables (ND), strict non-durables (SND), or total expenditures (TEXP) as the measure of consumption. We weight observations using the survey sampling weights for each household. $TaxCut^{MPC}$ corresponds to the response to the tax rebate in June 1994, L.$TaxCut^{MPC}$ is the July 1994 response, and L2.$TaxCut^{MPC}$ is the August 1994 response. These specifications include all survey panels, not just the March and April rotation groups. Robust standard errors clustered by household in parentheses. The p-values for the first three columns correspond to F-tests for equality of the coefficients on the interaction terms in (4.4). $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

B.7 Triple Differences for Strict non-durable expenditures

FIGURE B.2. Strict Non-durable Spending Responses by Housing Price Exposure



C A Simple OLG Model of Boom-Bust Cycles

In this appendix we present a stylized overlapping generations (OLG) model with renters and homeowners who face downpayment constraints to illustrate how heterogeneity in exposure to the housing cycle leads to the expenditure responses to the stimulus tax rebate we see in the data. The model illustrates how households may differ in their exposure to a housing price cycle based on their homeownership status and age when a boom-bust cycle occurs in the housing market.

Suppose the economy can be characterized by a constant number of households born each period and that lives for five periods, with the population of each cohort normalized to unity. In every period, households earn labor income, which may vary exogenously across age and cohort. Households consume in the first four periods and leave a bequest b in the final period.

We assume households always buy a house in the second period (i.e. roughly when they are between 20 and 35 years old), and rent up until that point in the life-cycle. Households sell the house in the last period of their lives and receive a capital gain, but do not derive any utility purely from owning the house. House prices P_t follow an exogenous martingale process and households have rational expectations of future prices:

$$\mathbb{E}_t[P_{t+k}] = P_t \quad \text{for} \quad k = 1, 2, 3, \ldots \tag{C.1}$$

Starting in the first period of homeownership, the house depreciates at constant rate δ, yielding a sale value at time t of $(1-\delta) \cdot P_t$. Thus, households born in period t pay the life-time housing cost δP_{t+1} and receive a capital gain (loss) of $G^t = (1-\delta) \cdot (P_{t+3} - P_{t+1})$.

House purchases are fully financed by a zero interest-rate mortgage, and so no payment is required besides the downpayment d, which we assume is an increasing function of the price of the house, $d(P_t)$. Households also face liquidity constraints which require non-negative financial assets. The life-cycle utility for a cohort born in period t is:

$$U_t\Big(c_t^t, c_{t+1}^t, c_{t+2}^t, c_{t+3}^t, b^t\Big) = \sum_{k=0}^{3} u(c_{t+k}^t) + \nu(b^t) \tag{C.2}$$

where we use the superscript to denote cohorts, and the subscript to denote the time dimension, so c_s^t is the consumption of cohort t in period s. For simplicity, we assume the felicity function $\nu(\cdot)$ of bequests is given by:

$$\nu(b^t) = (N-4) \cdot u\left(\frac{b^t}{N-4}\right) \tag{C.3}$$

where N is equal to the number of periods households live. $N = 5$ in our baseline case, but we characterize the results in terms of the parameter N to show how our results are generalizable to versions of the model with longer lives.

In the first period, households born in t maximize their lifetime utility subject to a lifetime budget constraint which can be written as:

$$\mathbb{E}_t[\mathcal{Y}^t] + \mathbb{E}_t[G^t] = \sum_{k=0}^{3} c_{t+k}^t + \delta \mathbb{E}_t[P_{t+1}] \tag{C.4}$$

where $\mathcal{Y}_s^t$ is the expected lifetime total income of cohort t at period s, or $\mathcal{Y}_s^t \equiv \mathbb{E}_s\Big[\sum_{k=0}^{3} y_{t+k}^t\Big]$ where y is the labor income endowment. In addition to the budget constraint, optimal consumption must meet the liquidity and downpayment constraints:

$$\sum_{k=0}^{3} y_{t+k}^t - c_{t+k}^t \geq 0 \quad \text{for} \quad k = 1, 2, 3 \tag{C.5}$$

$$y_t^t - c_t^t + y_{t+1}^t - c_{t+1}^t \geq d(P_{t+1}) \tag{C.6}$$

We now characterize optimal household consumption paths in this framework under quadratic utility over consumption to help illustrate how households will face differential exposure to a housing cycle based on liquidity and downpayment constraints and based on their position in the life-cycle. If the liquidity and downpayment constraints are non-binding, then the optimal consumption path will be the optimal smoothed consumption level planned in the initial period:

$$c_{t+k}^{t} = c_{t}^{t*} = \frac{\mathcal{Y}_{t}^{t}}{N} \cdot \left(1 - \frac{\delta P_t}{\mathcal{Y}_{t}^{t}}\right) \quad \text{for} \quad k = 0, 1, 2, 3 \tag{C.7}$$

Due to the martingale property for prices, the expected capital gain (loss) is always zero and $\mathbb{E}_t[P_{t+1}] = P_t$.

