Heterogeneous Wealth Effects[#]

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Abstract

We measure wealth effects on consumption using a novel research design: responses to direct survey questions asking how much a household would change consumption in response to unexpected (positive and negative) shocks to own home value. The average wealth effect is in the 2-5% range, in line with econometric estimates that associate changes in housing wealth with consumption realizations. However, our analysis uncovers significant heterogeneity. Extensive margin responses are limited: more than 90% of the sample reports no consumption adjustment to wealth shocks. On the other hand, conditioning on adjusting, intensive margin responses are substantial. Finally, the consumption response to positive wealth shocks is greater than the response to negative shocks, as suggested by the collateral channel mechanism of transmission between wealth shocks and consumption.

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1. Introduction

Housing is by far the most important asset in households' portfolios. As a result, the considerable volatility in house prices that many economies have recently experienced should have important implications for household spending. Spending responses are also likely to be heterogeneous across households and might differ between housing booms and busts.

Recent literature uses direct survey questions to elicit information about the consumption responses to scenarios involving positive and negative transitory income shocks (see, e.g., Christelis et al., 2019; Fuster et al., 2020). The approach is useful, as it provides household-specific estimates of the marginal propensity to consume (MPC) out of possible income changes. This allows associating MPC responses of each household with observables such as household resources and comparing MPCs out of income shocks of different sign.

The present paper builds on this literature using direct survey questions that allow estimating the impact of housing wealth shocks on households' expenditures (or "wealth effect", in short). It makes four contributions. It takes advantage of the first (to the best of our knowledge) population-representative household survey providing consumption responses to alternative scenarios of idiosyncratic housing *wealth* shocks. Most of the literature so far has focused on the consumption effect of income shocks and ignored wealth shocks.[1] Besides being the largest asset in household portfolios, housing is also illiquid and infrequently traded and has both a consumption and an investment component. Thus, it is instructive to directly measure homeowners' consumption response to possible shocks to the value of their own house by explicitly asking about it. This can be useful for estimating the consumer spending drop due to a housing bust, an important policy exercise considering the recent Great Recession.

Our second contribution is more conceptual. In principle, a wealth effect should measure the response of consumption to exogenous changes in asset prices However, in practice, consumption may spuriously respond to endogenous changes in home value (i.e., households buy new furniture when renovating their homes).[2] In addition, behavioral traits may correlate with housing values reported in surveys making hard to identify the effect of a wealth shock

[1] See Shapiro and Slemrod (1995; 2003); Jappelli and Pistaferri (2014); Graziani et al. (2016); Christelis et al. (2019); Fuster et al. (2020).

[2] Carroll et al. (2011) point out that identifying the "pure" housing wealth effect is hard, because "one would want data on spending by individual households before and after some truly exogenous change in their house values, caused for example by the unexpected discovery of neighbourhood sources of pollution." A similar problem arises in the analysis of the link between consumption and stock market wealth. Dynan and Maki (2001) distinguish changes in wealth that directly cause changes in consumption through their effect on households' contemporaneous budget sets, from situations in which changes in wealth predict changes in consumption because they signal changes in future income.

on spending. For example, individuals with optimistic economic outlooks are likely to report larger (smaller) increases (declines) in their housing values due to a given shock, compared to pessimistic individuals. More generally, a multitude of factors can contribute to the observed co-movements of spending and home prices. By using direct survey questions asking about house price *shocks*, we bypass the problem of distinguishing between exogenous (i.e., house price changes due to local housing conditions or labor market shocks) and endogenous (i.e., renovations, unit splits, etc.) movements in home values which plague most of the empirical literature using realized consumption and house value data. Moreover, given that all respondents in our survey face the same shock scenarios, the shocks are, by design, orthogonal to household behavioral traits and other unobserved characteristics.[3]

The third contribution is an analysis of heterogeneous wealth effects, as our approach elicits consumption responses to housing wealth shocks that are household-specific. We associate such heterogeneity in consumption responses to cash-on-hand, loan-to-value ratio (LTV), age and other demographic variables, and examine whether these associations are consistent with existing models of consumption behavior. Hence, we shed some light on the channels through which unanticipated housing wealth shocks induce consumption adjustments.

Finally, we test for asymmetric responses to positive versus negative housing price shocks. As is the case with income shocks, it is instructive to investigate whether consumption responds asymmetrically to wealth shocks of different signs. As we discuss, this asymmetry can provide insights about the role of liquidity constraints and about the impact of policies affecting house values (such as changes in the maximum allowed LTV ratio or property taxes).

We design a special module for a representative survey of Dutch households. The module includes questions on how respondents would change their consumption in response to an unexpected and permanent increase or decrease in their housing wealth corresponding to 10% of their current house value. As discussed in detail below, we conduct several validation checks to assess the quality of our special-purpose survey. The survey shows that among homeowners the average wealth effect is 4.7% for a housing price increase and 2.1% for a housing price decline. These values are broadly in line with econometric estimates that use actual housing wealth and consumption data.

[3] In the robustness section, we discuss extensive evidence on this, as responses to our survey questions are invariant to a multitude of factors, including individuals' general optimism, understanding of the survey questions and expectations about future house price shocks (aggregate or idiosyncratic).

The key empirical finding made possible by our research design is that there is significant heterogeneity in wealth effects, with over 90% of homeowners reporting no reaction to either positive or negative shocks. To the best of our knowledge, this is a novel finding in the literature on the consumption response to housing wealth shocks. For households who do react to the wealth shock, an important question is how they finance the change in consumption, given that housing wealth is illiquid and indivisible. We find that reducing savings or using current income (in the case of a positive housing wealth shock) or increasing saving (in the case of a negative wealth shock) are the typical strategies followed by households.

In line with predictions of models with precautionary saving and liquidity constraints, we find a negative association between the wealth effect and cash-on-hand. This is supported both by descriptive and by more formal regression analyses. We also examine whether households with different levels of debt also differ in their consumption response to housing wealth shocks. We find evidence that homeowners with high LTV ratios are particularly responsive to negative shocks. This is, as we shall discuss, consistent with the institutional environment of the Netherlands, where underwater households are limited in their residential mobility choices and may face difficulties in getting additional credit.

Last, we find that the consumption response to positive wealth shocks is greater than the response to negative shocks. As pointed out by Andersen and Leth-Petersen (2020), an asymmetric wealth effect is consistent with a collateral channel mechanism. According to this, increases in home values allow additional borrowing and spending, while decreases in home values do not necessarily require households to reduce borrowing given that the constraint binds only at the time of loan origination.

The paper is organized as follows. Section 2 discusses the economic motivation and empirical evidence on the microeconomic estimates of the wealth effect. Section 3 describes the survey design and discusses several validation checks and survey features that lend credibility to our analysis. Section 4 discusses the descriptive analysis and the regression results relating the wealth effects of consumption to household socio-economic characteristics, along with various robustness checks. Section 5 concludes.

2. Research hypotheses

Most of the literature estimating the consumption effect of a change in house prices relies on two implicit assumptions.[4] First, consumers do not anticipate these wealth shocks. Second, current house prices are the best predictors of future ones; hence, consumers view any house value changes as highly persistent. Under these assumptions, the life-cycle permanent income model suggests that unexpected changes in housing wealth affect the consumption of homeowners by an amount equivalent to the annuity value of the shock.[5] On the other hand, the value of the housing services homeowners consume increases commensurately, and thus it is not clear to what extent the consumption of other goods and services should be affected by the increase in house value.

In addition to this "pure" wealth effect, a change in house prices can affect consumption through other channels, introducing heterogeneity in the wealth effect. Furthermore, the wealth effect might be asymmetric, in response to shocks of different sign. We discuss in turn sources of possible *heterogeneity* and *asymmetries* of the wealth effects.

As noted by Campbell and Cocco (2007), age is a first source of heterogeneity. Most young households plan to increase house size later in life, while many old households plan to move to a smaller house. One should therefore expect to observe older homeowning households increasing their consumption when house prices rise. Using microdata merged with regional house prices, Campbell and Cocco (2007) estimate the largest effect of house prices on consumption for older homeowners. For the same reason, households who plan to leave the house as a bequest, and therefore less likely to sell the house, are less likely to change consumption in reaction to house price shocks. A first research hypothesis is that older households, households with plans to move to different dwellings, and households who do not plan to leave bequests, should exhibit larger wealth effects.

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[4] We focus here only on shocks to housing wealth, which may be hard to monetize due to the illiquidity and indivisibility of the asset. In contrast, shocks arising from changes in the values of financial assets are more easily converted into cash.

