Bunching and Learning in Targeting Poverty: Evidence from Vietnam

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Abstract

Manipulation of the eligibility criteria is one reason that could increase the number of non-poor participants in anti-poverty programs in developing countries. Despite ample evidence that households manipulate these criteria, little is known about how such behaviors evolve over time in a long-term program. Using data from Vietnam, I find that, early on in each phase of its National Anti-Poverty Program, about 1-2% of the population (or 8-18% relative to the program size) bunch at the official income cutoff in order to appear eligible. However, this fraction falls by 60-100% towards the end of the phase, only to increase yet again when a new phase starts with a new income cutoff. To explain this temporal pattern of bunching, I develop a model in which over time the program staff learn to rely on housing conditions, a less-manipulable criteria, to select households. This refined information, in turns, discourages households from manipulating their income. I find that an increase of 0.5 standard deviation in the housing quality index further reduces the chance of being accepted to the program by 25.11% after two years. Meanwhile, other criteria, including reported income and asset holdings, do not contribute any additional predictive power to the program status over the same period. Without this learning process, the program would have misallocated about 1.7%, or equivalently 32.3-36.4 million USD (PPP), of its budget to non-poor households during the first phase of the program.

Key words: Bunching, Learning, Anti Poverty, Means Testing, Welfare Policy, Vietnam

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1 Introduction

Welfare programs that specifically target low income earners are important redistribution schemes to fight poverty in many countries.[1] In developing countries, the screening of participants for these programs is prone to errors because income is rarely verifiable.[2] One reason that these errors exist is due to an incentive problem: more well-off households are motivated to misreport their income or what they own to appear eligible . These attempts to manipulate eligibility criteria result in "leakage", a type of screening error in which program resources are misallocated to the non-poor.[3] Previous studies have documented households' manipulation of a variety of targeting criteria (Camacho and Conover, 2011; Banerjee et al., 2017; Cassan, 2015; Besley and Kanbur, 1991; Ravallion and Chen, 2015). However, they focus on the one-off response of households to the program rules, limiting the ability to understand the dynamics of this strategic behavior along the course of a multi-year program.[4]

My paper fills out this gap by studying the incentive dynamics in the long-running National Anti-Poverty Program in Vietnam (hereinafter referred to as "the targeting program" or "the program"). As common in many developing countries (Coady et al., 2004), the Vietnamese targeting program combines a number of criteria to assess eligibility, including an income cutoff and proxies of income such as housing conditions and asset holdings.[5] Using rich data from a nationally representative household survey spanning 15 years of the program, I document clear evidence of manipulation of reported income. This is unsurprising, given the income cutoff is publicized as the primary criteria of eligibility. Intriguingly, this strategic behavior dissipates over time, and reappears strongly whenever a new income cutoff is introduced. To explain this novel pattern of incentive effects, I show empirically and by way of a structural model that the implementing officers may play a key role in the decrease in manipulation over time. In particular, I hypothesize that over time the implementing officers may have learned to rely on less manipulable criteria to select recipients.

[1]Targeted welfare programs are common across countries, and is especially the norm in developing countries, where program budgets are often limited. In contrast, universal basic income scheme that gives everyone the same transfer, though frequently mentioned in policy debates, is still rare in practice (Hanna and Olken, 2018).

[2]Indeed, 70-80 percent of employment in developing countries is in the informal sector (McCaig and Pavcnik, 2015; Gollin, 2008; Nataraj, 2011), where workers rarely have official documentation of earnings, such as labor contracts, tax records, or payslips.

[3]The other type of screening error is "undercoverage", i.e. when the program leaves out the poor. There is a tradeoff between undercoverage and leakage. To limit undercoverage, the program needs to increase the number of participants, thereby increase the risk of leakage. However, if the budget is fixed, this will result in lower the transfer size per recipient, which may be socially suboptimal if the government cares about helping the poor.

[4]Many countries have long-running targeting programs with periodically updated eligibility rules (Hanna and Olken, 2018).

[5]The choice to amalgamate different assessment criteria is precisely to resolve the aforementioned information challenge in less developed economies. See Coady et al. (2004) for a overview of the targeting performance of different assessment methods as well as some combinations of them.

This accumulated information thus drives away the incentive for households to under-report their income.

As a first step, I plot the distribution of reported income and document recurrent evidence that a number of households bunch right at the program's income cutoff, presumably to gain access to its benefits. I manually compile a list of income cutoffs varying at the province level between 2001 and 2015 to the appropriate cross-sections of the household survey.[6] To quantify the extent of excess bunching, I adopt the excess bunching estimation method from the public econonomics literature (Saez, 2010; Chetty et al., 2011; Kleven, 2016; Kleven and Waseem, 2013; Best and Kleven, 2017). This method essentially uses the non-bunching parts of the (reported) income distribution to generate its counterfactual distribution, which would have been smooth in the absence of bunching. Comparing the actual distribution with its counterfactual yields an estimate for the excess bunching mass. I find that, at the beginning of each phase, around 1-2% of the population are likely to shade their income to the cutoff level in order to benefit from the program. Relative to the size of the program, bunchers would make up 8-18% of the number of program participants if they got accepted. Towards the end of the first 5-year phase of the program (2001-2005), the share of excess bunchers in the population falls by 87%. A roughly similar pattern of "on-then-off" bunching repeats in the subsequent phases with updated eligibility rules; the reduction in excess bunching was 100% in Phase 2 and 60% over Phase 3.

Note that a serious concern for bunching is that it may not simply reflect under-reporting of income, but rather a distortion in labor supply (Besley and Kanbur, 1991; Banerjee et al., 2017; Moffitt, 1992, 2002; van de Walle, 1998). That is, households may find the program attractive enough to actually reduce their work efforts in order to reduce their real income. This would have *real* economic consequences because there would be less total output produced in the economy. I verify that bunching in this context is *unlikely* to be driven by labor supply distortion. Using a test akin to the Regression Discontinuity framework, I find that, around the income cutoff where reported income spikes up, work hours of household heads and their spouse are still continuous. Therefore, the sudden spur around the income cutoff is more likely to be the result of misreporting income rather than attempts to reduce real income by working fewer hours.

I propose a simple game theoretic model to generate testable predictions on how implementing officers learn to use more reliable criteria to counter the initial bunching behavior from house-

[6] Vietnam is divided into 64 provinces; each provinces is divided into districts, which is further divided into communes. The central government set the national income cutoff, but a number of wealthier provinces set their own higher cutoffs. The income cutoffs, whether from the central or provincial government, has two tiers for the urban and rural areas.

holds. In the model, households can signal their eligibility (by under-reporting their income) with some cost, knowing some of their publicly observable characteristics, especially housing conditions, would be difficult to shade. This assumption is reasonable in this context, because surveyors are instructed to verify housing conditions. Furthermore, attempts to actually keep housing conditions low presumably would come at a very high cost for households, as they would have to forgo much comfort in order to appear poor.[7] Therefore, to the implementing officer, housing conditions can be a more reliable source of information to infer the true income of households. The officer wishes to select households whose *true* income is below the income cutoff, however she starts out with an imperfect mapping between between housing conditions and true income. This generates a Bayesian-Nash equilibrium in which (i) higher income earners that appear eligible according to housing conditions and have low enough misreporting cost will bunch at the income cutoff, and (ii) the officer will accept all such households. A prediction following this equilibrium is that households must have relatively low housing conditions in order to bunch. The model also predicts that if the officer learns to sufficiently improve her housing-income mapping over time, her decision will increasingly depends on housing conditions. This, in turns, discourages households from continuing to shade their income. Results from the empirical analyses (including the bunching patterns) largely corroborate with these predictions.

To test the prediction on the officer's learning, I derive an explicit equation relating her acceptance/rejection decision with two main selection criteria - reporting income and housing conditions.[8] In particular, this equation shows that the officer's estimate of the household's true income is essentially a weighted average between her information sources: current realizations of housing conditions and reported income as well as her past information about the same household.[9] This equation informs my empirical strategy to identify learning effects over time with panel data. Specifically, taking the difference between iterations of this equation allows me to trace out the learning effects: housing is increasingly predictive of the officer's decision, while reported income does not gain any predictive power. Additionally, the panel data structure enables me to purge the learning effects occurring between two consecutive periods of other confounding factors, including: (i) officers' observation of the household in the past and (ii) any time-invariant household characteristics that may confound the contemporary effects of our variables of interests (housing and income) on the officer's decision.

[7] Similar to holding off labor supply to reduce *real* income, deliberately keeping housing conditions low is a *real* distortion of consumption behavior that could have real economic effects, such as lowering health outcomes due to poor sanitation.

[8] My model focuses on these two criteria to highlight the change in the focus of screening strategy from one to another. In the empirical analysis, I also control for additional selection criteria such as asset holdings.

[9] This captures the officer's "spot" valuation of the household in a given period.

I construct a housing index with a Principle Component Analysis (PCA), combining information from five categorical variables on housing conditions that are largely defined by the program rules. I find that an increase of 0.5 standard deviation (SD) in the housing index reduces its probability of accepting the household to the program by 25.11% over the course of a two-year panel. Reported income, while predictive of the household's program status, does not gain any additional predictive power over the same period. This pattern is varied across the sample. In particular, it seems concentrated in the bottom tercile of reported income, as well as among rural and mountainous areas and minority communities. For example, over time a 0.5 SD increase in housing conditions has further reduced the participation rate by 11.7% for households in the top two income terciles, whereas the same effect for the bottom tercile is six times as large. The shift to more reliable screening criteria remarkably concentrates in subsamples that tend to display more bunching, indicating that efforts to improve screening efficacy may act as a response to initial income manipulation by households.

I demonstrate the robustness of the main results on the learning effect in several ways. To begin with, I show that the results remain unchanged when augmenting the regression equation with additional time-variant factors that the officers may take into account, such as change in employment status of household members. In my model, the officer's past estimate of the household's true income plays a role - by representing all past information about the household - in her current decision. I proxy for this unobservable object with the officer's past decision (i.e. the household's past program status), which may introduce bias because it is binary, while officer's past estimate is continuous. To check the consistency of my estimates, I augment the main specification with measures of transfer amount, which presumably increases in the officer's belief of the household's need, and still find similar results as in the main empirical strategy.[10]

One may suggest an alternative theory for the observed "on-then-off" bunching pattern. In particular, strong economic growth over the study period has lifted many households out of poverty (World Bank, 2016), thus the overall housing conditions have gone up over time and whoever still remain with the program may tend to have lower housing conditions. Put it differently, my proposed empirical strategy may have captured the effects of economic growth rather than an improvement in information extraction. I show that this alternate narrative is unlikely to hold with two additional tests. First, I repeat the same exercise but attempt to cancel the economic growth channel, by restricting the sample to households with the same realization of housing over time.

[10] As explained later, data limitation does not allow me to systematically study the officer's decision on the transfer amount per se. I only use these sporadically available measures of transfer size in robustness checks. For studies that look at the impact of targeting criteria on the *transfer amount*, see Ravallion and Chen (2015) and Alderman (2002).

Even among households who exhibits virtually no change in their housing conditions, housing conditions still become strongly predictive of the program status over time. Second, I carry out a falsification test by estimating the same regression with additional panel data that spans over periods of program reforms. The transition to new eligibility rules (in particular a new income cutoff) provide an excellent opportunity to test the learning hypothesis, because the previous refinement of the housing-income mapping that the officers have accumulated with regarding to the old income cutoff may no longer apply to the new income cutoff. Indeed, over transitioning periods, the household's program status does not seem to get more dependent on housing condition at all.

To evaluate whether learning over time has improved the targeting performance of the program, I conduct a simple counterfactual analysis. I compare the allocation of program "slots" under the *status quo* with an alternative targeting mechanism, in which I turned off the learning effect. Comparing each of these allocations to a classification of true poverty (defined by per capita food consumption), I compute statistics, such as error rates, to evaluate how well the program targets the poor with and without learning. I find that if learning via housing conditions has been hindered, the leakage and undercoverage rates would go up by roughly 1.14-1.66 percentage point in the follow-up round. This implies that learning has helped transfer \$32.3-36.4 million dollars (PPP), which otherwise would have been misused, to the neediest households.

This study contributes the literature on the design of targeting programs. In particular, it brings together two aspects - community-based targeting and incentives effects - that have rarely been investigated together. Delegating the selection of program participants to local community representatives (usually community leaders and/or government officers from the lowest administrative level) is a common design feature in large-scale redistribution programs in transition economies, such as Uzbekistan, Albania, Armenia, China, Bangladesh, Mexico, Chile, Bolivia, and Senegal (Ravallion and Chen, 2015; Conning and Kevane, 2002; World Bank, 1999; Galasso and Ravallion, 2005; Alderman, 2002). Most of these community-based mechanisms allow the local implementing officers to exercise some amount of discretion within a set of program guidelines. Alternatively, they may devolve the selection task entirely to the community residents by having them rank the neediness of their neighbors based on their own perceptions.

Conceptually, these mechanisms could solve the information problem, because the local community presumably has a better idea of who are in needs as compared to outside representatives from higher-up government levels (Crémer et al., 1996; Seabright, 1996). However, empirical evidence on their informational advantage so far is still mixed. Bergeron et al. (1998) randomly chooses subgroups of community members to rank the wealth of their fellow villagers and found these rankings have fairly weak correspondence betweeen one another, while Adams et al. (1997) and Alatas et al. (2012) find participatory community meeting can identify poor households well based on proxies of income or consumption levels. A particularly relevant study is Alderman (2002) which finds evidence that local targeting officials may have access to information on household welfare that is unavailable to central authorities. His test for the informational advantage is the closest to mine: by regressing social assistance on the required criteria and a measure of welfare unspecified by the program design (expenditure). He interprets the positive coefficient on the latter, conditioning on other observable household characteristics, as evidence of additional informational gain during local targeting procedure. However, his strategy relies on a single cross-section, limiting the ability to investigate the information extraction process over time. My study documents evidence of this process, but highlight the learning through a set of criteria that is hard to tinker with, given that the main income-based criteria can easily be misreported.

It is worth noticing that relying on local agents may have a downside of elite capture, which happens when the non-poor (presumably could be relatives and friends of the local officials in charge) end up with resources intended for the poor (Bardhan and Mookherjee, 2000, 2005). Some empirical studies finds evidence of capture at the village level (Galasso and Ravallion, 2005; Lanjouw and Ravallion, 1999), while others do not (Alatas et al., 2012; Bardhan and Mookherjee, 2005). In this study, I assume that the incentives of the local implementing officers are aligned with the central government's objectives, thus the officers are interested in finding the neediest households. This assumption is reasonable in the current setting, because the list of recipients selected by the officers is further contested by a pubic community meeting. This program design arguably acts as an audit mechanism to keep the officers accountable (Conning and Kevane, 2002).

Within the large umbrella of targeting literature lies the literature on the incentives to manipulate eligibility criteria for welfare programs. In developing countries, manipulation of non-income criteria have been observed, such as proxy-means score (Camacho and Conover, 2011), identity (Cassan, 2015), and some salient assets such as TV (Banerjee et al., 2020). To add to the list of targeting criteria that can be manipulated, I document the evidence of misreporting income. Most importantly, my study illuminates a novel self-correcting mechanism whereby the local staff reacts to household incentives. This could provide an explanation for why Banerjee et al. (2020) find the underreporting of TVs was short-lived: households may find their tampering efforts unfruitful so they eventually stop altogether. This mechanism also relates to Ravallion and Chen (2015)'s speculation on their finding of incentive effects. Drawing on their fieldwork experience, the authors suggest that local officers are well aware of the income-based incentive problems and themselves respond by "actively smoothing out [the dependence of] payment and participation" on income.

While Ravallion and Chen (2015) lack data to test out this hypothesis, I can explore it in my context.

The remainder of this paper is organized as followed. Section 2 summarizes the institutional background of the targeting program in Vietnam. Section 3 describes the data and how I handle certain key variables. The end of this section also presents summary statistics. Section 4 documents bunching evidence and its temporal pattern. Section 5 formalizes the theoretical model to explain the observed bunching pattern and generates additional testable predictions. Section 6 carries out the empirical tests for these additional predictions. The strategies to implement these tests with the data are also discussed in details here. Section 7 benchmarks the targeting performance of the current program design against a hypothetical mechanism in which learning is muted. Section 8 concludes.