In the second period ($t+1$), households may update their consumption plan based on new information about housing prices. Here we introduce the concept of near-rationality by assuming households face an attention cost $A > 0$, or a cost of recalculating the optimal consumption plan. If the gains from recalculating optimal consumption are above A, we assume that households adjust their consumption based on the current realized shock. Once households decide to recalculate, the new optimal consumption path will be:

$$c_{t+k}^{t*} = \frac{\mathcal{Y}_{t+1}^{t}}{N-1} \cdot \left(1 - \frac{\delta P_{t+1}}{\mathcal{Y}_{t+1}^{t}} - \frac{c_t^t}{\mathcal{Y}_{t+1}^{t}}\right) \quad \text{for} \quad k = 1, 2, 3 \tag{C.8}$$

On the other hand, if the gains from reoptimizing are small relative to the attention cost, households will adopt a rule of thumb consumption path characterized by:

$$\widehat{c}_{t+1,t+k}^{t} = c_t^{t*} + \left(y_{t+k}^{t} - \mathbb{E}_t[y_{t+k}^{t}]\right) \quad \text{for} \quad k = 1, 2, 3 \tag{C.9}$$

$$\widehat{b}_{t+1}^{t} = c_t^{t*} + \delta \cdot \left(P_{t+1} - \mathbb{E}_t[P_{t+3}]\right) \tag{C.10}$$

where $\widehat{c}_{t+1,t+k}^{t}$ is the suboptimal consumption of cohort t in period $t+k$ decided in period $t+1$, and $\widehat{b}_{t+1}^{t}$ is the suboptimal bequest planned as of period $t+1$. Hence, households will reoptimize in response to newly realized shocks whenever the following condition holds:

$$3u(c_{t+1}^{t*}) + (N-4) \cdot u\left(\frac{c_{t+1}^{t*}}{N-4}\right) - \sum_{k=1}^{3} u(\widehat{c}_{t+1,t+k}^{t}) + (N-4) \cdot u\left(\frac{\widehat{b}^{t}}{N-4}\right) > A \tag{C.11}$$

If there is no uncertainty in labor income, the actual change in consumption between period t and $t+1$ is

$$\Delta c_{t+1}^{t} = \frac{\mathcal{Y}_{t+1}^{t}}{N} \cdot \left(1 - \frac{\delta P_{t+1}}{\mathcal{Y}_{t+1}^{t}} + \frac{y_t^t - c_t^t}{\mathcal{Y}_{t+1}^{t}}\right) \tag{C.12}$$

$$- \frac{\delta \cdot (P_{t+1} - P_t)}{N-1} \tag{C.13}$$

This shows that consumption drops if housing prices increase, since such booms entail higher lifetime housing costs. However, once households buy a house in the second period, the housing price change does not affect lifetime housing costs but does affect expected capital gains or losses:

$$\Delta c_{t+2}^{t} = \frac{(1-\delta) \cdot \mathbb{E}_{t+2}[P_{t+3}] - P_{t+1}}{N-2} = \frac{(1-\delta) \cdot (P_{t+2} - P_{t+1})}{N-2} \tag{C.14}$$

$$\Delta c_{t+3}^{t} = \frac{(1-\delta) \cdot (P_{t+3} - P_{t+1})}{N-2} \tag{C.15}$$

Hence, an increase in the housing price is a positive shock to the consumption of homeowners, since they consume out of the anticipated capital gain.

Within this framework with non-binding constraints, a housing price drop at time T generates heterogeneous responses across cohorts. Younger cohorts who are not yet homeowners and were born in $T-1$ and T are eager to increase their consumption, while the older cohorts born in $T-3$ and $T-2$ have to decrease consumption since their anticipated capital gain will be lower.

Finally, we consider what happens when either the liquidity or downpayment constraints are binding. Given the age-income profile, higher optimal consumption means individuals are more likely to be liquidity constrained. For example, based on (C.7), the liquidity constraint in (C.5) will not bind when

$$\frac{1}{N} \cdot \left(1 - \frac{\delta P_t}{\mathcal{Y}_t^t}\right) > \frac{y_t^t}{\mathcal{Y}_t^t} \tag{C.16}$$

This simple condition indicates that the optimal consumption level of households who are not yet homeowners will be higher whenever the current housing price is lower.

Further, even if liquidity constraints do not bind, the downpayment constraint might bind. As shown above, with quadratic utility, consumption is proportional to the housing price. If the downpayment requirement $d(P_t)$ is concave in the price, households are more likely to become constrained when a larger drop in the housing price occurs.