[5] To see this point, consider the consumption rule of an individual with an infinite horizon in a standard life-cycle model with certainty equivalence and constant interest rate equal to the rate of time preference. Consumption is just a proportion of initial wealth plus human wealth:

$$c_t = \frac{r}{1+r} a_t + E_t\left(\frac{r}{1+r} y_t + \frac{r}{(1+r)^2} y_{t+1} + \cdots\right)$$

A transitory and unexpected increase in a_t is akin to an experiment in which the individual learns that the price of a_t increases in period *t* and will drop in the future. Instead, a permanent and unexpected increase in a_t can be interpreted as an experiment in which the individual learns that the price of a_t increases in period *t* and will stay at this higher level forever. The increase in consumption is proportional to $\frac{r}{1+r}$, exactly as in the case in which there is a transitory shock in income in period *t*.

Liquidity and precautionary saving are additional sources of heterogeneity, especially because housing wealth can be used as collateral. When hit by a negative shock, households with low cash-on-hand are more likely to reduce consumption than those with high cash-on-hand, given that the latter can draw on accumulated savings. Moreover, a drop in the house value should make it more difficult to borrow to smooth consumption using the house as collateral, thus worsening the liquidity problem of those with low cash-on-hand. One should therefore expect a negative correlation between cash-on-hand and the wealth effect on consumption arising from negative wealth shocks.

A positive wealth shock, however, makes households with high cash-on-hand increase their consumption by drawing on existing savings, while cash-poor consumers can increase consumption only by borrowing (using the appreciated house as collateral) or by selling the house outright (to lock-in their capital gain). In other words, the positive wealth shock likely alleviates borrowing constraints of cash-poor households. This should in turn favor pent-up consumption, as it can enable these households to increase their spending by an amount large enough to match what they would have spent under no borrowing constraints. Hence, the relation between the wealth effect and cash-on-hand from positive wealth shocks is ambiguous, and it depends on whether the positive shock allows households with low cash-on-hand to borrow or sell the house.

The above imply that households with low cash on hand exhibit a stronger consumption response to both positive and negative housing wealth shocks. Hence, our second research hypothesis is that individuals with low cash-on-hand are more likely to adjust consumption in response to house price shocks.

Turning now to credit constraints, Andersen and Leth-Petersen (2020) point out that wealth shocks of different sign might affect consumption differently. Furthermore, these potential asymmetries can help disentangle the pure wealth effect from the collateral channel. For unconstrained households, the response should be symmetric for positive and negative price changes. For constrained households, the increase in home values offers the possibility for additional borrowing that can be used to finance spending. However, a decrease in home values does not necessarily require households to reduce borrowing, because the constraint binds at the time of loan origination. A third research hypothesis is that the collateral effect implies a stronger response of consumption to positive wealth shocks than to negative ones.

Given the different links between house prices and consumption, it is perhaps not surprising that from an empirical point of view it is difficult to pin down the wealth effect and that there is considerable disagreement over its magnitude. In the U.K., Disney et al. (2010)

find a low wealth effect (of the order of 1 percent), whereas Campbell and Cocco (2007) report a strong response for older owners. Attanasio et al. (2009) conclude instead that the co-movements in U.K. consumption and house prices reflect not a causal link but the action of common factors, contradicting the findings of Campbell and Cocco (2007).

In an influential paper, Mian et al. (2013) find a housing wealth effect in the U.S. in the order of 5–7 percent, and that areas with poorer and more levered households display a significantly higher wealth effect. Christelis et al. (2015) examine the effects of the recent crises in the US housing market on household spending, using micro data from the 2009 Internet survey of the Health and Retirement Study. They focus on negative and exogenous wealth changes and find a housing wealth effect of only 1 percent. They also find that consumption responds more strongly to permanent shocks than to shocks perceived as transitory. Aladangady (2017) finds a wealth effect of 4.7 cents on the dollar for homeowners, but a negligible response for renters, see also Contreras and Nichols (2010) for a comprehensive survey of the literature.

Some papers find larger responses among credit constrained households, suggesting that the loosening of borrowing constraints is a primary driver of the housing wealth effect. For instance, De Fusco (2018) estimates a marginal propensity to borrow out of housing collateral that ranges between 4 and 13% and is correlated with homeowners' initial leverage, suggesting a potentially important role for collateral constraints in driving household expenditures. On the other hand, Guren et al. (2021) estimate a housing wealth effect in the range of 3 to 6%, and find no evidence of a boom-bust asymmetry in the housing wealth elasticity. They show that these results are consistent with the behavior of the housing wealth elasticity in a standard life-cycle model, because in their sample low-leverage homeowners account for a substantial part of the aggregate housing wealth elasticity.

Estimates of the wealth effects for countries other than the UK or the US are more nuanced, possibly owing to institutional differences. Using a large panel of Danish households, Browning et al. (2013) find little evidence of a housing wealth effect. On the other hand, Zhang (2019), using a panel of Dutch households finds wealth effects in the order of 7 cents for owners, but negligible responses for renters.[6] Paiella and Pistaferri (2017) use the Italian Survey of Household Income and Wealth which contains data on subjective interest rate

[6] In the context of Dutch economy, Ji et al. (2019) explore the link between household debt and consumption. Using administrative data, they find that the consumption of highly indebted households decreased much more during the crisis than that of other households.

expectations to isolate exogenous changes in wealth (due only to asset price shocks) from anticipated changes (due to, for example, planned portfolio rebalancing). They estimate an overall wealth effect of about 3 cents per (unexpected) euro increase in wealth, primarily driven by changes in housing prices.[7] In a recent paper, Andersen and Leth-Petersen (2020) elicit subjective expectations of future home value for a sample of Danish households, and find that unanticipated increase in home values lead to an increase in mortgage debt and spending of 3-5 percent of the unanticipated gain, but no effect from negative shocks, supporting the view that the wealth effect operates primarily through the collateral channel.

In sum, the review of the literature points to several research hypotheses and methodological key issues that new research should try to gain insights into. Regarding the research hypotheses, one should expect the wealth effect to be stronger for older households, individuals with plans to move to different dwellings, individuals who do not plan to leave bequests, and individuals with low cash-on-hand. Furthermore, the collateral channel suggests that the response of consumption to positive wealth shocks should be stronger than the response to negative ones.

From the methodological point of view, when estimating the wealth effect, applied research should address the issue that movements in asset prices are likely to be correlated with other shocks that may have a separate effect on consumption. Second, it is instructive to differentiate between *anticipated* and *unanticipated* changes in wealth (see Jappelli and Pistaferri, 2011, 2017 for a survey of the literature). Third, it is important to distinguish between *positive* and *negative* wealth shocks, as the possible asymmetric response with respect to these shocks can shed light on the relevance of the collateral channel. Fourth, one should expect that the wealth effect is heterogeneous, and therefore examine how it varies across different demographic groups.

One limitation of the existing studies is that the estimated wealth effect on consumption is an average of individual responses, which misses the possibility of response heterogeneity. Moreover, in real life households experience either a positive or a negative wealth shock. As a result, estimates of positive or negative wealth effects are hard to generalize as they depend, to a certain extent, on the characteristics of the selected sub-group of households that has experienced a given type of shock.

[7] Contrary to the predictions from the theory, they find that the consumption response to anticipated changes in wealth is also large and significant, of the same magnitude as the response to unanticipated changes. They also find evidence that the wealth effect from negative price shocks is larger than that from positive shocks, consistent with the presence of liquidity constraints.

Against this background, for the first time in the related literature, instead of estimating an average household response to house price shocks, our survey records an unanticipated 10% increase or decrease in one's home value. Therefore, we do not face the challenge of trying to disentangle the response of spending to the wealth shock from responses to other shocks, nor do we rely on selected household groups that have experienced only one type of shock (that is, either positive or negative). We are thus able to elicit a household-specific wealth effect on consumption out of positive and negative housing wealth shocks. Our measure can also be compared with aggregate estimates, used to test for possible asymmetric responses to shocks, and related to household resources, indebtedness and demographic variables.

Our research design builds on the contributions of Shapiro and Slemrod (1995, 2003), who pioneered the use of survey data to elicit qualitative responses to income shocks. Jappelli and Pistaferri (2014, 2020) refined the approach, considering quantitative responses to income shock scenarios, and providing quantitative estimates of the MPC from income shocks. In some recent contributions, Christelis et al. (2019) and Fuster et al. (2020) distinguish between positive and negative income shocks of different size. More generally, the paper fits in the broad literature advocating the use of subjective perceptions and expectations in macroeconomics (for a survey, see Manski, 2017).

3. Survey design

3.1 The data

We use data from the CentER Internet panel, a project sponsored by the Dutch National Bank and maintained by CentERdata at Tilburg University. The baseline survey, known as the DNB Household Survey (DHS), is conducted once a year via the Internet and collects detailed information on a range of demographics and asset holdings for a representative sample of Dutch-speaking households in the Netherlands. In addition to the baseline survey, households may be asked, during the year, to participate in special purpose surveys. Response rates for the baseline and the special purpose survey are 72% and 77%, respectively.