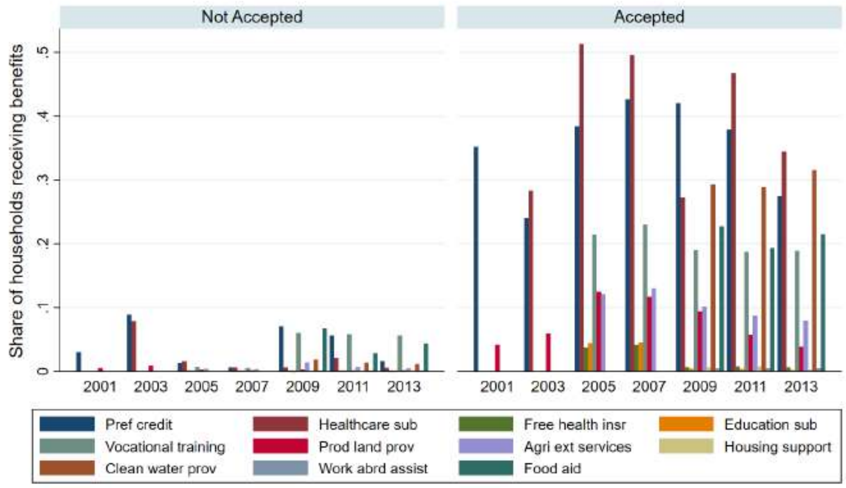
2 Institutional Background

In this paper, I study is the National Anti-Poverty Program in Vietnam between 2001 and 2015. Households who got accepted to the program may benefit from a host of policies under the National Anti-Poverty Program. The benefits include free health insurance and healthcare subsidy, children education subsidy, cash allowance, tax exemption for agricultural land use, agricultural land provision, housing and production support, preferential business credit, and vocational programs. Note program participants are not guaranteed with all the benefits, but they have a much higher chance of receiving them, as shown in Figure 1. For example, between 30% and 50% of accepted households receive health care subsidy, while less than 10% of non-participants get this benefit. I collate information from several policy notes and do a back-of-the-envelope calculation of the total monetary value of benefits in Table A2.[11] This amount is 96,000 Vietnam Dong (VND) per person per month in 2002 terms for a typical household of two adults and two children. The monetary value of benefits is roughly on par with the poverty line for rural areas (in real terms) in all phases.

The selection of program beneficiaries is conducted annually by a committee of local government officers. This committee is formed at the lowest administrative level — a commune, which on average is composed of 16 hamlets. Each hamlet has around 100 households, so a typical com-

[11] In my calculation, I consider only monetary sums that the household directly receive, and exclude benefits that are more difficult to quantify, such as preferential credits, production land, agricultural extension services, or vocational training.

Figure 1: Share of households receiving benefits



Notes: Data from VHLSS 2002-2014. The graphs report the share of households receiving each category of benefit by program status ("Not Accepted" households on the left versus "Accepted" households on the right). Both variables, program status and benefit receipt, reported here pertain to the calendar year preceding the survey.

mittee monitors 1600 households.[12] Since 2001, the process has included three steps: it starts with an assessment of non-income factors, followed by an income test, and ends with a community voting session.[13]

The non-income assessment serves at a screening tool to shortlist a subset of households to enter the income test. In this step, selection officers gauge the neediness of households by observing their housing conditions and assets. In certain periods, officers also inquire about the negative or positive shocks the household has experienced in the last 12 months. From this initial screening step, officers narrow down a smaller set of households to further investigate their income. In the income test, selection officers compute the average monthly per capita income and compare it with an income cutoff, which is the poverty line postulated by the central or provincial government (whichever higher will apply). In the last step, a semi-final list of PHC candidates is published for a few days, followed by a hamlet meeting in which households in the hamlet vote for final grantees of the card. Each household on the final list must win the favor of 50% of households present at the meeting.

[12]Estimates of population size of hamlet and commune are provided in a document accompanying the VHLSS datasets.

[13]Prior to 2001, a "caloric" income cutoff was used, that is, it was quantified in terms of the amount of rice it could buy. I focus on the era post 2001 when a monetary income cutoff was used.

It should be noted that data on the voting pattern and outcomes from these community meetings do not exist. Although I lack the data to further explore its role in reality, theories have suggested communal participation in targeting could be an effective audit device to prevent nepotism (Conning and Kevane, 2002). Following this theoretical stand, I assume that the local implementation officers' objectives in this context are line with the central government's goal of targeting truly needy households.

Policy change

The government of Vietnam raised the national income cutoff three times in 2001, 2006, and 2011, at the beginning of each five-year phase of the program. Measured as the monthly per capita income, these cutoffs are equivalent to \$6.79, \$12.5 and \$19.5 (current US dollars at the beginning of a phase) in rural areas, and \$10.19, \$16.26 and \$24.38 in urban areas. Besides the hikes at the national level, some wealthier provinces are free to set cutoffs that are higher than the national level at any point in time. Details on the hikes at the national level are in Table 1 and those at the provincial level are in Table A1.

Although there are cross-sectional variation in cutoffs (at the provincial and urban/rural levels), only the rural cutoff at the national level seems to induce substantial response from households, as documented in subsection 4.1 below. Urban areas and richer provinces tend to have a smaller fraction of destitute households, thus their cutoffs are not binding for the majority of the households. Nevertheless, temporal variations in the national cutoff, especially for the rural areas, offer great opportunities to understand the targeting procedure in anti-poverty programs. I exploit these reforms in two ways. First, I document evidence of manipulation of reported income (bunching) in Section 4 and establish an empirical fact that bunching dissipates over time within a given phase of the program. This motivates my theoretical model of officer learning and how this mechanism explains the observed temporal pattern of bunching. Second, in my empirical exercises to test the learning mechanism, I show how the learning process that accumulates over periods within the same phase no longer applies after the transition to a new phase. This provides another evidence that learning might indeed be a driver for the reduction of bunching within the same phase.

Table 1: Vietnam National Poverty Lines 2001-2016

Year	Phase	Urban		Rural	
		'000 VND	USD equiv	'000 VND	USD equiv
2001-2005	1	150	10.19	100	6.79
2006-2010	2	260	16.26	200	12.50
2011-2015	3	500	24.38	400	19.50

Notes: Cutoffs are measured in monthly per capita income. USD equivalent amounts of Phase 1, Phase 2, and Phase 3 are calculated with exchange rates for 2001, 2006, 2010, respectively. Exchange rates are obtained from the World Bank Open Data.

3 Data

3.1 Household data

I employ data from the Vietnam Household Living Standards Survey (VHLSS) to conduct this study. The VHLSS is a nationally representative survey that is conducted every two years by the Vietnamese General Statistics Office (GSO) through face-to-face interview during house visits. I use eight rounds from 2002 to 2016, which span over the time horizon of the three program phases: 2001-2005, 2006-2010, and 2011-2015. VHLSS contains rich information on income, education, employment, household expenditure, assets and housing conditions, as well as participation status in the National Anti-Poverty Program. Although this survey is *not* directly used to select beneficiaries for the program, households may still perceive that it can affect their eligibility and manipulate their answers to appear eligible (I verify evidence of income manipulation in Section 4). It is noteworthy that while most information is self-reported and can be tempered with, information on housing is generally hard to manipulate. This is because the surveyor is physically present and can verify the physical features of the dwelling, thus it is difficult for households to alter them on a short notice. As such, housing conditions will play a critical role in my empirical strategy for learning effects in Section 6.

Each round of VHLSS is a cross-section of approximately 45,000 households.[14] The survey also maintains a biennial rotating panel. In each round, the survey rotates out half of the enumeration areas (EAs) and rotates in newly selected enumeration areas. This means half of the households from the previous round are maintained as a panel.[15]

[14] An exception is the first round in 2002, which has a larger sample size of 75,000 households.

[15] One important note about this rotating structure is that it was disrupted between the 2008 and 2010 waves, due to an overhaul in the master sample. Consequently, households in 2008 are not followed to 2010.

3.2 Institutional information

From several decrees and circulars published from 2001 through 2015, I compile a list of national income cutoffs and a list of provincial income cutoffs in Table 1 and Table A1, respectively. I merge these values to the corresponding provinces, urban/rural regions and survey years in VHLSS. Note that whenever a new income cutoff is introduced, it starts taking effect in the certification season in the subsequent November. For example, the new cutoff for Phase 3 was announced in January 2011, so it applied to the certification season in November 2011. The certification season in the year before that, in November 2010, still used the previous cutoff from Phase 2.

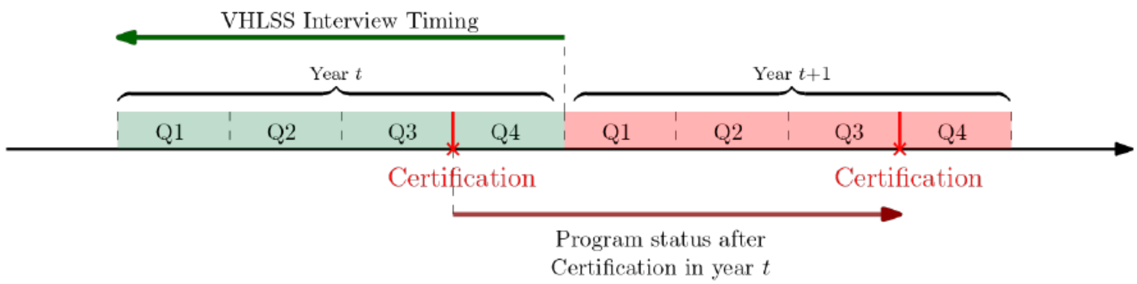
The same legal documents also contain information about non-income criteria, including housing conditions and assets. Of these components, housing conditions are more consistent throughout the three phases of the program and can also be measured more precisely in VHLSS (Table A3). For instance, whether the physical house is built with temporary materials is a criterion in all three phases. With regards to assets, not only their requirements change over the phases, but many of them are not observed consistently in VHLSS. I discuss how I construct time-consistent measures of housing conditions and assets in subsection 3.3 below.

3.3 Definition of some key variables

Program status This study looks at the *ex ante* behavior of households around the income cutoff that may determine their eligibility in the upcoming certification period. As illustrated in Figure 2, VHLSS obtains information about households throughout the year, but the upcoming certification for the next-year program status happens in the last quarter. To make appropriate inference about the behavior of households in anticipation of the upcoming certification season, I make use of the rotating panel structure to link the next-year program status at $t + 1$ to the variables (such as income, housing conditions, and assets) observed a given survey round collected at t. Because the survey is conducted biennially, this lead program status applies to the calendar year *in between* two rounds of survey. All households that could be linked to their lead program status form four two-round panels, including 2002-2004, 2004-2006, 2010-2012, and 2012-2014. These panels are used for analyses in Section 6. Note that, since 2008 households are not resurveyed in 2010, this entire round cannot be linked to a lead program status. Thus panels for 2006-2008 and 2008-2010 cannot be formed.

Non-income criteria I use several variables from the survey to construct proxies for the non-

Figure 2: Match lead program status to current observations



income assessment in the selection process. As mentioned in Section 2, these non-income criteria include housing conditions, assets, and plausibly some negative and positive shocks that households experience in the 12 months prior to the survey. While some of these variables are observed, many are not. To the best of my ability, I select variables that could be consistently measured in all survey rounds –these variables mainly proxy for housing conditions and asset holdings. In addition, there are several subcomponents in either housing or asset criteria, I encompass them into two following metrics.

First, I construct a housing index from five categorical variables classifying the physical structure of the dwelling, the source of drinking water, the toilet type, the method of trash disposal, and the source of lighting. Given the difference in the number of categories and the concepts that they measure, I combine and extract the variation in these variables with a Principle Component Analysis (PCA). This is a common data-reduction technique to summarize the maximum possible information from a large number of variables with only a few "components". I generate the first component and rescale it to range from 0 to 100. This constitutes the housing index. Each increment in this variable corresponds to a higher value of housing conditions. Second, to measure asset holdings, I construct an asset index in a similar manner using a variety of household appliances.[16] The resulting asset index is also the first component from the PCA, rescaled to range from 0 to 100. Table A4 details the raw variables that go into the constructions of the housing and asset metrics.

[16]The value of the appliances are taken into account in this index. It is the current value after depreciation.

3.4 Summary Statistics

Table 2 presents summary statistics of the data from 2002 to 2014.[17] Column (1) reports statistics for the full sample of all cross-sections. Column (2) is restricted to the "Lead-Status" panels.[18] The Lead-Status panels (Column (2)) yields similar statistics to the full sample (Column (1)), confirming that the Lead-Status sample is a random subsample of the full sample. Appendix B provides more information on these samples.

Considering the certification outcome from the upcoming selection period at $t+1$, about 12% of Vietnamese households were beneficiaries of the Poverty Reduction Program between 2002 and 2014. Consistent with the profile of a low-income country at the time, the income per capita of the average Vietnamese during over the study period is about VND 600,000 per month, or US$ 474.72 per year in 2002 terms.[19] Using this measure, around 4% of the population falls below the official income cutoff. Comparing this fraction with the program participation rate of 12% above, it is indicative that the income cutoff is not the only factor to determine selection process. As for the asset index, the average household scores 11 out of 100, again reflecting a low living standards. In terms of the housing index, Vietnamese households average at 16 points out of 100. With regards to labor supply, the main income-earners (head and spouse) in a household work 136-145 hours per month, or around 34-36 hours per week.

Columns (3) and (4) split the Lead-Status panels by the certification outcome to juxtapose the livelihood of households who were accepted versus those who were rejected. While only 3% of rejected households is below the official income cutoff, 18% of accepted households falls under this threshold. This fraction is well below unity, again reflecting the cutoff is not the only determinant of program status. Overall, accepted households are much worse off than those outside the program. The average monthly per capita income reported by accepted households is less than half of the income of rejected households. The same pattern is also reflected in measures of assets and housing conditions. Households certified as poor also tend to be less educated, more likely to belong to ethnic minority groups, and locate in rural areas. In general, the targeting program seems to select the "right" households, that is, those who exhibit lower socio-economic characteristics.

Regarding labor supply, the head of an accepted household on average works an equivalent of

[17] I only use 2016 round to link the lead program status in 2015 to observations in the 2014 round.

[18] A small number of observations in 2004 are duplicated because they appear in both 2002-2004 and 2004-2006 panels. Similar duplication also happens for some households observed in 2012. I keep only one copy of these duplicated observations in these summary statistics.

[19] Income is adjusted for inflation with regional and temporal CPI deflators accompanying the data. These deflators are computed by GSO. Exchange rate in 2002, obtained from the World Bank Open Data, is USD 1 = VND 15,279.5

Table 2: Summary statistics

	(1) Full sample	(2) Lead-Status panels	(3) Lead-Status = Accept	(4) Lead-Status = Reject
Lead-Status = Accept (at t+1)	.	0.117	1	0
	(.)	(0.322)	(0)	(0)
Below cutoff	0.0430	0.0429	0.176	0.0251
	(0.203)	(0.203)	(0.381)	(0.157)
Reported p.c. income (in 2002 '000 VND)	601.0	602.9	250.7	649.9
	(865.6)	(740.7)	(165.8)	(774.6)
Reported p.c. income (in 2002 USD)	39.34	39.46	16.41	42.54
	(56.65)	(48.48)	(10.85)	(50.69)
Ln reported p.c. income	6.100	6.087	5.352	6.185
	(0.781)	(0.770)	(0.579)	(0.739)
Asset index	11.11	11.03	9.488	11.23
	(2.339)	(2.156)	(0.997)	(2.186)
Housing index	60.35	59.73	46.34	61.52
	(18.16)	(17.74)	(14.09)	(17.41)
Monthly work hours - Head	136.5	141.9	127.3	143.7
	(94.95)	(92.99)	(89.55)	(93.23)
Monthly work hours - Spouse	138.0	145.2	136.4	146.1
	(91.61)	(90.24)	(82.88)	(90.96)
Rural	0.721	0.743	0.902	0.721
	(0.449)	(0.437)	(0.297)	(0.448)
Ethnic minority	0.120	0.129	0.354	0.0987
	(0.324)	(0.335)	(0.478)	(0.298)
Years of education - Head	7.200	7.121	4.619	7.457
	(3.686)	(3.669)	(3.542)	(3.554)
Household size	4.075	4.111	4.040	4.120
	(1.661)	(1.645)	(1.855)	(1.615)
Observations	249159	47663	5699	41938

Notes: Column (1) include all households surveyed in seven cross-sections, namely 2002, 2004, 2006, 2008, 2010, 2012, 2014. "Lead-Status panels" in Column (2) refers to households observed in round t that can be linked with their program status at year $t + 1$, thanks to the rotating panel structure. These households constitute four two-round panels used in Section 6: 2002-2004, 2004-2006, 2010-2012, 2012-2014 panels. Duplicates of a small number of observations that appear in two overlapping panels (2002-2004/2004-2006 and 2010-2012/2012-2014) are dropped. Column (3) and Column (4) split the same Lead-Status panels in column (2) into households that *will* be accepted and those that *will* be rejected in $t + 1$. Standard deviations are in parentheses.

31.75 hours a week, about 4 hours less than his or her counterpart from a rejected household. On one hand, this gap in work hours could reflect that accepted households tend to experience limited work opportunities or negative health shocks to begin with. On the other, this difference could reflect lower work efforts among beneficiaries of the program. Despite the "lazy poor" critiques from *laissez-faire* economics, there is little evidence on the income effect of targeting benefits in developing countries (Banerjee et al., 2017). However, together with the bunching evidence in the next section (Section 4), the lower labor supply among accepted households here could indicate a distortion due to a discontinuous change in the implicit marginal tax rate on work hours at the income cutoff (Saez, 2010; Chetty et al., 2011; Kleven, 2016; Hanna and Olken, 2018). I show in subsection 4.3 that it is *unlikely* that low-income households reduce their work hours in order to get in the program, therefore their lower labor supply is more likely to be driven by something else, perhaps the exogenous factors listed above.

Table 3 provides summary statistics for the panels by survey year. Breaking the panels by the survey round shows the coverage of the official income cutoff over time. Within the same phase (panels 2002-2004 and 2012-2014), a declining share of households remain under the cutoff. However, when the program transitions to a new phase (panels 2004-2006 and 2010-2012), the share of household under the cutoff increases, as the new and higher cutoff kicks in. This crude fraction, however, does not illuminate whether households manipulate reported income. In the next Section 4, I look more closely at the distribution of reported income to explore whether households bunch at the cutoff, how this behavior changes over time, and whether it responds to cutoff reforms. Another clear pattern to notice here is economic growth over time: per capita (reported) income, housing index and asset index all increase consistently within each panel and across the study period over all. Because economic growth alone may also explain the decline in bunching within a given program phase, in my tests for learning effects in Section 6, I take measures to account for such confounding economic trends.

Table 3: Summary statistics by survey round, panel data

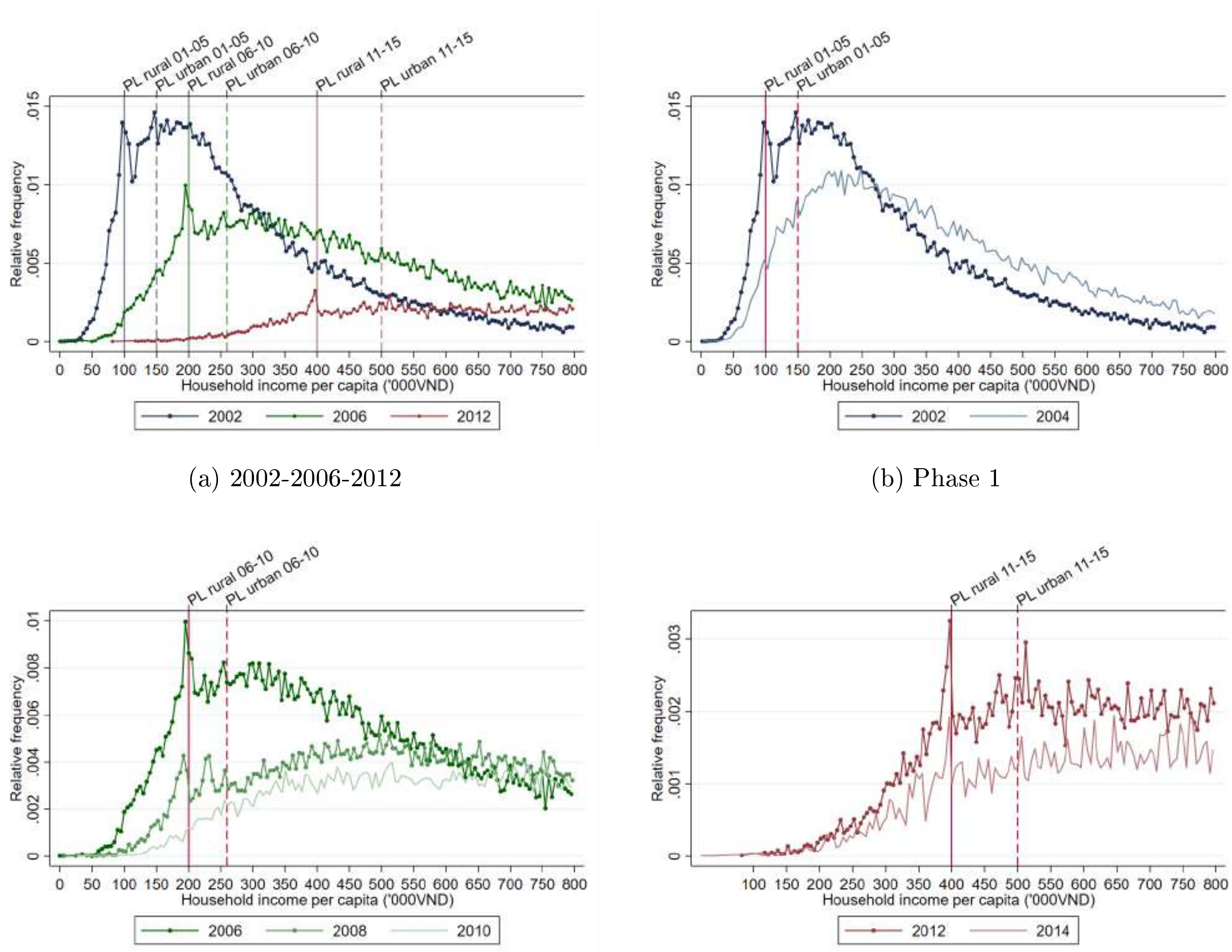
	2002-2004 panel Phase 1		2004-2006 panel Phase 1 to Phase 2		2010-2012 panel Phase 2 to Phase 3		2012-2014 panel Phase 3	
	(1) Base line 2002	(2) Follow-up 2004	(3) Base line 2004	(4) Follow-up 2006	(5) Base line 2010	(6) Follow-up 2012	(7) Base line 2012	(8) Follow-up 2014
Lead-Status = Accept (at t+1)	0.0901	0.114	0.119	0.126	0.141	0.122	0.112	0.102
	(0.286)	(0.318)	(0.324)	(0.332)	(0.349)	(0.328)	(0.316)	(0.302)
Below cutoff	0.0759	0.0305	0.0314	0.104	0.0151	0.0352	0.0438	0.0233
	(0.265)	(0.172)	(0.174)	(0.305)	(0.122)	(0.184)	(0.205)	(0.151)
Reported p.c. income (in 2002 '000 VND)	358.4	416.1	410.2	482.1	612.1	742.9	771.3	865.6
	(455.6)	(1033.2)	(471.1)	(464.2)	(525.7)	(595.8)	(885.2)	(945.8)
Reported p.c. income (in 2002 USD)	23.46	27.23	26.85	31.55	40.06	48.62	50.48	56.65
	(29.82)	(67.62)	(30.83)	(30.38)	(34.41)	(38.99)	(57.93)	(61.90)
Ln reported p.c. income	5.600	5.737	5.741	5.928	6.160	6.367	6.362	6.484
	(0.699)	(0.700)	(0.706)	(0.677)	(0.700)	(0.701)	(0.732)	(0.731)
Asset index	9.540	10.06	10.10	10.47	11.16	11.79	11.84	12.31
	(1.149)	(1.381)	(1.369)	(1.564)	(2.048)	(2.297)	(2.301)	(2.557)
Housing index	51.01	54.34	54.79	58.36	60.62	63.10	63.47	66.94
	(17.68)	(17.32)	(17.55)	(17.37)	(16.40)	(17.21)	(16.84)	(16.56)
Monthly work hours - Head	117.1	127.2	127.8	128.7	160.1	154.6	151.6	150.9
	(76.41)	(82.99)	(82.92)	(83.48)	(100.9)	(98.93)	(97.87)	(98.25)
Monthly work hours - Spouse	124.4	133.4	134.5	134.4	161.7	154.4	148.5	156.2
	(76.04)	(81.69)	(81.04)	(82.99)	(99.91)	(94.27)	(93.72)	(94.74)
Rural	0.764	0.760	0.765	0.746	0.726	0.735	0.735	0.725
	(0.424)	(0.427)	(0.424)	(0.435)	(0.446)	(0.441)	(0.442)	(0.447)
Ethnic minority	0.113	0.108	0.118	0.118	0.144	0.147	0.136	0.132
	(0.316)	(0.310)	(0.322)	(0.323)	(0.352)	(0.355)	(0.343)	(0.338)
Years of education - Head	6.685	6.840	6.997	7.114	7.306	7.326	7.227	7.241
	(3.643)	(3.616)	(3.684)	(3.667)	(3.698)	(3.651)	(3.661)	(3.678)
Household size	4.532	4.407	4.402	4.283	3.964	3.940	3.855	3.773
	(1.786)	(1.764)	(1.708)	(1.680)	(1.519)	(1.563)	(1.498)	(1.558)
Observations	10694	10413	9396	9677	1897	1900	1817	1829

Notes: All columns include households that are surveyed in round t and can be linked with their program status at year $t+1$, thanks to the rotating panel structure. These households constitute four two-round panels used in Section 6: 2002-2004, 2004-2006, 2010-2012, 2012-2014 panels. Columns (1) and (2) split the 2002-2004 panel by the survey round. Similarly, columns (3) and (4) for the 2004-2006 panel, (5) and (6) for the 2010-2012 panel, (7) and (8) for the 2012-2014 panel. Duplicates of a small number of observations that appear in two overlapping panels (2002-2004/2004-2006 and 2010-2012/2012-2014) are dropped. Note that panels for 2006-2008 and 2008-2010 cannot be formed, as explained in the text. Standard deviations are in parentheses.

4 Bunching Evidence

4.1 Empirical Distribution

Figure 3: Empirical distribution of nominal income



(a) 2002-2006-2012

(b) Phase 1

(c) Phase 2

(d) Phase 3

Notes: On horizontal axis is reported per capita income in nominal terms; the unit is thousand VND. The national cutoff levels for rural areas are marked with vertical solid lines, while the cutoff for urban areas are marked with vertical dash lines. Figure 3a reports the empirical distributions for three rounds 2002, 2006, and 2012; each of them is the first round of data observed under each phase of the program. Figure 3b, Figure 3c, and Figure 3d separate the phases and plot all rounds available in each phase. Figure 3b corresponds to Phase 1, Figure 3c Phase 2, and Figure 3d Phase 3.

This section documents the bunching evidence and its temporal pattern. Figure 3 plots the empirical density distribution of reported income. Each line represents the distribution of this variable that is observed in a survey round.[20] Figure 3a reports the empirical distributions for earliest rounds of data observed for each phase of the program. These rounds include 2002, 2006, and 2012, corresponding Phase 1, 2, and 3, respectively. Each phase is subject to a different set official cutoffs that are specific to rural and urban areas. The rural cutoff at the national level is represented by the vertical solid line, while its urban counterpart is captured by the vertical dash line. In the next three graphs in Figure 3b, Figure 3c, and Figure 3d, I plot the distribution of reported income for all rounds of data available under each cutoff regime. Rounds 2002 and 2004 are both observed under Phase 1; 2006, 2008 and 2010 Phase 2; 2012 and 2014 Phase 3.

A few insights emerge from these graphs. First, there is a sharp spike in the distribution that clusters around the rural income cutoff. This spike suggests that a number of households bunch at this cutoff. There seems to be a smaller spike clustering around the urban cutoff as well, however this spike is difficult to discern from noise in many cases.[21] Second, as the cutoff climbs up through each phase, so does the bunching mass. Third, within the same phase, the bunching mass dwindles over time. For example, by the second to last year of the first phase - 2004 - there is hardly any bunching. Note that 2008 has an additional, smaller spike to the right of the first spike at the official cutoff. This second spike is probably driven by a new "near-poor" cutoff introduced in 2009; this new threshold is 30% higher than the pre-exisiting poor cutoff. Overall, the movement in tandem between the cutoff and bunching mass around it, especially right after a hike, suggests a causal relationship between the two.

4.2 Estimation

Specification

I formally quantify the extent of excess bunching documented above in Figure 3. Chetty et al. (2011) developed an estimation approach which has become the standard in the bunching literature. This method requires only a single cross section, using parts of the distribution that are not

[20] I report the plots in nominal terms instead of real terms because some years experienced high inflation (notably 2008) so the bunching mass in the distribution of the deflated variable is not well aligned with the cutoff.

[21] Notice that bunching is only distinguished for the national cutoff (particularly the one applicable to rural areas), but not for any province-level cutoffs. This is reasonable because (i) the number of observations from each province is not large enough to plot a dense distribution at the province level and detect bunching, and (ii) provinces that raises their cutoffs above the national level tend to be richer and have few households that might respond to the cutoff. As a result, the cross sectional variations in cutoff have little value in spotting bunching responses. However, the fact that they are set indicates differences in standards of living across space (at a given point in time). To account for these differences in my empirical estimation of excess bunching below, I recenter the income measure around the cutoff applicable to each province and the rural or urban areas within it.

susceptible to bunching to construct the counterfactual density. I outline here the intuition. To estimate excess bunching, I will need to construct the counterfactual distribution of reported income, in order to compare with the observed distribution. In the absence of the cutoff, households near this cutoff level would have no incentive to manipulate their income and the distribution would have been smooth. Therefore, we can leave out the households located near this threshold, then use those far away from this region, hence not "affected" by the cutoff, to construct the counterfactual distribution.

To account for any differences in the cutoff levels across provinces or urban/rural areas, I recenter the reported income variable around the applicable cutoff, as well as adjust it for differences in price levels over time and across regions. I denote this "adjusted" income y, hence the cutoff $\bar{y}$ equals zero. For each cross section, I fit a polynomial to the observed income distribution, excluding data in the bunching range around cutoff $\bar{y}$, then extrapolate the fitted distribution to the range that has been excluded previously. As commonly done in the bunching literature, the boundaries of the bunching region are manually selected where the bunching spike visually starts and ends — roughly VND 30,000 above and below the cutoff. Denote the left and right boundaries be $\bar{y}_l$ and $\bar{y}_r$, respectively. Grouping households into bin j, I run the following regression:

$$c_j = \sum_{k=0}^{p} \beta_k (y_j)^k + \sum_{b=\bar{y}_l}^{\bar{y}_r} \gamma_b \mathbb{I}\{y_j = b\} + v_j \tag{1}$$

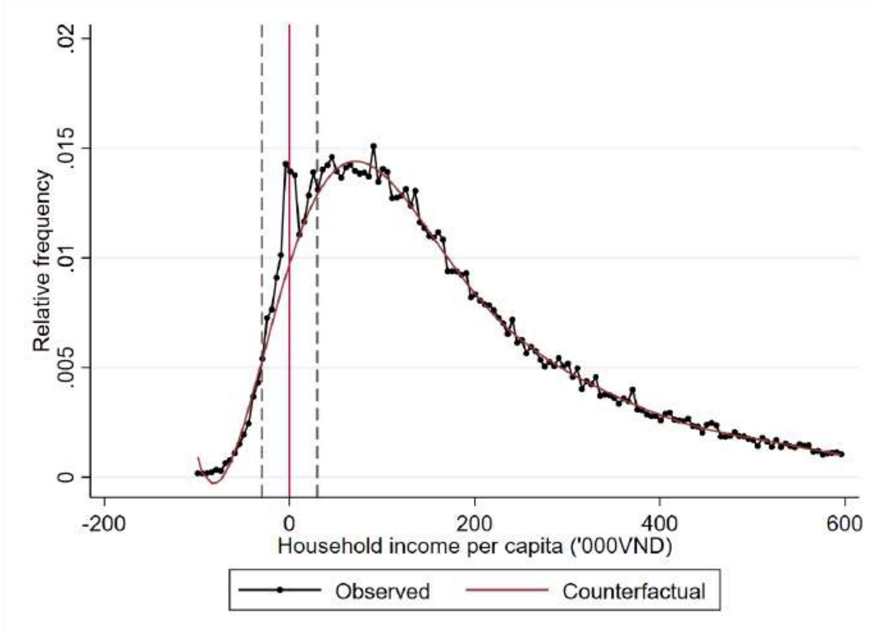
The unit of observation in this regression is a bin in the distribution. c_j is the number of households in bin j of the distribution observed in a cross-section, y_j is the income value of the midpoint of bin j, p is polynomial power. Then the counterfactual density is estimated by predicting the bin counts without using the coefficients on the bunching region dummy: $\hat{c}_j = \sum_{k=0}^{p} \hat{\beta}_k (y_j)^k$. Standard errors are bootstrapped by random resampling from the estimated residuals in Equation 1. The number of bunching households $\hat{d}$ is the difference between the observed and fitted counterfactual distributions:

$$\hat{d} = \sum_{j=\bar{y}_l}^{\bar{y}_r} c_j - \hat{c}_j \tag{2}$$

Results

I illustrate an example of the counterfactual density function in Equation 1. In this figure, I overlay to the counterfactual distribution (black connected line) with its empirical counterpart (red

Figure 4: Counterfactual distribution of real income example, 2002



Notes: Figure 4 overlays the empirical density (black connected line) and estimated counterfactual density (red smooth curve) of reported income for 2002. On horizontal axis is reported per capita income in 2002 terms; the unit is thousand VND. Income is deflated to 2002 terms and also re-centered around the applicable cutoff. The cutoff (zero) is marked with the vertical solid line. The vertical dash lines indicates the boundaries of the bunching region, about VND 30,000. The counterfactual density distribution is estimated with Equation 1.