We administered a special purpose survey in September 2018 to all adult members of the CentER Internet panel. Household panel members are randomly drawn from Dutch population registers in cooperation with Statistics Netherlands. At the end of a recruitment interview with questions on demographic and non-demographic topics, respondents are asked whether they are willing to participate in a longitudinal panel. These recruitment interviews always take place either face-to-face or over the phone. Importantly, only after respondents express their willingness to participate in the panel, it is explained that the panel answers questions via the

Internet. In case respondents do not have access to the internet they are provided with access to the Internet or alternative equipment. They may receive a demonstration how this works and there is free telephone access to a help desk for any questions of difficulties they may have. While nowadays there is almost full Internet coverage in the Netherlands, in previous years this unique feature of the recruitment process appeared essential for the representativeness of the panel. The annual panel attrition is about 15%. These households are replaced with new households with similar characteristics which are drawn from the group who stated their willingness to participate.[8]

The survey is a cross-section of 1,264 homeowners. It also merges information from the CentER Internet panel on several background characteristics (demographics, consumption, income, house value, financial wealth, and indebtedness) that are useful for our analysis.

To elicit the wealth effect, the survey first asks homeowners to provide an estimate of the current value of their house. It then asks them to report how they would change their consumption of durable and non-durable goods in the next 12 months in response to an unexpected change in the value of their house. The question explicitly refers to a one-off, permanent increase (or decrease) in this value. Given the significant heterogeneity of the home value (due to geography, type of unit, and other idiosyncratic factors), we present respondents with wealth changes (expressed in euro) that correspond to 10% of their (self-assessed) home value. By tailoring the shock to household-specific home values, we avoid biasing responses by the size of the shock.[9] Moreover, framing the question in a realistic context should increase the reliability of the consumption response. Finally, as the consumers provide a consumption change in euro, we can readily calculate an estimate of the wealth effect on consumption.

In more detail, we used the following question to elicit consumption responses to a positive wealth shock:

Suppose that the value of your house increases unexpectedly by YYY euro and that this increase in value of your house is ***permanent*** *(that is, it lasts forever). In the next 12 months, how would you use this* ***increase in the value of your house****? (You can add a euro amount to item 1, item 2, or both).*

[8] The overview by Teppa and Vis (2012) provides more information on the history of the panel, recruitment and sampling procedures and documents the representativeness on important demographic variables. Surveys in this household panel have been used for academic research extensively; some examples include Bellemare and Kröger (2007), van Rooij et al. (2011), Von Gaudecker (2015), Deuflhard et al. (2019).

[9] Presenting, instead, respondents with a fixed euro change in wealth would imply a very large (small) shock for households with relatively low (high) house value.

1. *Purchase non-durable goods and services (food, clothes, travel, vacation, entertainment, cosmetics, etc.) that you otherwise would not have purchased, namely an amount of XXX euro;*
2. *Purchase durable goods (cars, home improvement, furniture, jewels, etc.) that you otherwise would not have purchased or that you would have purchased later, namely an amount of XXX euro;*
3. *Would not spend more on durable and non-durable goods and services than you otherwise would do;*
4. *I don't know.*

The question for a negative wealth shock has been phrased symmetrically:

Imagine that the value of your house declines unexpectedly by YYY euro and that this reduction in value of your house is ***permanent*** *(that is, it lasts forever). In the next 12 months, how would you react to this* ***decrease in the value of your house****?*

1. *Reduce spending on non-durable goods and services (food, clothes, travel, vacation, entertainment, cosmetics, etc.), namely an amount of XXX euro;*
2. *Cancel or postpone for later the purchase of durable goods (cars, home improvement, furniture, jewels, etc.) that you otherwise would have purchased, namely an amount of XXX euro;*
3. *Would not spend less on durable and non-durable goods and services than you otherwise would do;*
4. *I don't know.*

An advantage of our survey is that the same respondent replies to the questions on positive and negative shocks. To minimize framing problems, we randomly split the homeowner sample into two, and ask the first (second) group the positive (negative) shock question. Two weeks later, we ask the first group the negative shock question and vice versa.

The survey includes the "don't know" option since forcing respondents to answer might induce them to drop out of the survey and/or to provide non reliable answers which would add noise in the data. This option is chosen by 15% of the sample, and the non-response is more prevalent among households with lower education and income. Given that the non-responses are not random, the potential selection bias might affect the results. To address this issue, we impute missing values, and discuss results in the robustness section.

Notice that the programming instructions restrict the total reported changes to be smaller than the change in the house value. Indeed, the survey program is designed to explicitly verify that the sum of the amounts (spending on non-durables, spending on durables, non-spending) does not exceed the hypothetical increase/decrease in home value and show an error message otherwise. The survey design also asks respondents to provide a single number for the value of the house, or if they fail to do so to choose an interval value. The house price shock (positive

or negative) is then computed as 10% of the reported house value if house value is available as a single number, or 10% of the midpoint of house value if house value is available in brackets.

In addition, we ask follow-up questions on how respondents would adjust consumption after a positive or negative shock. This allows distinguishing between the various possible channels through which housing wealth shocks might affect consumption.[10] Although the questions ask about consumption of non-durables and durables separately, for most of the analysis we combine the two answers into one.

Several features of the survey questions are noteworthy. First, all respondents reply to the same shock scenarios (scaled by the underlying housing value). Thus, the shocks are, by design, orthogonal to respondents' behavioral traits and other unobserved characteristics; as a result, we can interpret our estimates of the effects of the shocks as causal. In contrast, changes in housing values recorded in surveys may correlate with respondents' unobservable characteristics (e.g., optimism) making harder to identify the effect of the shock on consumption changes.

Second, the survey questions refer to idiosyncratic ("*the value of your house"*) rather than aggregate changes in housing prices. As Sinai and Souleles (2005) have pointed out, house price changes are generally correlated in the cross-section, and owning a home is a hedge against fluctuations in housing costs. The Sinai and Souleles (2005) channel implies that a household's response to a house price change should depend crucially on whether it is just the owner's house value that has changed (in which case the owner's real wealth is changing, as in our hypothetical question) or whether there is a boom or a bust, and all houses have changed in value at once (in which case real wealth is not necessarily changing).

Third, respondents report by how much they would increase or cut spending "in the next 12 months". A specific timeframe is important because it allows us to rule out that differences in the wealth effects on consumption arise from differences in the timing of planned spending. Of course, different adjustments in subsequent years cannot be ruled out.[11] In principle, it would be useful to ask similar questions with other time horizons (e.g., how would

[10] In the case of a positive shock, respondents are asked to indicate how they would finance the reported increase in spending by choosing at least one of the following options: (a) drawing from savings or using current income; (b) selling the house, to cash in the capital gain; (c) drawing funds from a (mortgage or home equity) loan, using the house as collateral; (d) other means. In case of a negative shock, respondents are asked to indicate how they would use the money that they subtract from spending by choosing at least one of the following options: (a) add to savings; (b) invest in home improvements; (c) pay-off some of their debts; (d) other uses.

[11] Carroll et al. (2011) show that the long-term effect of housing losses on consumption could be larger than the short-term one.

consumption change in the second or later years after the shock) but this would considerably increase the questionnaire complexity.

Fourth, the questions provide quantitative metrics for the proposed scenarios, as opposed to questions eliciting qualitative information ("mostly save/ mostly spend") on how people spend a windfall, such as tax rebates. Although business cycle effects can never be ruled out, the period in which the survey was administered (September 2018) was a "normal" one, with GDP growing steadily at about 2.5% on an annual basis. Furthermore, as discussed below, our results are not affected by respondents' expectations or uncertainty about future house prices.

Fifth, we focus on housing, as it is the largest component of wealth for most households. Moreover, direct survey questions do not easily distinguish changes in financial wealth due to market prices from changes due to active saving.[12]

Finally, note that a 10% permanent price change in the home value is non-trivial. In our sample, the mean (median) home value, among homeowners, is about 306,000 euro (250,000 euro). Our questions hence maximize the chance of observing a consumption response, as, due to possible adjustment costs, such a response is more likely to be observed when shocks are large. Indeed, it is possible that previous work has been unable to tightly estimate the wealth effect on consumption precisely because most shocks are small.

3.2 Validation checks and survey features

As discussed above, using a household survey to elicit individual-specific spending responses to housing wealth shock scenarios has several advantages (such as allowing for response heterogeneity and avoiding selection on the sign of the wealth shock). Nonetheless, it is instructive to examine the quality of the survey and the assumption that reactions to shock scenarios are correlated with actual behavior. In what follows, we discuss several validation checks and survey features that lend credibility to our analysis.

A first check, allowing comparison of our analysis with previous literature, is to use our sample to replicate a typical wealth effect regression, using panel data for 2017-18. For this purpose, we regress the growth rate of realized consumption on the growth rate of the home value in the past 12 months and on quartiles of lagged cash-on-hand.[13] Results are reported in Table 1, showing that the estimated wealth elasticity is 0.17 with a standard error of 0.08. The

[12] Household financial portfolios are also quite heterogeneous, implying that a change in stock prices might affect considerably stockholders, but leave non-stockholders indifferent. Moreover, the same stock market portfolio may itself being differently diversified across households.
[13] We recover the lagged value of cash-on-hand from the 2017 baseline DHS survey.

coefficients of other variables are not statistically different zero. Evaluated at the sample mean (median) of the ratio of home value to consumption, the implied wealth effect is 1.3% (1% at the median). These values are at the low-end of the range of wealth effect estimates obtained with macro and micro data for the U.S. and discussed in Section 2.

One weakness of this regression approach (besides endogeneity issues) is that it estimates an average effect. In the analysis below we show that the average of individual wealth effects elicited from the direct survey questions is in fact broadly aligned with the one estimated from the standard regression approach. However, as we shall see a similar mean masks non-negligible heterogeneity – with a large fraction of household not responding at all and a small fraction exhibiting very large responses to house price shocks.

Notice also that the wealth effect that one can estimate from hypothetical house price changes might be more reliable than wealth effects estimated from observational data, since in our experiment the shock to the home value is known with certainty and so is its persistence. In contrast, with actual home price changes households may not know in real time how large the shocks are, and they also may not know how persistent the shock is, making hard to separate empirically the causal wealth effect from the impact of incomplete information. Being able to specify these aspects explicitly is another strength of the survey approach.

One additional way to examine the validity of the directly elicited spending responses to our 10% unanticipated idiosyncratic shock questions is to check whether these spending responses are associated with households' expectations about local house prices. As mentioned, the fact that all respondents face the same shock scenarios implies that these shocks are not affected by individual traits, including own expectations. Also, these scenarios refer to the owner's house and not to local house prices. If such an association existed, then it could be the case that spending measures in our survey were not necessarily due to the 10% unanticipated shock questions on own house (i.e., the scenario presented to respondents) but rather to households' own assessment of the housing market's prospects. To check this, we ask in our special survey a set of questions that elicit, for each respondent, the first and second moments of the expected distribution of house prices in their neighborhood.[14] We find that neither expectations about average future house prices nor the underlying expected uncertainty about

[14] Respondents report the minimum and maximum values in the following question: "Imagine you own an apartment worth €100,000 in the neighbourhood you currently live in. What do you think will be the value of this apartment in 12 months, if you decide to sell it?" Using a simple triangular distribution, we calculate the mean and variance for each respondent. Guiso, Jappelli and Pistaferri (2002) first used this method to elicit individual-specific expectations and uncertainty about future income.

them correlate with our reported spending responses. We thus conclude that our spending measures indeed represent responses to the 10% unanticipated shock questions and are unlikely to be correlated with different outlooks about housing market prospects.[15]A third check is based on the theoretical prediction that individuals who adjust consumption due to permanent wealth shocks should be also more likely to adjust consumption in response to transitory income shocks (see Berger et al., 2018).[16] We obtain MPCs out of positive and negative transitory income shocks (equal to one month of household income) by asking our survey participants the same questions as in Christelis et al. (2019), who found that elicited MPCs are broadly in line with models of intertemporal choice with precautionary saving, borrowing constraints, and finite horizons. We find that there is indeed a strong positive correlation between the individual MPCs out of transitory income shock and the wealth effects from permanent wealth shocks.

Besides the above validation checks, there are several survey features that are worth mentioning and make the Netherlands a suitable country for our analysis. First, about two-thirds of the adult population are homeowners and thus can more easily conceptualize scenarios of appreciation or depreciation in housing values.[17]

Furthermore, homeowners likely have fairly accurate knowledge about changes in their own house value. Every year, municipalities assess the value of all houses falling under their property tax jurisdiction. Subsequently, municipalities send a notice of the assessed value to the homeowner for calculating the property tax. Importantly, this administrative value is updated every year based on housing transactions in the same area, giving most weight to nearby transactions.[18] Additionally, households must report the assessed home value in their income tax form to calculate the imputed rent tax.

In principle, one could also check if the tax value aligns with the stated value of the home. However, administrative data in the Netherlands become accessible with several years of delay; moreover, it is not obvious that survey respondents would authorize linking their responses to

[15] The same conclusion holds when we control for house price expectations at the national level that have a smaller idiosyncratic component compared to those regarding own neighbourhood. Since we cannot completely rule out that some individuals might interpret the home value changes as just pertaining to their house (as intended in the survey question), while others might interpret it as a broader effect in the market, in the robustness analysis we introduce separately expectations of general house price changes (asked in the baseline DNB survey few months before our special purpose survey).

[16] They show that consumption responses to permanent house price shocks can be approximated by the marginal propensity to consume out of temporary income times the value of housing.

[17] This is partly due to the large volatility in house prices of the last decade: a housing bust in 2008-13 (when house prices declined by more than 20%) was followed by a (still ongoing) housing boom.

[18] Likewise, mortgagors are typically aware of their outstanding mortgage amount, not only due to bank statements but also by having to report the outstanding mortgage amount in their tax declaration.

administrative data. Nevertheless, it is worth stressing that the average home value in our survey data is quite close to the average selling price based on actual house transactions as published by Statistics Netherlands.[19]

Given that in the Netherlands the cadastral value of the house is used for tax purposes, the housing shock scenarios also imply a tax change. Typically, the implied change in tax burden is small (in the order of about 100 euro per year), and hence it is unlikely to change behavior much relative to a case without tax implications.[20] Hence, in our calculations of the wealth effects we assume that households ignore tax changes when responding.

4. Empirical results

4.1. Descriptive analysis

Table 2 reports descriptive statistics on the respondents with non-missing data for consumption changes due to positive and negative wealth shocks. Other statistics refer to the whole sample. The fraction of households reporting that they would increase consumption in response to a positive shock is 8.4%, while the fraction reporting that they would reduce consumption in response to a negative shock is 9.4%.[21]

One reason why most respondents report no consumption response may be that a home price change affects in the same direction also the value of housing services consumption (i.e., imputed rent). Hence, respondents may think that the shock has not materially changed their intertemporal budget constraint. On the other hand, when the shock is positive, liquidity-constrained households could still choose to increase their spending to approach their desired non-constrained spending. Another reason for consumption inertia could be adjustment costs affecting durables. Finally, the illiquidity and indivisibility of the house could make it difficult to translate a capital gain into actual spending through borrowing or selling.

Using information on the house price change, one can compute the wealth effect on consumption for each homeowner for both positive and negative shocks. The sample averages

[19] The average transaction price in October 2018 was €294,000, while the average home value of homeowners in our survey is €305,985.

[20] The amount is calculated using the median reported house value (€250,000) and the average rates on three real estate-related taxes (i.e., the municipality tax, the flood management, and the tax on imputed rents). For a more expensive house of half a million euro the annual additional tax is about €200. There are 62 houses in our sample with a value of €500.000 or more. We do a robustness check deleting these 62 observations and find no differences in the results.

[21] Focusing on the sample with non-missing observations for positive and negative shocks (869 observations), Table A1 in the Appendix shows that 85% report no consumption effect from both positive and negative house price changes, about 6% react to only one of the two shocks, and 2.3% to both.

of these wealth effects are 4.7% for positive shocks, and 2.1% for negative shocks.[22] As discussed in Section 2, these estimates of the wealth effects are in line with existing ones derived from micro-level and aggregate data. Nevertheless, these average responses hide vast heterogeneity, with most homeowners reporting that they would not react to house price changes, and a minority reporting both an extensive margin and a substantial intensive margin response.

Figure 1 plots the entire distribution of wealth effects from positive and negative shocks, conditional on recording a change in consumption. The figure clearly shows that the distribution of wealth effects from positive shocks (left panel) dominates the distribution for negative shocks (right panel). Over 30% would spend the entire house price increase within 12 months, as opposed to less than 10% who would reduce consumption by the entire house price drop.

This reported wealth effect heterogeneity implies that for those who adjust consumption, the adjustment is considerable, with an average wealth effect equal to 56% for positive shocks and 21% for negative ones. Furthermore, most of the adjustment refers to durable goods that people plan to buy in case of positive shocks or postpone purchasing in case of negative shocks.