Table 4: Estimates of excess bunching at the official income cutof

	Phase 1		Phase 2			Phase 3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	2002	2004	2006	2008	2010	2012	2014
Number of excess bunchers	279985.0***	41537.2	272549.2***	257790.8***	-33072.0	224515.4***	119667.8***
	(43753.3)	(35733.5)	(37788.2)	(45229.0)	(31713.4)	(31250.9)	(26165.9)
Share of excess bunchers in population	0.0162***	0.00237	0.0139***	0.0103***	-0.00148	0.00975***	0.00442***
	(0.00253)	(0.00204)	(0.00193)	(0.00180)	(0.00142)	(0.00136)	(0.000967)
Excess bunchers relative to N participants	0.175***	0.0204	0.105***	0.0878***	-0.0115	0.0840***	0.0492***
	(0.0274)	(0.0176)	(0.0145)	(0.0154)	(0.0111)	(0.0117)	(0.0108)
Observations	1000	1000	1000	1000	1000	1000	1000

Notes: This table reports estimates of excess bunching in the reported income distribution, following the procedure described in Equation 1 and Equation 2. Income is deflated to 2002 terms and also re-centered around the applicable cutoff. The number of excess bunchers is the difference between the observed and the counterfactual densities plotted in Figure B3 within the bunching region. The boundaries for this region are VND 30,000 above and below the cutoff. Standard errors in parentheses are bootstrapped by random resampling from the estimated residuals in Equation 1.

smooth curve) for 2002 in Figure 4.[22] As before, the official income cutoff is marked with the ver-

[22]The counterfactual density is estimated with a polynomial of degree nine or ten in Equation 1, depending on the shape of this distribution in a given survey round. Polynomials of a lower degree, for instance degree 8, do not

tical solid line. The vertical dash lines indicates the boundaries of the region where the bunching mass visuall starts and ends, about VND 30,000. In Figure 4, we can visually see the excess bunching mass as the distinct gap between the counterfactual and empirical distributions within the bunching region. Outside this region, the two curve match each other very well. Similar plots for the counterfactual distributions of reported income for all survey rounds are available in Appendix C.

The estimates of the excess bunching mass for each cross section is reported in Table 4. Given the difference in sample size across survey rounds, I also report the number bunching households relative to the population and the size of the program. The share of bunching households in the population ranges between 0.4% and 1.6% and are highly significant, except for 2004 and 2010, when bunching is scant. Relative to the size of the program, the number of bunchers would account for 5-18% the program if they got accepted. Again, these estimates confirms that (i) bunching increases whenever the cutoff goes up, and (ii) over time the same cutoff is associated with less bunching.

The pattern of depleting bunching throughout each phase suggests that some countering force may have evolved dynamically and deterred households' behavioral response. Such a force could make the income criterion less binding and result in a smaller amount of bunching. I propose a possible counter force that could gradually discourage household strategic behaviors. While reported income could be easily be altered, some other criteria, specifically, housing conditions are much harder to manipulate. As time goes by, if the local officers learned to rely more on observed housing conditions to gauge the true income, such information revelation could drive away the incentive for households to misreport income. In Section 5, I show analytically how this works and derive theoretical predictions to test the model in the data. To inform the modeling assumptions on the cost of manipulation, below I verify whether households bunch by simply misreporting their income or by reducing their labor supply. The latter would incur an additional cost to society, as some real income will be lost.

4.3 How do households bunch?

The main goal of this exercise to get an idea of whether bunching is driven by misreporting income or by real behavior such as reduction of work efforts. Households may simply misreport their income to appear eligible. However, they may also cut down labor supply in order to earn

fit the empirical density function well enough, while polynomials of degree 11 or more do not improve the fit much more than polynomials of degree 9 or 10.

a lower income and stay "in line" with the cutoff. If households truthfully report their income, then bunching would be consistent with such labor supply response and would imply lower total output.

To distinguish labor supply response from misreporting, I conduct a Regression-Discontinuity (RD) type analysis. This test intuitively relies on the fact that the portion of bunchers in the mass just below the cutoff tend to be larger than in the mass just above the cutoff, as illustrated in Figure 9 of Section 6. Because the cutoff is set on the increasing portion of the density distribution of income, even if bunchers are equally distributed on either side of the cutoff, the mass of nonbunchers just below the cutoff is still smaller than its counterpart just above the cutoff. As a result, bunchers will make up a relatively larger share to the left of the cutoff than to its right. If most bunchers reduce their labor supply, then households who end up just below should work less than those remaining above the cutoff. Note that my goal here is not to identify a treatment effect of the targeting program by comparing households around the cutoff, but to capture what drives the manipulation of income. If we detect discontinuity in labor supply at the cutoff, we will have suggestive evidence that households shade their income by reducing their work efforts. If not, bunchers are likely to just misreport their earnings.

Pooling all years together, I fit a parametric relationship between work hours and reported income as summarized in Equation 3. I allow for different polynomials to fit each side of the cutoff, as well as a dummy for whether reported income is below the official cutoff to capture any discontinuity right at this threshold.

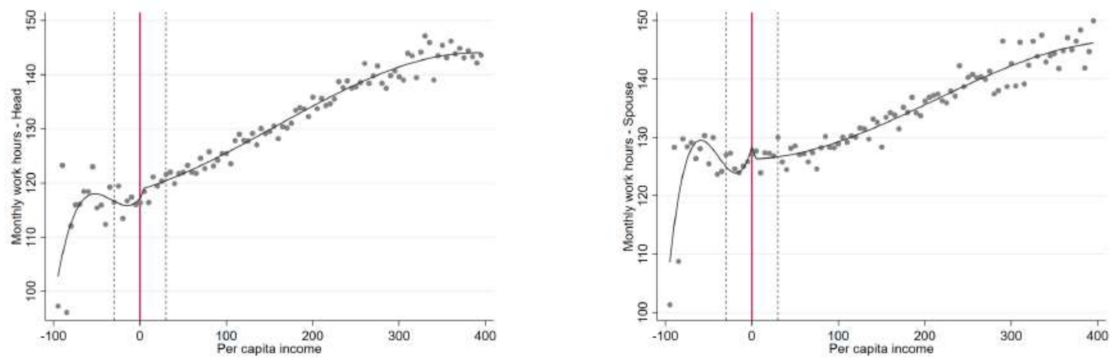
$$hours_{ipt} = \gamma_0 + \gamma_1 \mathbb{I}\{y_{ipt} \leq \bar{y}\} + \gamma_2 (y_{ipt})^k + \gamma_3 \mathbb{I}\{y_{ipt} \leq \bar{y}\}(y_{ipt})^k + \gamma_4 X_{ipt} + \phi_p + \tau_t + \epsilon_{ipt} \tag{3}$$

For this specification, the unit of observation is a household i in province p in year t. $hours_{ipt}$ is labor supply measured by work hours of the main income earners in the households, namely, head and spouse. As before, y_{ipt} is reported per capita income, $\mathbb{I}\{y_{ipt} \leq \bar{y}\}$ is below-cutoff indicator, k is the degree of the polynomial. Covariates X_{ipt} controls for individual and household characteristics that could correlate with both household income and the labor supply of the main income earners. This includes head's education attainment, head's gender, head's age and its square, urban dummy, minority dummy, household size, household composition measured by the shares of children below 6 and elderlies above 65. I also add province and survey-round fixed effects to account for differences in the cutoff and general economic conditions across provinces and changes in the income reporting behavior over time. $\gamma_1 < 0$ would suggest that households who bunch at the cutoff work less than those who remain above it.[23]

Figure 5: Labor supply around the income cutoff

(a) Hours worked - Head

(b) Hours worked - Spouse



Notes: On the horizontal axis is reported per capita income in thousand VND, deflated to 2002 terms and also recentered around the applicable cutoff. The income cutoff is marked with the vertical solid lines, while boundaries for bunching region at VND 30,000 above and below the cutoff are marked with vertical dash lines. Each graph pools data from all years and plots the relationship between the variable on the vertical axis and household income. A cubic function is fitted separately on each side of the cutoff and a below-cutoff dummy is included to capture any jump at the cutoff level. The sample is restricted to households with reported income within VND 400,000 of the cutoff to better fit the cubic function near the cutoff.

I fit cubic relationships and present the results in Figure 5 and Table 5.[24] In Figure 5, there is virtually no discontinuity at the cutoff when looking at the relationship between income and working hours of the main income earners. Although there is a dip in labor supply around the cutoff, this seems to be the result of a few noisy observations toward the very left end of the distribution of reported income. These households tend to experience temporary negative shocks, possibly explaining their low income despite the slightly greater working hours. These results indicate that the bunching response does not reflect a reduction of labor supply, but rather a matter of misreporting. Table 5 reports the RD estimates of work hours for head and spouse in the first two column. Consistent with the lack of discontinuity in Figure 5, columns (1) and (2) suggest that the main income earners in households just below the cutoff only work half an hour (per month) less than those located just above it. This is insignificant both statistically and economically. In column (3), I average the hours across the two main income earners. The difference in

[23] Because the official cutoff is set at quite a low level of income, I restrict the sample here to households with reported income no more than VND 400,000 above the cutoff. This allows the right-side parametric line to fit better near the boundary (and not be biased by observations very far away from it.). The resulting sample covers 81% of the 2002 round, 78% of the 2004 round, 76% of the 2006 round, 64% of the 2008 round, 60% of the 2010 round, 51% of the 2012 round, and 41% of the 2014 round. The coverage decreases monotonically across the rounds because the distribution of income shifts up over time.

[24] As seen in Figure 5, the cubic curve fits the the scatter points relatively well. Polynomials of higher degree tend to overfit this relationship, especially on the left of the official income cutoff (solid vertical line) where the number of of observations is small. Meanwhile, a lower polynomial, such as linear or quadratic curves do not fit the scatter points well enough and result in overestimation of the discontinuity at the cutoff.

Table 5: Labor supply around the official income cutoff

	Global cubic fit on each side			Local linear regressions	
	(1) Hours Head	(2) Hours Spouse	(3) Avg. Hours Head & Spouse	(4) Avg. Hours Head & Spouse	(5) Avg. Hours Head & Spouse
Below cutoff	-0.625	-0.0508	-1.037	1.416	1.646
	(0.900)	(0.995)	(0.839)	(1.985)	(1.101)
Observations	231903	184012	176077	22990	49235
Mean DV at cutoff	117.392	127.375	139.49	139.49	139.49
Mean of DV	129.966	133.462	150.972	150.972	150.972
Prov and Round FEs	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓

Notes: All columns report parametric estimates for discontinuity in work hours at the income cutoff. Reported per capita income is deflated to 2002 terms and also re-centered around the applicable cutoff. Columns (1)-(3) fit a cubic function separately on each side of the cutoff and a below-cutoff dummy is included to capture any jump at the cutoff level. The sample is restricted to households with reported income within VND 400,000 of the cutoff to better fit the cubic function near the cutoff. Columns (1) and (2) report the RD estimate for monthly work hours of household head and spouse with pooled data from all years, respectively. Columns (3) reports the same estimate, but for work hours averaged between head and spouse. With the same dependent variable as in Column (3), Columns (4) and (5) report RD estimate using local linear regressions within a small bandwidth of the income cutoff. In Column (4), the bandwidth size is set to VND 30,000 above and below the cutoff, so that this bandwidth matches the region where the bunching spike visually starts and ends. In Column (5), the bandwidth size (common for both sides) is selected optimally to minimize means squared errors. All regressions control for head's education attainment, head's gender, head's age and its square, urban dummy, minority dummy, household size, household composition measured by the shares of children below 6 and elderlies above 65, province fixed effects and survey round fixed effects. Robust standard errors are in parentheses.

the average working hours for a main income earner between two sides of the cutoff is only one hour per month, still an insignificant estimate both statistically and economically. The same RD estimates are also available for each survey round in Table C1, again they confirm that there is little significant difference in work hours (averaged between head and spouse) between two sides of the cutoff in any given round.[25]

I also examine the RD estimate with local linear regression in the last two columns of Table 5. With average work hours between head and spouse as the dependent variable, column (4) reports this estimate for a local bandwidth of VND 30,000, i.e. the same boundaries used to estimate excess bunching in Section 4. Column (5) chooses the bandwidth (common for both sides) optimally by minimizing the mean squared error of the RD estimate, as discussed in Calonico et al. (2019). Again, the RD estimates in these two columns are small and imprecise. This lack of evidence of labor supply distortion is consistent with findings from similar programs in other developing countries (Banerjee et al., 2017; Hanna and Olken, 2018).

[25]The only year for which this estimate is statistically significant is 2004, however, its sign is opposite of what we expect.

The results here suggest that households are more likely to shade their income to the cutoff level by merely misreporting them. The next section takes this stand when setting up the theoretical model to explain the temporal pattern of bunching.

5 Theoretical Framework

To explain the decaying pattern of bunching over time, I propose that the officers in charge of screening households learn to rely on another criterion that is less susceptible to manipulation. This in turns deters households from continuing to shade their income. I build a simple model of household signaling and officer learning. The game has two players – the household and the targeting officer. In each period, players move sequentially. The household knows its true income, but the officer only knows the distribution of income, thus the household could misreport its income to appear eligible. In addition to income reported by the household, the officer also receives another independent signal that correlates with household's true income. This signal is public information that arrives to both players at the beginning of each period, so the household observes it before reporting its income. This ordering of events emulates the essential features of the selection process described in Section 2: the independent signal represents the first screening step based on non-income criteria, followed by the second step where the household self-reports income. The officer receives these two sources of information, then decides whether to accept the household to the program. In this framework, the following Bayesian Nash equilibrium will emerge in each period:

(i) Households with higher chance of passing off as poor (according to the independent signal) and low enough cost of misreporting shade their earnings to the cutoff level.

(ii) The officer accepts all bunchers if the expected income, given the information up to date, falls below the official cutoff.

To model learning, I allow the public independent signal to get more precise after each period. Thus this learning process is public, that is, if the officer gets better at inferring true income from housing conditions, the household is also aware of the officer's updated information set. This leads the officer to increasingly rely on the independent signal to target households, thereby drives away the incentive for households to continuing to manipulate their income. One important requirement for the independent signal to be a reliable source of information for the officer is that it cannot be manipulated by households. Therefore I measure this signal with physical housing conditions, because they are visually inspected by the officers (and surveyors of VHLSS) thus are *unlikely* to be manipulated by households. Going forward, I refer to the independent signal component as the housing signal.[26]

In essence, we could imagine that the selection officers have an idea of how to gauge true income from the physical housing conditions, i.e. a mapping between housing and true income with some noise. If this mapping gets relatively more accurate, housing conditions will play a more important role in the selection process. How learning actually occurs to improve this mapping can take different forms, such as learning by doing or learning from the villagers. I abstract from this process to focus on what would happen *if there was learning*. Therefore, I take a more general approach to model public learning: by assuming that the precision of the commonly-observed housing signal improves *exogenously* with time. In the context of predominantly rural Vietnam, this assumption is quite plausible. Vietnamese households live in tight-knit communities, thus neighbors presumably know about each others' livelihood very well.[27] Anecdotal stories in the media report that households file complaints when they think they are more deserving than accepted households. To better understand the program implementation, I had conversations with officers on the field. They confirm that households seem to believe the program is a zero-sum game. Therefore, it is reasonable in this context to assume that neighbors have an incentive to reveal their private information about their neighbors to the officer. At the same time, the frequent communal contacts also suggest that the updates revealed to the officer are also shared among villagers. Thus, the context at hand can reasonably justify my general approach to model the public learning process.

Overall, the model results in the following testable predictions to bring to the data in Section Section 6.

1. Bunching in each period is more prevalent if the realization of the housing signal is low.

2. As housing signal precision increases over time, bunching mass decreases; and

3. For the same reason, the certification decision is increasingly dependent on housing signal.

[26] Note that the screening step also considers assets (and possibly contemporary shocks). However, these information (if observable) are self-reported in my data, thus they could suffer from misreporting if households believe VHLSS can impact their chances of getting into the program. Without loss of generality, I do not explicitly include such self-reported non-income variables in the model, but I control for them (to the best of my ability) in the empirical tests.