Financial market frictions (e.g., inability to access products like home equity loans) may induce asymmetries in the consumption response to positive and negative shocks. For example, when households cannot borrow to increase their consumption and have little liquid wealth, they will not respond to house price increases. In contrast, they may still reduce consumption when a negative permanent wealth shock occurs (since the future liquidation value of the asset has declined and consumption needs to be adjusted accordingly).

When testing the equality between the mean responses to positive and negative shocks, the average wealth effect from positive shocks exceeds that of negative ones at the 1% confidence level. This holds both unconditionally and conditionally on a non-zero response. When comparing the distributions of the two MPCs using the Kolmogorov-Smirnov test, the two unconditional distributions were not statistically different from each other, a result that can be explained due to the very large prevalence of zero responses in both distributions. On the other hand, the conditional distribution of the MPC out of the positive shock strongly stochastically dominates that of the negative shock (at the 1% level).

[22] As shown in Table 2, winsorizing responses at 99th percentile does not change appreciably the sample means.

The larger response of consumption to positive shocks is consistent with the collateral channel view, namely that increases in home values allow additional borrowing and spending, while decreases in home values do not necessarily require households to reduce borrowing, as found by Andersen and Leth-Petersen (2020).

Our survey contains follow-up questions that shed light on the various channels through which unanticipated changes in housing wealth might increase consumption. Indeed, we find that a non-negligible fraction of households reports that they would use their house as collateral to "monetize" an unexpected house value increase. Of those reporting a positive consumption change, the majority (65%) report that they would draw from their savings or use current income; 26% that they would top-up their mortgage, using their house as collateral; and 20% that they would sell the house to cash the increase in home value.[23] Note that the survey allows for multiple answers, so the fractions do not sum to 1. The last two responses indicate the presence of liquidity constraints that households try to alleviate by increased borrowing or by selling the house to lock in the capital gains.

On the other hand, of those reporting that they would reduce consumption in response to a negative shock, 55% mentions that they would increase savings, 51% that they would pay off some of their debts, and 21% that they would use the money that is not spent on consumption to invest in home improvements.

This evidence suggests that drawing from one's savings represents the main channel through which housing wealth shocks induce consumption adjustments. This is in contrast with evidence from the US, where the mortgage refinancing channel has been shown to play a dominant role (see recent contributions by Berger et al. (2018) and Beraja et al. (2019)). One possible reason for these differences is that home equity extraction is more costly in the Netherlands than in the US. Loan refinancing requires the payment of a penalty fee, except when the house is sold, or the interest rate period is upon maturity. Additional transaction fees apply if refinancing or equity extraction result in a larger loan size. Homeowners also face costs for mortgage advice, origination fees, appraisal costs and notary fees when they switch to a lender offering more competitive mortgage terms. Finally, while the interest payments on the original mortgage are tax-deductible, the interests on additional loan amounts that originate from refinancing or equity extraction are not (except if used for home improvements).[24]

[23] While this may seem a large number, it may also reflect the decision to sell a house earlier than planned (i.e., a pure intertemporal effect).

[24] Some other features of the Dutch mortgage market are worth mentioning. The Netherlands is among the countries with the highest national mortgage debt worldwide and almost ninety percent of homeowners take up a

The upper-left panel of Figure 2 shows the relation between log cash-on-hand (grouped into 20 equally sized bins) and the wealth effect out of positive shocks. We define cash-on-hand as the sum of household net income and financial wealth, net of consumer debt. An additional reason for adopting this definition is that households likely face high transaction costs from selling illiquid assets.[25]

The figure suggests a negative association between the wealth effect from positive shocks and cash-on-hand. While below median cash-on-hand the wealth effect is between 5 and 10 cents per euro, in the top percentiles it is close to zero, with almost no household adjusting consumption in response to the shock. Instead, looking at the top right panel, we find no clear pattern between the wealth effect from negative shocks and cash-on-hand.

The two other panels of Figure 2 refer to the distribution of the wealth effect with respect to age, again grouped in 20 equally sized bins. For positive shocks, the correlation between age and the wealth effect is positive, so that moving from the youngest to the oldest cohorts, the wealth effect increases from about 2 to 6 cents per euro. Again, for negative shocks we find essentially no relation between age and the wealth effect.

4.2. Regression results

Table 3 reports average marginal effects from probit and Tobit models for the probability of making a consumption adjustment following positive and negative shocks equal to 10% of one's home value (i.e., the extensive margin decision), and for the size of the adjustment (i.e., the intensive margin), respectively. Each model includes quartiles of cash-on-hand, age and family size dummies, and dummies for high school and college education.

The probability of reporting a positive wealth effect declines quite significantly with cash-on-hand, particularly at low levels of cash-on-hand. A move from the first quartile (the excluded category) to the second quartile of the cash-on-hand distribution is associated with a reduction of the probability of 4.1 percentage points, while a move to the fourth quartile reduces the probability by 10.6 percentage points.[26]

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mortgage. Mortgage lenders do not usually require a down payment. The typical duration of a mortgage is 30 years. Many mortgage loans have interest rates fixed for 5, 10 or 20 years, while adjustable-rate mortgages are uncommon.

[25] Therefore, low cash-on-hand households in our data include both those who have low net worth as well as those who are "wealthy hand-to-mouth", as defined by Kaplan and Violante (2014). Households of the latter type might have a large amount of illiquid assets (such as a house) but low levels of (liquid) financial assets.

[26] Results are similar if we replace cash-on-hand quartile dummies with log cash-on-hand or the deciles (or ventiles) of the cash-on-hand distribution.

One explanation for the negative correlation between the wealth effect and cash-on-hand is that low cash-on-hand households are much more likely to sell the house after the positive shock (26%) compared to those with cash-on-hand above the median (10%). The negative correlation also suggests that credit markets are an important channel linking wealth shocks to consumption. Indeed, 32% of households with cash-on-hand below the median report that they would top up the mortgage after the positive shock, against 20% among those with cash-on-hand above the median. Except for the coefficient for the 35-49 age dummy, the other coefficients are not statistically significant. Overall, the probability of reporting a positive wealth effect is rather insensitive to age, education, and family size.

In column (2) we complement the analysis above by estimating a Tobit regression. Specifically, we model the response of the wealth effect (which is censored at 0 and 1) to the shock, controlling for the same socio-economic characteristics used above. We report the corresponding average marginal effects. Results are qualitatively like those discussed for the probit estimates, suggesting that a similar set of characteristics contributes to both the intensive and extensive margin response of consumption to the two shocks. In terms of economic magnitudes, moving from the first to the fourth quartile of the cash-on-hand distribution reduces the wealth effect by 7.2 percentage points. The similarity of probit and Tobit results in not surprising, given that only about 10% of the sample reports a non-zero response.

The counterpart specifications for negative wealth shocks are reported in columns (3) and (4) of Table 3. The wealth effect is again higher in the lower quartiles of cash-on-hand, showing that a move from the first to the fourth quartile of cash-on-hand reduces the probability of a negative consumption adjustment by 8.8 percentage points. In the Tobit estimates, moving from the first to the fourth quartile of cash-on-hand reduce the wealth effect by 2.7 percentage points.

In Table 4 we check whether the housing wealth effect on consumption is related to bequest motives by constructing a variable that considers both the probability of making a bequest as well as the planned amount.[27] We also include an indicator for whether the household has plans to move. As mentioned in Section 2, one would expect that bequest motives reduce the wealth effect, while plans to move raises it. We find that the coefficient of planned bequests (standardized by total cash-on-hand) is indeed negative in all specifications,

[27] We ask households to report the probabilities that they will leave an inheritance of: (i) €10,000 or more; (ii) €100,000 or more; and (iii) €500,000 or more. Based on reported probabilities to these questions we deduce a measure of planned bequest for each respondent.

but statistically significant (at the 10% level) only in the probit estimates for positive wealth shocks. We also find that plans to move are positively associated with wealth effects, but the coefficients are again imprecisely estimated.[28]

Next, we examine the role of indebtedness and credit constraints by associating consumption responses to wealth shocks with the LTV. In the Netherlands, a 100% LTV represents an important indebtedness threshold for several reasons. First, applications for new mortgage debt are subject to a legal LTV cap of 100%; many first-time homebuyers take out mortgages at or close to this capped value. Second, when households with high mortgage debt go underwater (due to declining house prices), the cost of their debt increases when moving to a new house or refinancing the mortgage. This is partly because the tax code does not allow mortgage interest rate deductions for the part of the mortgage loan exceeding the home value, and partly because mortgage interest rates on loans with an LTV higher than 100% face a surcharge. Moreover, higher mortgage payments reduce access to consumer credit. Finally, if an important vehicle for financing a wealth effect on consumption is equity extraction through home equity loans, an LTV of 100% or more implies there is no equity to extract in the first place (at least for younger homeowners), and hence a lower or no consumption response (no wealth effect) would not be surprising.