[27] It is well-known that rural communities in developing countries often live in close proximity to one another and maintain strong ties within with their social groups. In fact, there is a large literature that has developed models of social learning within such communities (Foster and Rosenzweig, 1995; Munshi, 2004; Bandiera and Rasul, 2006; Conley and Udry, 2010; Adhvaryu, 2014).

5.1 Setup

Formally, the game is played between two players - the household and the targeting officer – over several periods in a given phase of the program. Households have private information about their true income, i.e. their "type", but the officer only know the distribution of types. Indexing households by subscript i, I assume their true income y_i is identical and independently distributed with $F(y)$. As typically assumed for income, y_i is assumed to follows a log-normal distribution, thus its natural log, $\theta_i = \ln y_i$, is normally distributed.

At the beginning of each period t, both players receive a public housing signal: a noisy mapping between true log income and housing conditions that takes the form $h_{it} = \theta_i + \epsilon_{it}$, where ϵ_{it} is i.i.d. with mean 0, serially independent, and independent from everything else. The distribution of ϵ_{it} is also bounded, which enables the officer to define a threshold for housing conditions to screen out some households. As mentioned earlier, this signal represents the information the officer gathered during the initial screening step during the house visits. After the arrival of the signal, players move sequentially: the household first reports an income to the officer, then the officer decides whether to accept it to the program. The structure of the game and all payoff functions are known to both players. At their turn, each player maximizes their expected utility, given their belief about by the other player's strategy. After their turns are finished, the game repeats in the next period, but with a more precise housing signal.

Each household chooses an income level $\hat{y}_{it}$ to report to the officer, which is received as $\tilde{y}_{it} = m_{it}\hat{y}_{it}$, where $m_{it} > 0$ represents the *accidental* misreporting rate.[28] Assuming m_{it} is also log-normal, then operationally, each household chooses to report a log income level $\hat{\theta}_{it} = \ln \hat{y}_{it}$, which arrives to the officer as $\tilde{\theta}_{it} = \ln \tilde{y}_{it} = \hat{\theta}_{it} + \eta_{it}$, where $\eta_{it} = \ln m_{it}$. The reporting errors η_{it} reflects the calculation mistakes that arise when households report income from several sources. This random reporting error enables a realistic prediction of bunching that matches the data: the observed messages are bunched up *with some noise* around the official cutoff. It also has similar independence properties like the signal noise ϵ_{it}, but its precision will not change over time, so hereafter I omit its t subscript and denote it as η_i .

A household with true log income θ_i will incur a cost $c = (\hat{\theta}_{it} - \theta_t)^2$ if it misreports income by sending the message $\hat{\theta}_{it}$.[29,30] Since $c = (\hat{\theta}_{it} - \theta_t)^2 = (\ln \hat{y}_i - \ln y_i)^2$, this cost represents the squared

[28] For example, a given reported income can arrive as 10% lower if $m_{it} = 0.9$ and 10% higher if $m_{it} = 1.1$.

[29] The cost is symmetric for both under-reporting and over-reporting. However, only the under-reporting constraint is binding for the marginal households.

[30] This cost function satisfies the single-crossing property. I choose this functional form to model misreporting, as suggested by the empirical evidence in subsection 4.3. A more general cost function $c(\hat{\theta}, \theta)$ with $c_{\hat{\theta}}(\hat{\theta}, \theta) > 0$ and

percentage difference between the household's message and its true income in levels. If accepted, the household gets positive payoff from a lump sum transfer. Denote the natural log of this lump sum transfer with T, the household's utility function is given by:

$$u_t(\theta_i, \hat{\theta}_{it}, d_{it}) = \mathbb{I}\{d_{it} = \mathit{Accept}\}T - (\hat{\theta}_{it} - \theta_i)^2 \tag{4}$$

where t indicates the relevant time period and d_{it} is the officer's action in this stage game.

The officer cares about selecting the "*right*" households, in terms of true income. After receiving the housing signal and the message from the household, she decides whether to *Accept* or *Reject* the household. Let $\bar{y}$ be the official income cutoff level postulated by the central government, and $\bar{\theta} = \ln \bar{y}$ be its natural log. Then, $\bar{\theta} - \theta_i = \ln \bar{\theta} - \ln y_i$ represents the percentage difference between the official cutoff and the household's true income. The officer's utility function is summarized as:

$$v_t(\theta_i, d_{it}) = v_t^{d_{it}} = \mathbb{I}\{d_{it} = \mathit{Accept}\}(\bar{\theta} - \theta_i)$$

$v_t(\theta_i, d_{it})$ reflects that the officer gets positive utility only if she accept households who is truly below the cutoff, and poorer households yield her higher utility. Conversely, admitting households whose true income is a above the cutoff yield negative payoff, and the richer they are the worse her payoff. However, she earns zero utility if rejecting the household. It follows that with full information, she would accept only households with true income below the cutoff, since $v_t^A \geq v_t^R \Leftrightarrow \bar{\theta} - \theta_i \geq 0 \Leftrightarrow \theta_i \leq \bar{\theta}$ for $d_{it} \in \{\mathit{Accept}, \mathit{Reject}\}$. With imperfect information, the officer will compare her expected payoff from each option. As it will become clear below, this implies that she will accept the household if the expected type of the household, given the current information, is below the cutoff.

Going forward, the model will utilize only the log of income-related variables, therefore hereafter, I refer to θ_i as the household's type or true income, $\hat{\theta}_{it}$ as the income reported by the household, $\tilde{\theta}_{it}$ as the version of this message received by the officer, and $\bar{\theta}$ as the official income cutoff.

The sequences of events in each stage game t is summarized as below:

Timeline in every period t:

1. Both the officer and the household observe an independent signal $h_{it} = \theta_i + \epsilon_{it}$.

$c_{\hat{\theta}\theta}(\hat{\theta}, \theta) > 0$ could model a *real* reduction in labor supply.

2. The household sends a message $\hat{\theta}_{it}$ (report its income) to the officer.

3. The officer receives $\tilde{\theta}_{it} = \hat{\theta}_{it} + \eta_i$, and decides to choose $d_{it} \in \{Accept, Reject\}$.

Information Structure

Let $I_{i,t-1} \equiv \{(h_{i0}, \tilde{\theta}_{i0}), ..., (h_{i,t-1}, \tilde{\theta}_{i,t-1})\}$ denotes the history of all information up to the previous period $t-1$. The common prior at the beginning of period t is $f(\theta_i|I_{i,t-1})$, and the officer's posterior belief after observing of the housing signal h_{it} and the message $\tilde{\theta}_{it}$ is $f(\theta_i|I_{i,t-1}, h_{it}, \tilde{\theta}_{it})$. She updates her beliefs with Bayesian updating as followed:

$$
\begin{aligned}
f(\theta_i|I_{i,t-1}, h_{it}, \tilde{\theta}_{it}) &= \frac{f(h_{it}, \tilde{\theta}_{it}|\theta_i, I_{i,t-1})f(\theta_i|I_{i,t-1})}{f(h_{it}, \tilde{\theta}_{it}|I_{i,t-1})} \\
&= \frac{f(\tilde{\theta}_{it}|\theta_i, I_{i,t-1}, h_{it})f(h_{it}|\theta_i, I_{i,t-1})f(\theta_i|I_{i,t-1})}{f(\tilde{\theta}_{it}|I_{i,t-1}, h_{it})f(h_{it}|I_{i,t-1})} \\
&= \frac{f(\tilde{\theta}_{it}|\theta_i, I_{i,t-1}, h_{it})f(h_{it}|\theta_i, I_{i,t-1})f(\theta_i|I_{i,t-1})}{\int_{\theta_i} f(\tilde{\theta}_{it}|\theta_i, I_{i,t-1}, h_{it})f(h_{it}|\theta_i, I_{i,t-1})f(\theta_i|I_{i,t-1})d\theta_i}
\end{aligned}
\tag{5}
$$

The expression following the second equality breaks down the posterior into two sequential sources of information in period t, the physical housing conditions and the income reported by the household (conditioning on the realization of housing). As shown in Equation 5, when $f(h_{it} = \theta_i|\theta_i, I_{i,t-1})$ is large (high precision), the posterior tends to 1, thus housing conditions will have a greater influence on the officer's decision. On the other hand, if $f(h_{it} = \theta_i|\theta_i, I_{i,t-1})$ is small (low precision), the posterior would shrink to 0, making housing conditions unreliable to the officer.

5.2 Equilibrium in each period t

The household

The household's problem can be summarized as:

$$\max_{\hat{\theta}_{it} \in \{\theta_i, \bar{\theta}\}} E_{d_{it}}[\mathbb{I}\{d_{it} = Accept\}T - (\hat{\theta}_{it} - \theta_i)^2]$$

This statement indicates that the household chooses a message, $\hat{\theta}_{it}$, to maximize its expected utility. This expected utility increases with the receipt of program benefits T, which is contingent on the officer's decision,d_{it}, and increases in the psychic cost, $(\hat{\theta}_{it} - \theta_i)^2$. To keep the model tractable, I limit the action space of the household to either reporting its true income, θ_i ,or shading to the official cutoff, $\bar{\theta}$. Given this setup, there are two factors affecting the behaviors of households: the psychic cost when the household misreports its income, and the housing signal that limits who can possibly pass off as someone below the official income cutoff.

Figure 6 illustrates the impact of the psychic cost (that is directly dependent on true income). This figure plots the psychic cost and program benefit functions for a given type, with log true income on the horizontal axis and log benefit or cost on the vertical axis. The black quadratic curve shows the psychic cost when a household with true income equal to $\bar{\theta}$ misreports its income. This cost is zero if the household (intend to) send an honest message $\hat{\theta}_{it} = \bar{\theta}$, but gets larger as the household diverges from its true income. The program benefit is depicted by a horizontal like at T that extends to only the official income cutoff $\bar{\theta}$. Households of type $\bar{\theta}$ do not have an incentive to shade there income, because if they tell the truth, they still get accepted to the program and enjoy the benefit T. For the same reason, all types below $\bar{\theta}$ also report an honest message

However, some households with true income above $\bar{\theta}$ will find it profitable to shade their income to the official cutoff, i.e. $\hat{\theta}_{it} = \bar{\theta}$, because the program benefit may exceeds the cost of misreporting. The red quadratic curve represents the same cost function for the highest type who could afford to bunch, θ_t^*. For this type, the cost of misreporting exactly equals the benefit, depicted by the intersection between the red curve and the horizontal benefit line. All households whose true income lie between $\bar{\theta}$ and θ_t^* could potentially bunch, because their cost of shading income to $\bar{\theta}$ is lower than the benefit T when accepted (in equilibrium). For all households above θ_t^*, their psychic cost curve will not touch the horizontal benefit line, thus bunching at $\bar{\theta}$ will cost them more than the potential benefit T. Therefore, these households find it too costly too bunch.

In addition, the household's action is also dependent on the independent housing signal, which the household cannot manipulate. Thus the realization of the housing signal h_{it} has two consequences on the household's action: (i) it effectively refines the marginal type by lowering the probability of getting accepted; and (ii) it limits the ability to bunch for a given type (who could bunch). These effects can be seen in Figure 7. This figure describes the equilibrium behavior of households, which depend on their true income (horizontal axis) and their housing conditions (vertical axis). Here, the 45°line represents the perfect mapping between housing and true income. The housing signal is noisy, implying that a given type θ_i, in the view of the officer, can reside in a range of housing conditions. This range for a given type is captured by the height of the colored area at a given value of true income. For example, type $\bar{\theta}$'s housing conditions could range from h_t^{**} to h_t^*.[31]

[31] These bounds are defined by $h_t^* = \bar{\theta} + \epsilon_t^*$ and $h_t^{**} = \bar{\theta} - \epsilon_t^*$, where $-\epsilon_t^*$ and ϵ_t^* are the bounds of the signal noise. One could define $-\epsilon_t^*$ and ϵ_t^* as the 1st and 99th percentile of the untruncated distribution of ϵ_t.

Figure 6: Psychic cost in households's problem

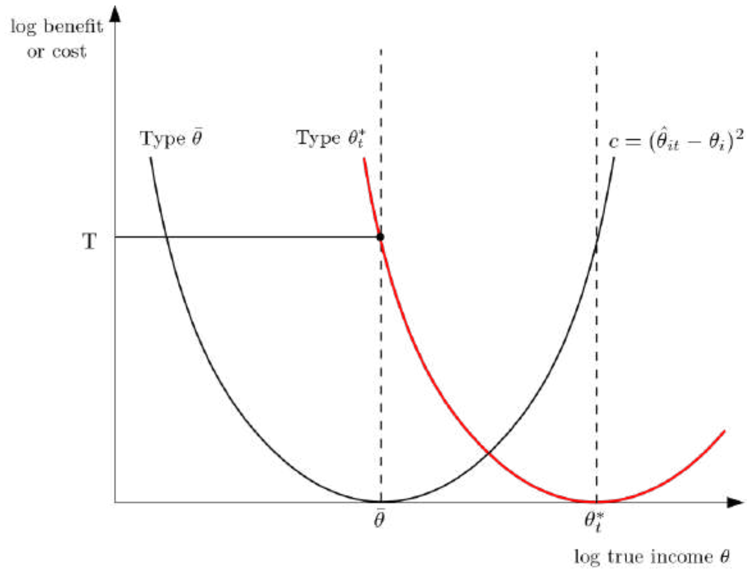
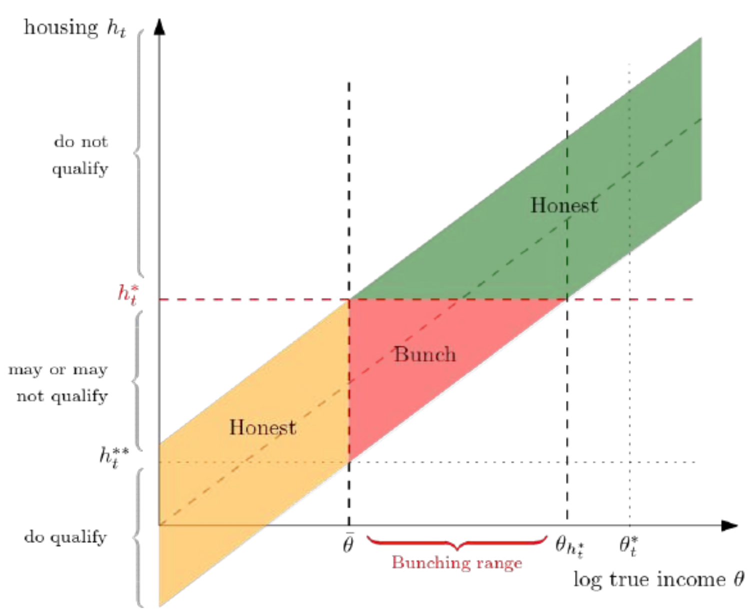


Figure 7: Housing signal and the marginal household



The upper bound h_t^* defines the highest housing conditions that the officer would believe a household of type $\bar{\theta}$ could live in. However, some households with true income above $\bar{\theta}$ may still get a low draw of housing conditions that could qualify them for the program. In particular, any households with a realization of $h_{it} \leq h_t^*$ has a chance of passing off as $\bar{\theta}$. Let $\theta_{h_t^*}$ be the highest type who could do so. Assuming $\theta_{h_t^*} \leq \theta_t^*$ (the marginal type defined earlier by the psychic cost), then the housing signal further refines the value for marginal household.[32] All households to the right of the marginal type $\theta_{h_t^*}$ cannot bunch, either because their psychic costs are too high, or their housing conditions look too good. Importantly, some households in the potential bunching range $[\bar{\theta}, \theta_{h_t^*}]$ cannot bunch either, because their housing conditions turns out to exceed h_t^*. Taken together, all households with housing conditions above h_t^*, captured by the green area in Figure 7, tell the truth.

The households represented by the red area in Figure 7 have both low enough psychic costs and low enough housing conditions, thus they bunch. Finally, the yellow area describes households with true income below the official cutoff, plus they surely get a low enough draw of housing conditions. Such households need not lie, so they also report truthfully.