In view of the above, we check whether relaxing a (likely) binding liquidity constraint affects the probability of increasing consumption after a positive shock. To this end, in Table 5 we use a dummy for households with a loan-to-value ratio (LTV) between 100 and 110%, since for these households a 10% house price increase will lower their LTV ratio below 100%, with potential liquidity benefits.[29] On the other hand, such a house price increase will not relax the liquidity constraint that households with LTV greater than 110% presently face. According to results shown in Table 5, we find that only the former group plans to increase consumption in response to a positive shock: the probit and Tobit coefficients are, respectively, 0.11 and 0.077%, and both are statically different from zero at the 1% level.

The counterpart specifications for negative shocks are reported in columns (3) and (4) of Table 5. Like in the case of positive shocks, we use dummies denoting households for which a 10% house price decrease implies either the onset of a liquidity constraint (i.e., an LTV between

[28] We also run regressions for the difference between consumption changes from positive and negative wealth shocks. Given that only 10% of the sample reports positive or negative wealth shocks, the data do not allow reliable inference on the determinants of the difference, as coefficients are not statistically different from zero.
[29] Due to missing values for the LTV ratio, the samples in columns (2) and (4) are slightly reduced. We also experiment with a richer specification that includes dummies for LTV thresholds below 100%, such as 50% and 70% and we did not find any statistically significant differences in responses for these groups.

90 and 100%) or further tightening of a current liquidity constraint (i.e., LTV> 100%). We find that both groups plan to reduce spending in the face of a negative shock, as an LTV higher than 100% impairs access to credit. Specifically, we find that, in case of a negative shock, the probability of a consumption adjustment is 6.8 and 9.4 percentage points higher for these two groups of households. Both coefficients are statically different from zero (but not from each other). In the Tobit estimates for negative wealth shocks, the coefficients are, respectively, 2.0% and 2.3%, and in both cases they are statistically different from zero.

Cash-on-hand is defined as current net household income plus net financial wealth. In additional regressions we check whether income and cash-on-hand have a different effect on the probability of making a consumption adjustment and on the reported size of the wealth effect on consumption. For this purpose, we use separate dummies for household net income and net financial wealth quartiles. Arguably, the consumption response to house price shocks might depend on the size of the shock itself, which by construction is proportional to the value of the house. Thus, in additional specifications, we also check whether the house value matters by controlling for housing wealth quartiles. The results, reported in Table 6, suggest that the main driver of the wealth effects is net financial wealth. Net income plays essentially no role, as the income quartile dummies are never statistically significant. Coefficients on the dummies for real assets are also mostly insignificant.

In sum, results from the descriptive and regression analyses suggest that the average wealth effect is the result of about 10% of households changing their spending decisions, and the remaining 90% not responding to the shock. The evidence points to a relevant role of liquidity constraints and the collateral channel, for three reasons. First, the wealth effect from positive house price shocks exceeds the wealth effect from negative shocks. Second, the wealth effect is stronger at low levels of cash-on-hand, particularly the financial wealth component of cash-on-hand. Third, households with high LTV ratios (where “high” is set by the institutional environment), and therefore more likely to be constrained, react more to the shock. Our findings do not support other potential explanations, such as bequest motives and plans to move, possibly because our tests have low power, due to limited sample size and/or low prevalence of reports of bequest motives, moving plans, and response to the shock.

4.3. Robustness checks

Presenting the same shock scenarios to survey respondents implies that the underlying house price shock is not correlated with household characteristics, expectations, and other behavioral traits. As a result, responses to the survey questions should be insensitive to

variables reflecting households' expectations and uncertainty about own income, expectations about future housing price shocks (aggregate or idiosyncratic) and the future state of the macroeconomy as well as individuals' general optimism and understanding of the survey questions. To examine this, we exploit the richness of our data and add to our baseline specifications indicators for the above variables.

First, we use questions on income expectations asking about the minimum and the maximum expected income in a typical month. Expected mean income and the standard deviation of expected income (as a proxy for idiosyncratic income risk) are computed assuming that the distribution between the minimum and the maximum is triangular, as in Guiso et al. (2002). Considering these additional variables does not change our baseline results (see Online Appendix, Table A2).

Second, we check whether expectations about future house prices influence the wealth effect. To this end, we utilize expectations about aggregate price movements in the Dutch housing market. Note that this information is collected in the baseline DNB survey (contacted few months prior to our special purpose survey), asking respondents to indicate the kind of price movements they expect on the housing market in general in the next two years. Therefore, when respondents report the consumption effect out of their own house price changes, it is unlikely that they anchor their responses on the question about general housing market movements. Moreover, we consider respondents' expectations about house prices in their own neighborhood (which clearly have a large idiosyncratic component). As for income, these expectations are calculated using a special set of questions that allow us to compute, for each respondent, the first and second moments of the expected distribution of house prices. Note that also this set of questions was asked in a different part of our special purpose survey, prior to the main questions on the consumption adjustments out of housing wealth shocks. Results are reported in Tables A3 and A4 (panel on the right side) of the Online Appendix and are in line with our baseline ones.

To control for macroeconomic expectations, we rely on aggregate spending expectations, also available in the DNB 2018 baseline survey using the min/max framework.[30] Results with

[30] The question on aggregate spending is the following: "*How much higher or lower do you think total spending on nondurable goods and services will be for the Dutch economy (that is, all households in the Netherlands) in the next twelve months compared to the last twelve months?* Please provide an answer in percentage terms. If you think total spending on nondurable goods and services for the Dutch economy on average will decrease, please fill in a negative percentage (insert a minus sign for the number). If you think total spending on nondurable goods and services for the Dutch economy on average will increase, please fill in a positive percentage. If you think total spending on nondurable goods and services for the Dutch economy on average will not change, please fill in 0 (zero)."

these indicators are reported in the Online Appendix (Table A4; panel on the left side) and, once more, leave our baseline findings broadly unaffected.

It is important for our analysis that respondents understand the counterfactual nature of the question, and specifically that they answer the question as an "additional" change in spending due to the price increase. There is a debriefing question at the end of our special purpose survey on whether the respondents found the questions easy or difficult to understand (scale 1-5). Most respondents did not report any issues in understanding these questions. Moreover, given that survey responses may be affected by financial sophistication, we control explicitly also for respondents' financial literacy.[31] We report these regressions in Table A5 of the Online Appendix and results remain unchanged.

We also construct an indicator of optimism by taking the difference between subjective life expectancy (i.e., self-reported probability to survive upon age 65, 80 or 90, depending on the current age) and objective life expectancy (by gender and age) from official mortality tables.[32] Results in Table A6 that takes into account the proxy for optimism are similar to the baseline estimates.

A different issue mentioned above is that our survey includes the "don't know" option to the wealth shock questions, since forcing respondents to answer might induce them to drop out of the survey and/or to provide non reliable answers which would add noise in the data. We analyze the characteristics of the 15% of non-respondents and find that non-responses are more prevalent among households with lower education and income. Given that the non-responses are not random, results could be potentially affected by selection bias. To address this issue, we impute the missing values using multiple imputation (results are shown in Table A7 of the Online Appendix). We observe that the negative effect of cash-on hand on the MPCs and the probabilities that they are larger than 0 is present also in this case.

5. Summary

We use the responses of a representative sample of Dutch households to survey questions that ask how much their consumption would change due to unexpected, permanent shocks (positive or negative) to their house value. By design, these shocks are orthogonal to observable and unobservable household characteristics. Our survey questions elicit in a simple and

[31] We proxy financial literacy by the number of correct answers out of the three basic questions measuring financial knowledge developed by Lusardi and Mitchell (2011).
[32] Puri and Robinson (2007) use a similar measure of optimism.

effective way respondent-specific consumption responses to a given change in home value (the wealth effect). Thus, we avoid the issue of estimating the wealth effect using observed co-movements of spending and home prices, which may not necessarily reveal the causal effect of home value changes on consumption. Moreover, our approach allows measuring heterogeneous wealth effects and studying their association with household characteristics. In addition, we test for asymmetric responses to positive and negative wealth shocks without relying on selected household groups exposed to one of the two types of shock.

The average effect on consumption that we calculate out of individual reported responses to a change in the value of their house is in the range of 2 to 5 cents per euro, in line with econometric estimates that use wealth and consumption realizations. However, the respondent-specific wealth effects masks significant heterogeneity of spending responses. In particular, the extensive margin response is limited, with more than 90% of the sample not responding to positive or negative housing wealth shocks. This represents a novel finding in the literature on the consumption response to housing wealth shock. On the other hand, intensive margin responses (conditional on adjustment) are large.