These effects of the housing conditions on the bunching behavior imply that households need to have relatively low housing conditions in order to convincingly appear as the cutoff type $\bar{\theta}$. Thus, it follows that:

Proposition 1. *Bunching in each period is more prevalent if the realization of housing signal h_t is low.*

The officer

The officer observes the housing signal $h_{it} = \theta_i + \epsilon_{it}$ and the message from the household with reporting errors $\tilde{\theta}_{it} = \hat{\theta}_{it} + \eta_i$, then she calculates her expected payoff and accepts household if:

$$E[\bar{\theta} - \theta_i | I_{i,t-1}, h_{it}, \tilde{\theta}_{it}] \leq 0 \tag{6.1}$$

$$\Leftrightarrow E_{g(\theta_i)}[I_{i,t-1}, \theta_i | h_{it}, \tilde{\theta}_{it}] \leq \bar{\theta} \tag{6.2}$$

$$\Leftrightarrow \int_{-\infty}^{\tilde{\theta}_t^*} \int_0^{h_t^*} \int_{-\infty}^{\theta_{h_t^*}} \theta_i g(\theta_i, h_{it}, \tilde{\theta}_{it}) d\theta_i dh_{it} d\tilde{\theta}_{it} \leq \bar{\theta} \tag{6.3}$$

[32] This assumption helps simplify the derivation of the officer's estimation of the household's true type in subsection 5.4, which relies on the housing condition threshold h_t^* to infer the effective marginal bunching type $\theta_{h_t^*}$. Technically, the marginal type could be θ_t^* or $\theta_{h_t^*}$, whichever is lower.

where $g(.)$ is the density function of the joint distribution of θ, h_t and $\tilde{\theta}_t$.[33] When maximizing her expected payoff (Equation 6.1), the officer effectively estimates the true income of the household (Equation 6.2), given her observations of housing conditions and reported income, then compares this estimate with the official cutoff $\bar{\theta}$. This expectation is computed by integrating over the joint distribution of type θ_i, housing conditions h_{it}, and reported income $\tilde{\theta}_{it}$, up to the marginal values for each variable (Equation 6.3).[34]

In particular, h_t^* denotes the marginal housing conditions. Recall it is the upper bound of the housing conditions h_{it} in which a household earning a true income of $\bar{\theta}$ could live in. Since the officer wants to admit households with true income no greater than this level, she would only consider those with housing conditions below h_t^*. In addition, she can infer that the true income of these potentially eligible households will not exceed the marginal type $\theta_{h_t^*}$, because those with income higher than $\theta_{h_t^*}$ will surely get housing conditions above h_t^*.[35] In addition to housing conditions, the officer also incorporates information from reported income. Because its distribution is unbounded, the received message, $\tilde{\theta}_{it}$, due to calculation mistakes, could erroneously end up at a very high value, even though the household intends to send a low message. In such a case, the officer may reject the household. Therefore, there is also a marginal threshold in terms of reported income $\tilde{\theta}_t^*$, below which she would accept the household.

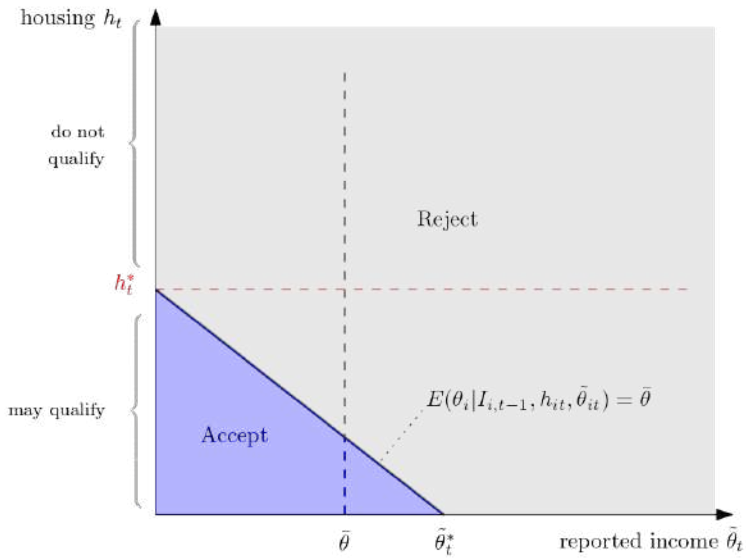
In Figure 8, I illustrate her decision as summarized in Equation 6, with regards to the two variables she observes, namely housing conditions (vertical axis) and reported income (horizontal axis). Her marginal decision is depicted by the diagonal line in Figure 8, which crosses the housing conditions and reported income axes at their respective thresholds. The combination of message and housing conditions in the area below this line (colored purple) indicates acceptance, while the combination in the area above it (colored gray) denotes rejection. Moving from the purple area in the bottom left to the gray area in the top right, both housing conditions and reported income increase in value. Thus, the officer estimate of the household's true income based on these information, $E[\theta_i|I_{i,t-1}, h_{it}, \tilde{\theta}_{it}]$, also increases. When this estimate exceeds the official cutoff $\bar{\theta}$ as stated in Equation 6, this decision switches from *Accept* to *Reject*.

[33] $g(\theta_i, h_{it}, \tilde{\theta}_{it})$ is rescaled by $F_{\theta, h_t, \tilde{\theta}_t}(\theta_{h_t^*}, h_t^*, \tilde{\theta}_t^*)$.

[34] To keep the exposition clean, Equation 6.3 uses the true distribution of types $f(\theta_i)$ as the prior. Technically, the officer does this estimation in every period, so the prior should be her belief about the household up to the previous period $f(\theta_i|I_{i,t-1})$. Adding this element, however, complicates the expression, but will not change the implications on the marginal values.

[35] These types are illustrated by the green mass to the right of $\theta_{h_t^*}$ in Figure 7, whose housing conditions are distributed above h_t^* according to the data generation process.

Figure 8: Officer's decision



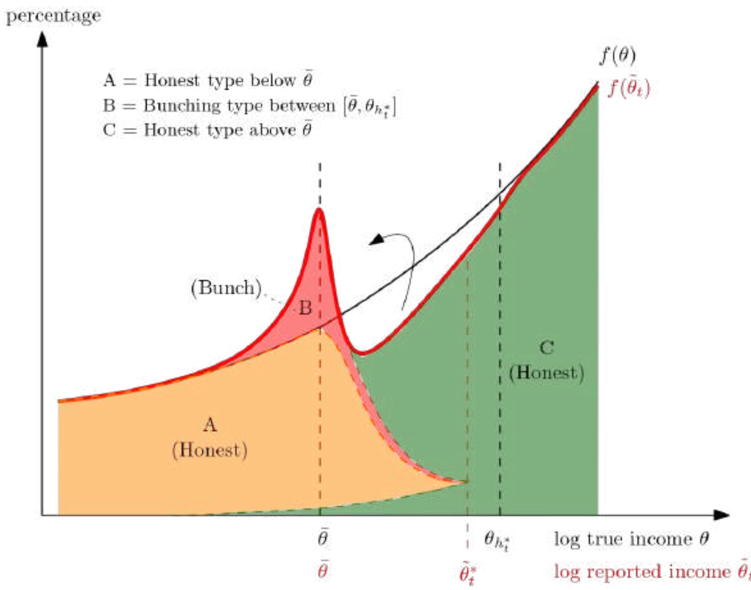
5.2.1 Equilibrium in the data

The previous subsection highlights the equilibrium behaviors of the household and the officer. It is useful to see how these results match the observed data. In Figure 9, I overlay the density distribution of true income $f(\theta_i)$ (black solid curve) with the density distribution of reported income $f(\tilde{\theta}_{it})$ (red solid curve).[36] While the density of true income is smooth, households in the range $[\bar{\theta}, \theta_{h_t^*}]$ with housing conditions below h_t^* shade their income to the cutoff because these households have an incentive to bunch. Due to reporting errors, the bunching messages may not land precisely at $\bar{\theta}$, but end up some where near it. Thus, the density of reported income has a bunching mass around $\bar{\theta}$, similar to what we observed in the empirical distribution plotted in Figure 3 (when bunching is prevalent).

The color-coded areas here correspond to the household behaviors described in Figure 7. Area A (yellow) are the households with true income $\theta_i \leq \bar{\theta}$; they send truthful messages that could end up above the official cutoff because they may make calculation errors when tallying up income from different sources. Similarly, area B represents the bunching households, whose true type is above $\bar{\theta}$, but report an income lower than they have. They intend to bunch right at $\bar{\theta}$, and on average they do end up there, but the calculation errors could land them anywhere else, thus their reported income distributed near the cutoff. Area C represents households with true income above

[36]For clarity, this figure only zooms in the area near the official income cutoff $\bar{\theta}$, which is set at bottom of (true) income distribution.

Figure 9: Bunching equilibrium in the data



$\bar{\theta}$ who remain truthful because of high housing conditions or high cost of misreporting.[37] The officer cannot distinguish households in mass A and mass B because their housing conditions and reported income are low enough for her to believe that they are poor. However, she can screen out those in mass C due to their high housing conditions. We can rewrite the officer's acceptance condition in Equation 6 to incorporate these insights:

$$E_{g(\theta_i)}[\theta_i | I_{i,t-1}, h_{it}, \tilde{\theta}_{it}] \leq \bar{\theta}$$
$$\Leftrightarrow \underbrace{\int_{-\infty}^{\tilde{\theta}_t^*} \int_{-\infty}^{h_t^*} \int_{-\infty}^{\bar{\theta}} \theta_i g(\theta_i, h_{it}, \tilde{\theta}_{it}) d\theta_i dh_{it} d\tilde{\theta}_{it}}_{A} + \underbrace{\int_{-\infty}^{\tilde{\theta}_t^*} \int_{-\infty}^{h_t^*} \int_{\bar{\theta}}^{\theta_{h_t^*}} \theta_i g(\theta_i, h_{it}, \tilde{\theta}_{it}) d\theta_i dh_{it} d\tilde{\theta}_{it}}_{B} \leq \bar{\theta} \quad (7)$$

This equation re-expresses the integration in Equation 6.3 as the weighted average between the true income of households in mass A and the true income of households in mass B in Figure 7. Again, both of these types show up in the officer's estimation, because she cannot tell them apart.

[37]Note that in this illustration, the masses A, B, and C still have unlimited tails, but the mass on these far ends are negligible. The marginal message $\tilde{\theta}_t^*$ that the officer accepts is likely to be set where the upper tail of masses A and B is small.

5.3 Next period $t+1$

The last subsection details the bunching equilibrium in a given period t. In particular, some households with higher income are able to bunch because the noisy housing signal limits the officer's ability to screen them out. After t, the officer continues to monitor households.[38] Thus her mapping between housing and true type can improve. It is straightforward to see that, in period $t+1$, if the housing signal $h_{i,t+1} = \theta_i + \epsilon_{i,t+1}$ becomes more precise, $f(h_{i,t+1} = \theta_i | \theta_i, I_{i,t})$, the probability that the housing signal correctly maps to true income, in Equation 5 increases and raise the posterior belief, thus the housing signal would play a larger role in the decision of the officer.

A more precise housing signal $h_{i,t+1}$ lowers the housing condition threshold that the officer would considers h^*_{t+1}, thus lowers the value for the marginal household: $\theta_{h^*_{t+1}} < \theta_{h^*_t}$. As a result, the fraction of bunching households across the sample will reduce over time.

Proposition 2. *As the signal precision increases over time, the fraction of bunching households ω_t decreases in t.*

The proof for Proposition 2 is detailed Appendix E.

A smaller fraction of bunching household means a smaller bunching mass B and a larger truthful mass C in Figure 9 as time goes by. This also means the term B in Equation 7 (intergrated over mass B in Figure 9) becomes smaller, making the officer more likely to correctly identify and accept households with true income below the cutoff $\bar{\theta}$. Intuitively, the more precise signal helps the officer better distinguish truly poor househods (mass A) from bunchers (mass B).

Notice that, among the households the officer considers (masses A and B), the average reported income slightly lowers from t to $t+1$, because there are fewer bunchers. Meanwhile, the official cutoff $\bar{\theta}$ on the right hand side of Equation 7 remains unchanged over time, since the government maintains the same cutoff for a five-year phase. This implies that the marginal message $\tilde{\theta}^*_{t+1}$, the highest reported income that the officer accepts, is likely to be larger than the marginal message of the previous period, $\tilde{\theta}^*_t$. In other words, the officer is likely to relax the reported income criteria in her decision over time. In the following subsection 5.4, I formalize these predictions on the officer's learning process over time in Proposition 3.

[38] It is likely that the officer will focus her monitoring efforts on households who previously appeared eligible. For example, their reported income and housing conditions are near their respective thresholds. These are the households she could have mistakenly included (if their true income is above $\bar{\theta}$) or exclude (if their true income is below $\bar{\theta}$) in t. Therefore, over time, she may learn more about these households than about better-off households. subsubsection 6.2.3 investigates this potential heterogeneity in learning effects.

5.4 Normality Assumptions

I now incorporate the distributional assumptions of the stochastic components of the model in order to derive an explicit equation for the officer's decision. This equation will inform my empirical strategy in subsection 6.2. In essence, I assume that all (primitive) random variables are normally distributed. This allows me to express the expectation of the households' true income at time t in Equation 6 – a key object that drives the officer's decision – as a linear function of all information sources up to period t.

True (log) income is identical and independently distributed with a normal distribution: $\theta_i \sim$ i.i.d. $\mathcal{N}(\mu_\theta, \sigma_\theta^2)$. The housing signal is centered around the true (log) income with some bounded noise: $h_{it} = \theta_i + \epsilon_{it}$, $\epsilon_{it} \sim$ i.i.d. $\mathcal{N}^*(0, \sigma_{\epsilon_t}^2)$ and serially independent, where $\mathcal{N}^*$ is the truncated normal distribution.[39] The bounds in this signal allow the officer to form the housing threshold h_t^*, below which the officer believes the household may be poor. The (log) income reported to the officer $\tilde{\theta}_t$ is noisily distributed around the message $\hat{\theta}_t$ *intended* by the household, so: $\tilde{\theta}_{it} = \hat{\theta}_{it} + \eta_i$, $\eta_i \sim$ i.i.d. $\mathcal{N}(0, \sigma_\eta^2)$, also serially independent. Furthermore, both error processes in these informational variables, ϵ_{it} and η_i, are independent from everything else.

While the housing signal cannot be manipulated by household, the message could be. In particular, $\hat{\theta}_{it}$ is endogenous, as it could be either a truthful report of true income $\hat{\theta}_{it} = \theta_i$ or it could be a bunching message $\hat{\theta}_{it} = \bar{\theta}$. Knowing this, the officer needs to rely on housing conditions to proceed with Bayesian updating.

When the officer observes that the housing conditions are above the critical housing threshold h_t^*, she can infer the following:

$$\text{If } h_{it} > h_t^*, \;\; \theta_i > \bar{\theta} \text{ and } \tilde{\theta}_{it} = \theta_i + \eta_i.$$

This statement says that when seeing the property in good conditions (above h_t^*), the officer can infer that the true income of such households must be greater than $\bar{\theta}$. This is because all households with true income below $\bar{\theta}$ will surely get draws of housing conditions below this level.[40] At the same, she is confident that such households will not find it profitable to bunch, thus they truthfully report their income.

On the other hand, when the officer sees that physical housing conditions falls below h_t^*, she

[39] The truncation bounds could be defined as the 1st and 99th percentile of the unbounded normal.

[40] Some households with true income in bunching range $[\bar{\theta}, \theta_{h_t^*}]$ may get a realization of housing conditions above h_t^*. All households with true income above the marginal type $\theta_{h_t^*}$ surely get a draw of housing conditions above h_t^*.

can infer that the true income of the household in question must be lower than $\theta_{h_t^*}$. Recall that this is the highest income that could be associated with h_t^*. Moreover, this range of income further splits into two types: the bunching and honest households. Specifically:

$$\text{If } h_{it} \leq h_t^*, \quad \begin{cases} \theta_i \in [\bar{\theta}, \theta_{h_t^*}] & \text{and } \tilde{\theta}_{it} = \bar{\theta} + \eta_i \\ \theta_i < \bar{\theta} & \text{and } \tilde{\theta}_{it} = \theta_i + \eta_i \end{cases}$$

The bunchers are those with true income in the range $[\bar{\theta}, \theta_{h_t^*}]$: they are above the official cutoff but can bunch, as their low housing conditions allow them to convincingly pass off as poor. Other households with housing conditions below h_t^* will actually have very low income (below the official cutoff $\bar{\theta}$), so they do not need to bunch.

Expecting such income-reporting behaviors from households, the officer's Bayesian updating procedure will differ depending on whether the realization of the housing conditions falls below or above h_t^*. This process is detailed in subsection E.2, first for the *high* housing segment ($h_{it} > h_t^*$), then for the *low* housing segment ($h_{it} \leq h_t^*$). Below I will describe the officer's estimation of the household's true income when pooling across the two housing segments. Note that, to keep the derivation simple, I use the true distribution of types $f(\theta_i)$ as the prior in these steps, but after arriving at the results, I replace it with the prior *up to the most recent period* $f(\theta_i | I_{i,t-1})$.

If the officer observes $h_{it} > h_t^*$, her expectation of the household's true income given the observed housing conditions and the reported income is given by:

$$E(\theta_i | h_{it} > h_t^*, \tilde{\theta}_{it}) = \Big[(1 - b_t)(1 - a_t)\mu_\theta + (1 - b_t)a_t h_{it} + b_t \tilde{\theta}_{it}\Big] (1 - \omega_t) \tag{8}$$

If the officer observes $h_{it} \leq h_t^*$, she obtains a similar, yet different, estimation:

$$E(\theta_i | h_{it} \leq h_t^*, \tilde{\theta}_{it}) = \Big[(2 - b_t)(1 - a_t)\mu_\theta + (2 - b_t)a_t h_{it} + b_t \tilde{\theta}_{it}\Big] \omega_t \tag{9}$$

where $a_t = \frac{\rho_{\epsilon_t}}{\rho_\theta + \rho_{\epsilon_t}}$, $b_t = \frac{\rho_\eta}{\rho_\theta + \rho_{\epsilon_t} + \rho_\eta}$ are parameters resulting from the Bayesian updating process, μ_θ is the mean of the prior belief, and $\omega_t = F_{h_i}(h_t^*)$ is the share of household with low housing conditions.

Here in Equation 9, the coefficients on housing conditions h_{it} and the prior mean μ_θ in the brackets are large than their counterparts in Equation 8. This is because some households with $h_{it} \leq h_t^*$ can manipulate their income. Therefore, the officer needs to rely more on information from the housing conditions (and her prior) in her estimation, since housing conditions are harder to manipulate.

Pooling cross the two housing segments, we have:

$$\begin{aligned} E(\theta_i|h_{it},\tilde{\theta}_{it}) &= E(\theta_i|h_{it} \leq h_t^*,\tilde{\theta}_{it})\omega_t + E(\theta_i|h_{it} > h_t^*,\tilde{\theta}_{it})(1-\omega_t) \\ &= \underbrace{\left[(1-b_t)(1-a_t)(1-2\omega_t+2\omega_t^2)+(1-a_t)\omega_t^2\right]}_{A_t}\mu_\theta \\ &+ \underbrace{\left[(1-b_t)a_t(1-2\omega_t+2\omega_t^2)+a_t\omega_t^2\right]}_{B_t}h_{it} \\ &+ \underbrace{\left[b_t(1-2\omega_t+2\omega_t^2)\right]}_{C_t}\tilde{\theta}_{it} \end{aligned} \tag{10}$$

It is important to note that if there was no bunching, then officer would not need to split up her Bayesian updating process by the housing segments to account for bunching. In such a case, $\omega_t = 0$ and we would get the result of standard Bayesian inference with normal distribution.

Recall that the prior at the beginning of period t is $f(\theta_i|I_{i,t-1})$, the officer's estimate of the household's true type, given all the information she has received up to period t is actually given by:

$$E(\theta_i|I_{i,t-1},h_{it},\tilde{\theta}_{it}) = A_t\mu_{\theta_{i,t-1}} + B_t h_{it} + C_t\tilde{\theta}_{it}$$

where $\mu_{\theta_{i,t-1}} = E(\theta_i|I_{i,t-1})$. This expectation determines officer's *Rejection* decision in equilibirum in each period t:

$$Reject_{it} = \mathbb{I}\{E_t[\theta_i|I_{i,t-1},h_{it},\tilde{\theta}_{it}] \geq \bar{\theta}\} \tag{11}$$

In essence, Equation 11 says that the expected type given the information up to date is a weighted average between all information sources in the current period and the prior mean in the last period. This is a standard result of Bayesian updating with Gaussian information (see Chamley, 2003), because, conditioning on the true income θ_i (and consequently the equilibrium action chosen by that type), both the housing signal and the reported message are essentially independent sources of information.

Tracing out the effects of learning over time I show in Appendix E that as the precision of the housing signal ρ_{ϵ_t} grows over time, there are two opposing effects on the coefficient B_t on housing conditions over time. The first effect raises B_t over time, because the growing precision of the housing signal directly makes the officer more reliant on this criteria to screen households. However, the second effect lowers B_t over time, because the term that is present due to bunching,

$a_t\omega_t^2$, reduces when the housing signal gets more precise.[41] Therefore, if the positive effect of more precise housing signal is sufficiently larger than the negative effect from bunching reduction, then B_t may be larger than B_{t-1}. Which effect dominates is an empirical question, which will be verified in subsection 6.2.

An important note is that, in this framework, if ρ_{ϵ_t} remains unchanged over time, then $B_t < B_{t-1}$ for sure. In other words, a more precise signal over time is the only way that the *spot* coefficient on housing condition may grow between $t-1$ and t, *conditioning* on the stock of information up to t.

Similarly, these two forces also affect the coefficient C_t on reporting income over time, but in the opposite direction as compare to how they affect B_t. This is because a more precise housing signal allows the officers to rely less on reported income, but less bunching also makes reported income slightly more informative. It follows that:

Proposition 3. *If the precision the housing signal sufficiently grows over time (ρ_{ϵ_t} is sufficiently larger than $\rho_{\epsilon_{t-1}}$) and the share of households with previously low housing conditions (ω_{t-1}) is sufficiently small, then $B_t > B_{t-1}$ and $C_t < C_{t-1}$.*

The proof for Proposition 3 is available in Appendix E.

Note that I develop the model here assuming true income to be constant over time, so that the learning effects can be highlighted. This is unlikely to be true, as there is substantial real income growth over this period. I extend the model in Appendix F to allow for economic growth with a simple upward shift in true income over time. In doing so, I also cancel out the learning channel, by fixing ρ_ϵ constant across periods. In such a case, we still get similar predictions as the main model here, however the coefficient B_t on housing conditions is much less likely to to grow over time, compared to the case with learning effect. This contrast highlights that an increase in the precision of housing signal over time is indicative of the presence and the importance of the learning channel over time.

6 Testing the model

The theoretical model in the previous section yields three predictions, of which, Proposition 2 is consistent with the bunching pattern documented in Section 4. In this section, I conduct empirical

[41] This term is dependent on ω_t^2— the share of housing with low enough housing condition and therefore can bunch

tests for two remaining predictions: Proposition 1 and Proposition 3.

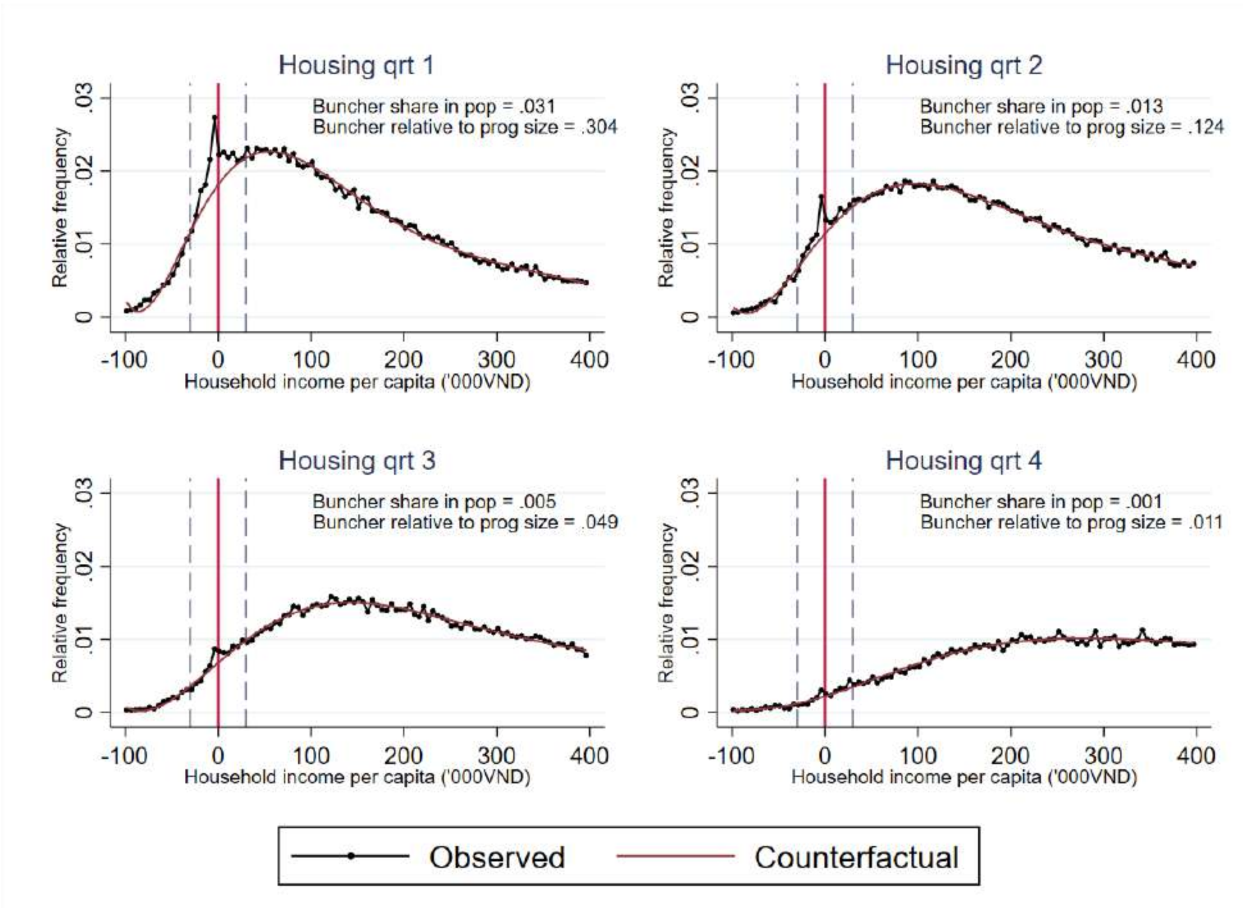
6.1 Prediction on bunching in a given period t (Proposition 1)

Proposition 1 suggests that, in any given period, households are more likely to bunch when they appear poorer in terms of housing conditions. This results from the fact that only households with realization of housing conditions below a certain threshold can convincingly pass off as a low-income household. I test this prediction by inspecting the extent of excess bunching by the housing index.

Figure 10 groups households in each survey round into four quartiles of housing index and plots the distribution of reported income with observations across all rounds. The first quartile represents the households with the poorest housing conditions. The black connected line is the observed distribution, while the red curve is its counterfactual distribution if there was no bunching. As in Section 4, the share of excess bunchers is defined as the gap between the observed and counterfactual distributions around the official income cutoff (solid vertical line). Consistent with Proposition 1, we see a mass of reported incomes around the cutoff, but this mass gets smaller when looking at a higher quartile of housing conditions. For each quartile, I also report the number of excess bunchers relative to the population, as well as relative to the program size. The share of excess bunchers is the highest among those observably most destitute, about 3 percent relative to the entire population. This fraction decreases monotonically as we move to higher quartiles of the housing index; in the top housing quartile, this fraction drops to almost zero.

A similar pattern also appears if I split the sample by another proxy for poor housing conditions, such as mountainous areas where living situation tend to be lacking. Figure B4 shows that, as a fraction of the subsample, the bunching fraction is greater in mountainous areas than in the plains.

Figure 10: Bunching by housing index quartiles



Notes: On horizontal axis is reported per capita income in 2002 terms; the unit is thousand VND. Income is deflated to 2002 terms and also re-centered around the applicable cutoff. Each graph presents the empirical and counterfactual distributions of the resulting adjusted income. The income cutoff (zero) is marked with the vertical solid line. The vertical dash lines indicates the boundaries of the bunching region, about VND 30,000. The counterfactual density distribution is estimated with Equation 1. All graphs pool observations from eight cross-sections: 2002, 2004, 2006, 2008, 2010, 2012, 2014. The graphs are plotted separately for each quartile of housing index; the quartiles are defined separately for each survey round. The texts in the graphs provide estimates for the number of bunchers relative to (i) the population, (ii) the bunching region within VND 30,000 around the income cutoff, and (iii) the number of program participants in the subsequent year (at $t + 1$).

6.2 Prediction on Learning over time (Proposition 3)

I exploit the rotating panel feature of the VHLSS to test the learning effect summarized in Proposition 3. I assemble four panels of households over the following sets of two consecutive waves: 2002-2004, 2004-2006, 2010-2012, and 2012-2014 panels.[42] Of these four panels, the 2002-2004 panel covered some years during Phase 1 of the program, while the 2012-2014 panel spanned over

[42]Household panel identifiers are provided by McCaig and Pavcnik (2015) and GSO.

some years during Phase 3. The remaining panels tracked households when the program transitioned between phases. In particular, the 2004-2006 panel applies to the transition from Phase 1 to Phase 2, and the 2010-2012 panel covers the switch between Phase 2 and Phase 3.

In subsubsection 6.2.2 and subsubsection 6.2.3 below, I only use the *within-phase* panels, 2002-2004 and 2012-2014, to trace out the learning effects predicted by my model. Within a given phase, all program rules, such as the official income cutoffs, remain unchanged, thus any changes in the selection process that the empirical tests pick up are likely to reflect the officer's learning over time. Of course, there could be several confounding factors, as discussed in more details in the empirical strategy below.

6.2.1 Empirical Strategy

Recall the empirical version of the Rejection decision summarized by Equation 11 from Section 5:

$$Reject_{it} = \mathbb{I}\{K_t + A_t\mu_{\theta_{i,t-1}} + B_t h_{it} + C_t\tilde{\theta}_{it} + \nu_{it} \geq \bar{\theta}\}$$

where i indexes housholds and t refers to calendar year. This equation says that the officer rejects a household if her guess of its true income exceeds the official cutoff $\bar{\theta}$. Her decision is given by the indicator variable $Reject_{it}$, which equals 1 if she rejects the household and 0 otherwise. Her estimate of the household's true income depends on her past belief about the household $\mu_{\theta_{i,t-1}}$, as well as her current observation of housing conditions h_{it} and reported income $\tilde{\theta}_{it}$. In addition to these variables, the officer is likely to consider other household characteristics, on which I lack data. These unobservables (to the econometrican) are captured by ν_{it}. Since the VHLSS is collected every two years, the empirical analysis hereafter will refer to t as a *survey round*.

$\mu_{\theta_{i,t-1}}$ represents the officer's belief about household i in the previous period and therefore is unobservable in nature. I proxy her past belief with her rejection decision in the base line round, $Reject_{i,t-1}$, which equals 1 had the officer believed that the household had high income. On one hand, this may not be a perfect proxy, because $\mu_{\theta_{i,t-1}}$ is continuous while $Reject_{i,t-1}$ is binary. On the other, the past program status may do a decent job at capturing the information that the officer observes up to $t-1$. In fact, the program instructs the officers to check past participant status in order to simplify the recertification process. This program feature also ensures that, if a different officer carries out the recertification task in the next period, the decision of the previous officer provides her some refined prior information about the household. To further support the use of $Reject_{i,t-1}$ to proxy for past beliefs, I conduct robustness checks in Table C2 (columns (5) and (6)) with other continuous proxies that are presumably commensurate with the officer's belief about the household.[43] I measure the housing variable h_{it} with a housing index ranging from 0 to 100, with higher value indicating better housing conditions. The construction of this variable is explained in subsection 3.3. As for $\tilde{\theta}_{it}$, the reported income, I measure it with the natural log of inflation-adjusted reported household income per capita.[44]

h_{it} and $\tilde{\theta}_{it}$ are likely to be correlated with other variables that the officer is aware of. To the extent that the variables are observable in the data, I add them as controls, denoted as X_{ipt}. In particular, I control for an asset index generated by a Principal Component Analysis; this index encompasses the values of a large set of durables that the officer may consider per the program guidelines. Some of these correlated variables may be unobservable and end up in the error term, ν_{it}, in the equation above. Their presence will confound the coefficient B_t (and also C_t) if they do not represent the officer's learning process via the housing variable as described by the theoretical model. The unobservables may be fixed at the household level, for example, the program may prioritize households in remote locations or recipients who have severe disabilities. With panel data, I can control for such household time-invariant factors with household fixed effects, ψ_i. In addition, some other unobservables may be time-variant. For example, there could be some top-down changes in the local targeting criteria that may affect the officer's decision. For example, some wealthier provinces can raise their own income cutoff above the national level if they have enough funding. I add province-by-survey-round fixed effects, κ_{pt} (where p denotes province), to absorb such conflating policy change at the province level. In addition, the province-by-survey-round fixed effects also control for economic growth that could potentially confound the learning effect, as explained in the model extension in Appendix F.[45] To put it another way, I replace ν_{it} above with $\psi_i + \kappa_{pt} + u_{ipt}$.

However, adding a large number of fixed effects while having a small number of periods can cause convergence issues in a threshold crossing model such as Probit or Logit regression. There-

[43] These measures of transfer size and the number of benefits received are piecemeal in VHLSS and are not consistently measured across all survey waves. To the best of my ability, I select the variables that are consistently measured within a panel, and only use them to check the robustness of $Reject_{i,t-1}$. Their availability is not sufficient to study a model where the officer's decision is to choose a transfer amount.