The relation between the wealth effect on consumption and cash-on-hand is negative, consistent with models with precautionary saving and liquidity constraints. In a related vein, homeowners that are underwater reduce consumption significantly when hit by a negative housing wealth shock. We also find that adjustments in savings represent the main channel through which consumption responds to housing wealth shocks. Finally, we find evidence for asymmetric wealth effects. That is, the consumption response to positive wealth shocks is greater than the response to negative shocks, as suggested by the collateral channel mechanism of transmission between wealth shocks and consumption.

Overall, our approach provides not only a methodological alternative to existing studies investigating the links between housing wealth shocks and consumption adjustments, but also reveals important elements of response heterogeneity that may be useful when considering the effect of housing policies.

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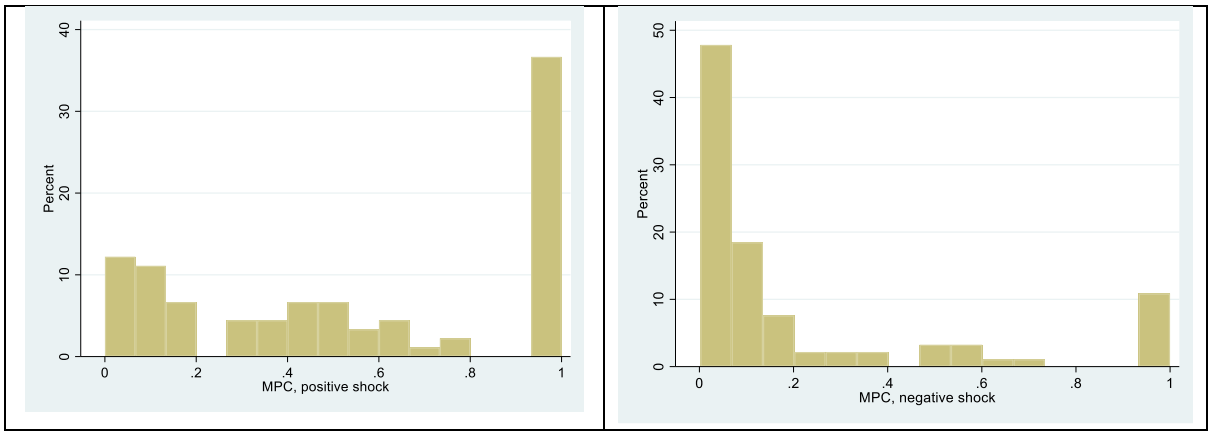
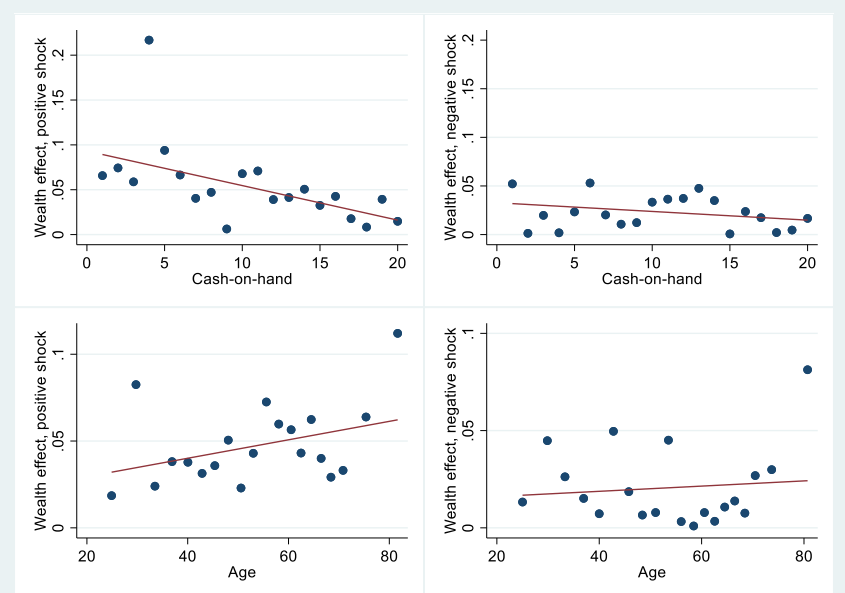
Figure 1
The distributions of the wealth effects from positive and negative shocks



Note. The left panel plots the distribution of the wealth effect from a positive house price shock, conditional on reporting a change in consumption. The right panel plots the distribution of the wealth effect from a negative house price shock, conditional on reporting a change in consumption

Figure 2
The relation between the wealth effect, age and cash-on-hand



Note. The upper panels plot the wealth effect from positive and negative shocks against 20 percentiles of the cash-on-hand distribution. The bottom panels plot the wealth effect from positive and negative shocks against 20 percentiles of the age distribution.

Table 1
Regression of realized consumption growth on realized house price growth

Growth rate of home value	0.167
	(0.084)**
II cash-on-hand quartile	0.012
	(0.010)
III cash-on-hand quartile	0.003
	(0.010)
IV cash-on-hand quartile	0.002
	(0.011)
Constant	0.001
	(0.008)
R^2	0.01
N	867

Note. The dependent variable is the growth rate of realized consumption. The growth rate of home value is calculated from the difference in reported home values in 2018 and in 2017. The regression uses CentER panel data for 2017-18. Huber-White standard errors are reported in parentheses. * denotes statistical significance at 10%, ** at 5%, and *** at 1%.

Table 2
Sample statistics

	Mean	*S.D.*	*Median*	*95th pct*	*N*
Proportion positive ΔC from positive ΔW	0.084	0.278	0.000	1.000	1,069
Proportion negative ΔC from negative ΔW	0.094	0.293	0.000	1.000	974
Wealth effect out of positive ΔW	0.047	0.190	0.000	0.412	1,069
Wealth effect out of positive ΔW, winsorized at 1%	0.047	0.190	0.000	0.412	1069
Wealth effect out of negative ΔW	0.021	0.116	0.000	0.068	974
Wealth effect out of negative ΔW, winsorized at 1%	0.020	0.113	0.000	0.068	974
Age	52.518	15.379	53.000	77.000	1264
Female	0.434	0.496	0.000	1.000	1264
Family size	2.341	1.183	2.000	5.000	1264
High school	0.353	0.478	0.000	1.000	1263
College	0.409	0.492	0.000	1.000	1263
Cash-on-hand	79.49	170.88	32.15	303.90	1136
Planned bequests	0.943	0.954	0.822	1.935	845
Plans to move	0.226	0.418	0.000	1.000	1090
100% < LTV < 110%	0.039	0.193	0.000	0.000	1192
LTV>110%	0.039	0.195	0.000	0.000	1192
90% < LTV < 100%	0.099	0.299	0.000	1.000	1192
LTV>100%	0.065	0.247	0.000	1.000	1192

Note. The table reports means, medians, 95th percentiles and standard deviations of the variables used in the estimation. The number of observations refers to the number of non-missing values. Planned bequests are divided by cash-on-hand, and multiplied by the probability of making a bequest. Data are drawn from a special purpose survey in September 2018 to all adult members of the CentER Internet panel.

Table 3
Regressions for the determinants of the wealth effects, baseline specification

	Pos. shock, 0/1	MPC, positive shock	Neg. shock,0/1	MPC, negative shock
Age < 35	-0.021	-0.015	0.034	0.006
	(0.031)	(0.020)	(0.032)	(0.010)
Age 35 to 49	-0.057	-0.035	-0.037	-0.013
	(0.029)**	(0.019)*	(0.029)	(0.010)
Age 50 to 64	0.003	0.000	-0.043	-0.017
	(0.021)	(0.014)	(0.026)*	(0.009)*
Female	-0.001	-0.001	-0.020	-0.008
	(0.018)	(0.012)	(0.019)	(0.007)
HH size = 2	-0.003	-0.005	-0.041	-0.016
	(0.022)	(0.014)	(0.022)*	(0.008)**
HH size >2	-0.018	-0.018	-0.055	-0.019
	(0.028)	(0.019)	(0.029)*	(0.010)*
High school	-0.021	-0.013	-0.045	-0.009
	(0.023)	(0.015)	(0.029)	(0.009)
College	-0.029	-0.021	-0.002	0.002
	(0.023)	(0.015)	(0.026)	(0.008)
II cash-on-hand qrt	-0.041	-0.030	-0.042	-0.009
	(0.023)*	(0.016)*	(0.025)*	(0.008)
III cash-on-hand qrt	-0.040	-0.029	-0.068	-0.016
	(0.023)*	(0.015)*	(0.027)**	(0.009)*
IV cash-on-hand qrt	-0.106	-0.072	-0.088	-0.027
	(0.029)***	(0.020)***	(0.030)***	(0.010)***
N	980	980	906	906

Note. The table reports average marginal effects from probit regressions for the probability of reporting a consumption adjustment in response to positive and negative house price shocks and average marginal effects of Tobit estimates (censored at 0 and 1) of the MPC from positive and negative shocks. The following categories are omitted: age over 64, singles, less than high school, first cash-on-hand quartile. Robust standard errors clustered by household are reported in parentheses. * denotes statistical significance at 10%, ** at 5%, and *** at 1%.

Table 4
Regressions for the determinants of the wealth effects, with probability of leaving a bequest and plans to move

	Pos. shock, 0/1	MPC, positive shock	Neg. shock, 0/1	MPC, negative shock
Age < 35	-0.061	-0.038	0.003	-0.002
	(0.039)	(0.023)	(0.039)	(0.012)
Age 35 to 49	-0.053	-0.027	-0.024	-0.008
	(0.032)	(0.019)	(0.031)	(0.010)
Age 50 to 64	-0.016	-0.010	-0.039	-0.014
	(0.023)	(0.014)	(0.029)	(0.010)
Female	0.011	0.006	0.007	0.002
	(0.019)	(0.011)	(0.021)	(0.007)
HH size = 2	-0.010	-0.003	0.001	-0.001
	(0.025)	(0.014)	(0.026)	(0.008)
HH size >2	-0.022	-0.014	-0.013	-0.004
	(0.034)	(0.020)	(0.035)	(0.012)
High school	-0.002	-0.004	-0.078	-0.021
	(0.026)	(0.015)	(0.034)**	(0.011)*
College	-0.006	-0.006	0.007	0.005
	(0.026)	(0.015)	(0.027)	(0.009)
II cash-on-hand qrt	-0.032	-0.021	-0.071	-0.021
	(0.026)	(0.016)	(0.030)**	(0.010)**
III cash-on-hand qrt	-0.024	-0.015	-0.085	-0.023
	(0.026)	(0.016)	(0.032)***	(0.010)**
IV cash-on-hand qrt	-0.089	-0.055	-0.102	-0.033
	(0.033)***	(0.021)***	(0.033)***	(0.012)***
Planned bequests	-0.027	-0.013	-0.008	-0.002
	(0.015)*	(0.008)	(0.012)	(0.004)
Plan to move	0.025	0.021	0.033	0.010
	(0.022)	(0.014)	(0.025)	(0.008)
N	652	652	617	617

Note. The table reports average marginal effects from probit regressions for the probability of reporting a consumption adjustment in response to positive and negative house price shocks and average marginal effects of Tobit estimates (censored at 0 and 1) of the MPC from positive and negative shocks. Planned bequests are standardized by cash-on-hand and multiplied by the probability of making a bequest. The sample size is smaller than in Table 3 due to missing values of the "Planned bequests" and "Plan to move" variables. The following categories are omitted: age over 64, singles, less than high school, first cash-on-hand quartile. Robust standard errors clustered by household are reported in parentheses. * denotes statistical significance at 10%, ** at 5%, and *** at 1%.

Table 5
Regressions for the determinants of the wealth effects, with dummies for LTV ratios

	Pos. shock, 0/1	MPC, positive shock	Neg. shock, 0/1	MPC, negative shock
Age < 35	-0.046	-0.029	0.019	0.002
	(0.032)	(0.021)	(0.032)	(0.010)
Age 35 to 49	-0.066	-0.040	-0.055	-0.018
	(0.028)**	(0.019)**	(0.030)*	(0.010)*
Age 50 to 64	0.002	-0.000	-0.046	-0.019
	(0.021)	(0.013)	(0.026)*	(0.009)**
Female	0.005	0.003	-0.015	-0.006
	(0.018)	(0.011)	(0.019)	(0.006)
HH size = 2	-0.001	-0.002	-0.046	-0.017
	(0.021)	(0.014)	(0.022)**	(0.008)**
HH size >2	-0.012	-0.013	-0.059	-0.020
	(0.028)	(0.018)	(0.029)**	(0.010)**
High school	-0.029	-0.020	-0.051	-0.011
	(0.022)	(0.014)	(0.029)*	(0.009)
College	-0.035	-0.025	-0.007	0.000
	(0.022)	(0.015)*	(0.026)	(0.008)
II cash-on-hand qrt	-0.034	-0.024	-0.038	-0.009
	(0.023)	(0.016)	(0.025)	(0.008)
III cash-on-hand qrt	-0.029	-0.021	-0.057	-0.013
	(0.023)	(0.015)	(0.028)**	(0.009)
IV cash-on-hand qrt	-0.088	-0.059	-0.075	-0.023
	(0.028)***	(0.019)***	(0.030)**	(0.010)**
100% < LTV < 110%	0.110	0.077		
	(0.038)***	(0.026)***		
LTV > 110%	0.065	0.045		
	(0.042)	(0.027)		
90%<LTV<100%			0.068	0.020
			(0.033)**	(0.011)*
LTV > 100%			0.094	0.023
			(0.037)**	(0.011)**
N	958	958	895	895

Note. The table reports average marginal effects from probit regressions for the probability of reporting a consumption adjustment in response to positive and negative house price shocks and average marginal effects of Tobit estimates (censored at 0 and 1) of the MPC from positive and negative shocks. Loan-to-value (LTV) dummies distinguish among groups with different implications for their indebtedness due to wealth shocks. In the positive wealth shock regression, LTV dummies equal one if LTV is between 100 and 110% and if LTV exceeds 110%. In the negative wealth shock regression, LTV dummies equal one if LTV is between 90 and 100% and if LTV exceeds 100%. The following categories are omitted: age over 64, singles, less than high school, first cash-on-hand quartile. Robust standard errors clustered by household are reported in parentheses. * denotes statistical significance at 10%, ** at 5%, and *** at 1%.

Table 6
Regressions for the determinants of the wealth effects,
with financial wealth, real wealth and income quartile dummies

	Pos. shock, 0/1	MPC, positive shock	Neg. shock, 0/1	MPC, negative shock
Age < 35	-0.017	-0.014	0.034	0.007
	(0.030)	(0.020)	(0.033)	(0.010)
Age 35 to 49	-0.057	-0.037	-0.040	-0.014
	(0.028)**	(0.019)*	(0.029)	(0.010)
Age 50 to 64	0.000	-0.002	-0.042	-0.017
	(0.021)	(0.014)	(0.026)	(0.009)*
Female	-0.003	-0.003	-0.021	-0.008
	(0.018)	(0.012)	(0.020)	(0.007)
HH size = 2	0.016	0.009	-0.037	-0.014
	(0.025)	(0.015)	(0.026)	(0.009)
HH size >2	0.005	-0.003	-0.053	-0.018
	(0.031)	(0.019)	(0.033)	(0.011)
High school	-0.016	-0.010	-0.041	-0.008
	(0.023)	(0.015)	(0.028)	(0.009)
College	-0.022	-0.015	0.001	0.004
	(0.023)	(0.015)	(0.027)	(0.009)
II Net fin wealth qrt	-0.030	-0.022	-0.043	-0.008
	(0.022)	(0.015)	(0.024)*	(0.008)
III Net fin wealth qrt	-0.027	-0.022	-0.062	-0.016
	(0.025)	(0.016)	(0.029)**	(0.009)*
IV Net fin wealth qrt	-0.085	-0.057	-0.097	-0.029
	(0.029)***	(0.019)***	(0.030)***	(0.010)***
II Net income qrt	-0.015	-0.013	0.015	0.003
	(0.024)	(0.015)	(0.029)	(0.009)
III Net income qrt	-0.022	-0.013	-0.030	-0.009
	(0.027)	(0.017)	(0.034)	(0.012)
IV Net income qrt	-0.031	-0.019	0.008	-0.001
	(0.031)	(0.019)	(0.035)	(0.012)
II House wealth qrt	-0.029	-0.020	0.009	0.005
	(0.025)	(0.016)	(0.027)	(0.009)
III House wealth qrt	-0.056	-0.041	-0.006	-0.003
	(0.024)**	(0.016)**	(0.027)	(0.009)
IV House wealth qrt	-0.018	-0.018	0.001	0.002
	(0.025)	(0.016)	(0.028)	(0.009)
N	980	980	906	906

Note. The table reports average marginal effects from probit regressions for the probability of reporting a consumption adjustment in response to positive and negative house price shocks and average marginal effects of Tobit estimates (censored at 0 and 1) of the MPC from positive and negative shocks. The following categories are omitted: age over 64, singles, less than high school, first net financial wealth quartile, first net income quartile, first house wealth quartile. Robust standard errors clustered by household are reported in parentheses. * denotes statistical significance at 10%, ** at 5%, and *** at 1%.