[44] Reported income is adjusted with temporal and regional CPI deflators for all goods. GSO prepared these deflators and included them in VHLSS datasets.

[45] I provide robustness checks controlling for time trends at a finer locality definition, i.e. at the district level. Using time trends at an even lower level (commune) can remove too much variation in the data, because the average sample size in each commune is 14 households for the 2002-2004 panel and 3 households for the 2012-2014 panel.

fore, I consider the following linear probability model:

$$Reject_{ipt} = \kappa_0 + A_t Reject_{i,p,t-1} + B_t h_{ipt} + C_t \tilde{\theta}_{ipt} + \omega_t X_{ipt} + \psi_i + \kappa_{pt} + u_{ipt}$$

The coefficients B_t and C_t can be consistently estimated if u_{ipt} is mean independent from h_{ipt}, $\tilde{\theta}_{ipt}$, conditioning on past program status, household fixed effects and province-specific time trends.

Most importantly, the theory explains the observed bunching pattern with learning over *time* via the housing conditions. To identify this learning effect over *time*, I take the difference between the two rounds of the panel as followed:

$$\begin{aligned} Reject_{ipt} = \kappa_0 + (\underbrace{\beta_0}_{B_t} h_{ipt} + \underbrace{\gamma_0}_{C_t} \tilde{\theta}_{ipt}) + (\underbrace{\beta_1}_{B_t - B_{t-1}} h_{ipt} \times \tau_t + \underbrace{\gamma_1}_{C_t - C_{t-1}} \tilde{\theta}_{ipt} \times \tau_t) \\ + \alpha Reject_{i,p,t-1} + \omega X_{ipt} + \psi_i + \phi_{pt} + u_{ipt} \end{aligned} \tag{12}$$

where τ_t denote the indicator for the follow-up round, and κ_{pt} absorbs the stand-alone follow-up round dummy τ_t . The coefficients of interest are β_1 and γ_1, which trace out how the impacts of housing condition and reported income change over the two waves of the panel. For example, for the panel 2002-2004:

$$\beta_1 = B_{2004} - B_{2002}$$
$$\gamma_1 = C_{2004} - C_{2002}$$

β_1 captures the learning effect over time via the h_{it} variable, because B_t is increasing in the precision of the housing signal, ρ_{ϵ_t}, which presumably increases over time. In other words, if $\rho_{\epsilon_{2004}}$ is sufficiently larger than $\rho_{\epsilon_{2002}}$, then :

$$B_{2004} > B_{2002} \text{ and } C_{2004} \leq C_{2002}$$
$$\iff \beta_1 > 0 \text{ and } \gamma_1 \leq 0$$

Lastly, the standard errors for this regression in Equation 12 are clustered at the primary sampling unit, i.e. the enumeration area, as suggested by one approach in Cameron and Miller (2015).[46]

Table 6: Impact of Selection Criteria on Program Status over Time, within Phase 1 and Phase 3

	(1) Reject	(2) Accept
Housing Index X Follow-up	0.00286***	-0.00286***
	(0.000896)	(0.000896)
Housing Index	0.0000567	-0.0000567
	(0.000990)	(0.000990)
Ln reported income X Follow-up	-0.00782	0.00782
	(0.00639)	(0.00639)
Ln reported income	0.0262***	-0.0262***
	(0.00605)	(0.00605)
Asset index X Follow-up	-0.000571	0.000571
	(0.000819)	(0.000819)
Asset index	0.00198**	-0.00198**
	(0.000934)	(0.000934)
Number of observations	24548	24548
Mean outcome	0.899	0.101
Household FEs	✓	✓
Province-by-Round FEs	✓	✓
Cluster	Enum. Area	Enum. Area
Subsample	All	All

Notes: All columns pool together observations from two panels: 2002-2004 (during Phase 1), and 2012-2014 (during Phase 3). Column (1) implements the full specification in Equation 12 by regressing the indicator for not participating in the program in the following year (*Reject* at $t+1$) on housing index, reported income, and asset index, together with the interaction terms of these variables with the follow-up round dummy, while controlling for past program status at $t-1$, household fixed effects and province-by-round fixed effects. Column (2) changes the dependent variable to *Accept* at $t+1$, to help interpret the estimated effects with regards to the acceptance rate. All regressions are estimated with OLS. Standard errors in parentheses are clustered at the enumeration area level.

6.2.2 Main results

In Table 6, I report the results from implementing the empirical strategy developed from the model (Equation 12) to test for learning effects. In particular, I regress the indicator for not participating in the program in the following year (*Reject* at $t+1$) on housing index, reported income, and asset index, together with the interaction terms of these variables with the follow-up round dummy. In doing so, I control for past program status at $t-1$, household fixed effects and province-by-round fixed effects.

[46]There are 785 and 705 enumeration areas in 2002-2004 and 2012-2014 panels, respectively. The enumeration area in VHLSS is effectively a commune.

Column (1) reports the results of the regression in Equation 12 estimating the learning effects through the housing signal. I pool together data from households on panels 2002-2004 and 2012-2014. It is useful to benchmark the effect of housing and log reported income on program status at the baseline. Although both housing conditions and reported income are positively correlated with being rejected from the program in this round, the impact of housing conditions is small compared to that of reported income. The point estimates in the baseline round suggests that an increase of half a standard deviation (0.5 SD) in the housing index is associated with an increase of 0.5% in the probability of being disqualified from the program, while an increase of 0.5 SD in log reported income increases this probability by 10%. The weak impact of housing conditions on the officer's decision here is likely to reflect the low precision of the housing signal at the beginning, making this criteria less important relative to report income.

However, over time, housing conditions becomes much more important in the targeting process, as the coefficient on the interaction between housing conditions and the follow-up round dummy (β_1) is large, positive and statistically significant. To put the interpretation of this effect in perspective, I run the same specification in column (2) as in column (1), but replace the outcome variable with an indicator for being accepted to the program. Doing so simply switches the sign of the coefficient, but allows us to interpret the learning effect with respect to the probability of acceptance. Over the course of two calendar years, a 0.5 SD increase in the housing index is associated with a 25.11% reduction in the chance of being accepted to the program.[47] On the contrary, reported income is an important determinant of eligibility at the baseline, yet its impact over time (γ_1) is slightly negative but indistinguishable from zero. This suggests that officers indeed learn more about the household via its housing conditions and put more emphasis on this variable over time as entailed by the model.

In Table 6, I also control for assets index, as the officer is likely to observe them. This criteria also contributes to the household's program status, reflecting the role of assets in the targeting process. The baseline-round estimate implies that an increase of 0.5 SD in the asset index is associate with an increase of 2.11% in the acceptance probability. To comprehensively control for the role of asset holdings on eligibility over time, I also interact asset index with the follow-round indicator. This may capture changes in the reporting of assets on the households' end, should they learn about the importance on some particular items as in Camacho and Conover (2011). Note that such an alternative channel could also explain why households stop bunching: because over time they may learn that assets matter more and switch to underreporting assets instead.

[47] Given a 0.5 SD increase in housing index equals to an increase of 17.74 percentage point in this variable, β_1 is 0.00286, and the baseline acceptance probability is 0.101, this effect calculated by: $(0.5 \times 17.74 \times 0.00286/0.101) \times 100 = 25.11\%$

To the extent that this behavior is concentrated among the same households whose information contribute to the officer's learning process, it could conflate the officer's learning effect identified by the housing variable. I separately account for any possible change in reported assets by allowing it to flexibly change with time. However, the role of assets seems quite stable over time in this pooled regression.

Taken together, the results in Table 6 indicate that while all targeting criteria shown here are important in the certification process, their roles over time are nuanced. Consistent with Proposition 3, the only selection criterion that seems to gain in importance over time is housing conditions. On the other hand, reported income (and asset holdings) do not gain any additional predictive power over time. Towards the end of the five-year phase, housing conditions is arguably the most important factor (by the impact associated with a 0.5 SD increase) among the three criteria in determining eligibility.

Robustness Checks I check the robustness of these results with various specifications in Table C2. I start with checking the precision of the main results in Table 6 at different levels of clustering. The regressions in Table 6 are clustered at the primary sampling unit, i.e. the enumeration area. While the consensus seems to prefer lower level of clustering (thus larger number of clusters) (Bertrand et al., 2004; Cameron and Miller, 2011), there could be reasons to cluster at a broader level, such as the province level if errors in the targeting process are correlated within the province. In columns (1) and (2) of Table C2, I raise the cluster definition to the district and province level, respectively. Doing so raises the standard errors slightly, however the estimate of the learning effects over time loaded on the housing variables is still statistically significant.

Next I check whether the main results are robust to alternate control of location-specific time trends. My inclusion of province-by-survey-round fixed effects is meant to control for the changes in the province-specific income cutoffs within a phase. However, the targeting procedure may still evolve differently across lower administrative level, given that implementation of fiscal budget in Vietnam has been substantially devolved to the district level (World Bank, 2015). However, it is important to note another role of the location-specific time trends. It effectively controls for macro economic changes that could offer the alternative explanation to the "on-then-off" bunching pattern. As explained in Appendix F, economic growth may have improved the living standards for so many that only few households are left susceptible to bunching. To sufficiently control for trends in the local economic conditions, the definition of the economy needs to be large enough. For example, consider commune-specific time trends, if daily commute across communes for work is common enough, then restricting time trends to vary within the commune may not sufficiently capture such spillovers across communes. According to statistics from VARHS 2008-2012, another small-scale household survey in rural Vietnam, 35-44% of wage workers work outside their home commune but 27-29% still work within the same province. Column (3) of Table C2 controls for trends at the district level, which is below the province level, but still large enough to define the local economy. In this specification, the learning effect reflected through the coefficient on housing over time is slightly smaller, but remains significant.

The analysis so far pools together two panels that are ten years apart from one another. In Table C2, I repeat the analysis for each panel separately to check whether learning effects exist in both panels, which represent different phases of the program. Most of the pooled effect is driven by the 2002-2004 panel (column (4)). The coefficient on housing over time is smaller for the 2012-2014 panel (column (5)), about half as much as in the earlier panel, but it is imprecisely estimated. One reason for this weak effect for the 2012-2014 panel is the small sample size. Another could be the reliance on housing to target households may not be as critical in this period (Phase 3) as in the earlier period (Phase 1). Compared to Phase 1, Phase 3 of the program spells out a very detailed location-specific formula to incorporate a large number of assets and durables. It is possible that such refined criteria relieves the need to increasingly rely on housing characteristics to select households. Interestingly, the coefficient on reported income interacted with later round fixed effect is rather strong in the 2012-2014 period. It roughly cancels out the baseline effect, indicating reported income practically has no impact on the household's program status in the follow-up round.

Columns (6) and (7) of Table C2 add additional information that the officer may observe and utilize in the selection process. Both columns controls for time-varying household characteristics, including urban dummy, minority dummy, education and age of household's head, female head dummy, share of dependents (any household members who are not working), and household size. It is possible that the officers observe positive change in income-generating activities, particularly among those with poor housing conditions, then we may conflate the impact of housing conditions with such changes on employment status. Column (6) add the industry codes of head and spouse interacted with the follow-up round indicator, respectively. Along the same lines, perhaps the officers take into account contemporary idiosyncratic shocks, which may spuriously correlate with housing conditions. Column (7) controls for an indicator for whether the household experienced negative shocks in the last twelve months interacted with the follow-up round dummy. This information on idiosyncratic shocks is only available for the 2002-2004 panel. In all these specifications, housing conditions matter more over time and the coefficients remain significant.

In Equation 12, the housing variable is time-varying. In the model, the coefficient β_1 captures the improvement in the officer's mapping of housing and true income, regardless of changes in the realization of housing over time. In other words, even if the physical dwelling of the household remains exactly the same over the course of the panel, the officer has learned to judge the same housing characteristics more accurately. Column (8) of Table C2 restricts the sample to households with the same *realization* of housing conditions over time (56% of the sample used for Table 6). The estimate of β_1 reduces slightly, by 0.1 percentage point, but it remains significant. Here I am focusing on households that are observably the same to the officers over time, yet their eligibility based on the same housing conditions have increased over time. This suggests that the officers have learned to better extract information from the housing signal, as suggested by the model.

Columns (9) and (10) in Table C2 address the potential concern about using $Reject_{i,t-1}$ to proxy for the officer's past belief about the households. VHLSS has some information about the benefits and transfers the households have received during the past year – such variables could arguably better proxy for the officer's past judgment of the household. Despite the sporadic availability of these variables in VHLSS, I am able to gather some consistently-measured metrics of benefit amounts for the panels used in the main analysis. For the 2002-2004 panel, I use the discount percentages on tuition and other education fees for children of poor households. For the 2012-2014 panel, I generate the count of benefits and the amount of electricity subsidy the household received. Columns (9) and (10) augment the main specification in columns (4) and (5) with these continuous measures of benefits, respectively for each panel. Clearly, adding these measures to $Reject_{i,t-1}$ does little to the learning effect loaded on housing conditions over time. The F-test for the joint significance of these variables also yields p-values of 17.9% for the 2002-2004 panel, indicating that they are not statistically significant once we have controlled for $Reject_{i,t-1}$. In other words, $Reject_{i,t-1}$ alone can sufficiently capture the officer's belief up to $t-1$. For the 2012-2014 panel, the F-test's p-values is 4.7%, suggesting better proxies of past belief may be warranted. Nevertheless, these alternate proxies of past belief do not change the point estimate of β_1 much for this panel.

All in all, the robustness checks here are all consistent with the model and indeed suggest that officers put more importance on housing conditions over time.

6.2.3 Heterogeneity in learning effect

The analysis so far has assumed the officer gains enhanced knowledge about households via their housing conditions for *all* households in the sample. There could be reasons to expect the same model may yield different estimates for different populations of households. For example, the selection officers may focus on updating their mapping between housing conditions and true income for low-income earners, rather than carry out this task for all households. This lower spectrum of income earners are closer to the official income cutoff, at the same time more likely to live in poorer housing conditions. In other words, they tend to have lower cost of misreporting and higher chance of passing off as eligible (even though their true income are above the cutoff). This implies that the officer can make both inclusion and exclusion errors among these households, while they would be less likely to make mistakes with high income earners. Therefore, it is reasonable that the officer would put more efforts in rescreening households reporting income in the bottom of the distribution. In the context of the theoretical framework, this could be viewed as the precision of the housing signal, ρ_{ϵ_t}, substantially growing over time for relatively poorer households, but not much for richer households.

This view is particularly useful to explain the lack of learning effects from housing conditions when the central government reformed the targeting program and raised the income cutoff. As the new phase commenced, the new official income cutoff would allow a number of higher income households to be considered for the first time. It is possible that the local officers had not gained any additional information about such households during the previous phase, because their reported income was outside the range to be rescreened.

Here, I probe the heterogeneity of learning within a given phase with the specification in Equation 13 and in subsubsection 6.2.4 I inspect the lack of learning effects when the program started targeting a new set of slightly better-off households.

$$\begin{aligned} Reject_{ipt} = \kappa_0 &+ (\beta_0 h_{ipt} + \gamma_0 \tilde{\theta}_{ipt}) + (\beta_1 h_{ipt} + \gamma_1 \tilde{\theta}_{ipt}) \times Poorer_{ipt} \\ &+ (\beta_2 h_{ipt} \times \tau_t + \gamma_2 \tilde{\theta}_{ipt} \times \tau_t) + (\beta_3 h_{ipt} \times \tau_t + \gamma_3 \tilde{\theta}_{ipt} \times \tau_t) \times Poorer_{ipt} \\ &+ \alpha_0 Poorer_{ipt} + \alpha_1 Reject_{i,p,t-1} + \psi_i + \kappa_{pt} + u_{ipt} \end{aligned} \tag{13}$$

Equation 13 fully interacts the officer's selection criteria with $Poorer_{ip,t-1}$, an indicator for being observably poorer in the initial survey round thus more likely to be rescanned. One candidate definition for $Poorer_{ip,t-1}$ is a dummy for the bottom tercile of reported income .[48] Others definition

[48] The official income cutoff lies within this bottom tercile.

of $Poorer_{ip,t-1}$ could be dummies for rural and mountainous areas, where the pool of potential qualifiers could be large. The coefficient of interest is β_3, which tells us how the learning effect via the housing signal differs between these low income earners compared to those in top two terciles.

Table 7: Heterogeneous Impact of Selection Criteria on Program Status over Time, within Phase 1 and Phase 3

	Reject		
	Poorer definition is:		
	(1)	(2)	(3)
	Bottom income tercile	Rural	Mountains
Housing X Follow-up X Poorer	0.00660***	0.00322*	0.00622**
	(0.00252)	(0.00175)	(0.00279)
Housing Index X Follow-up	0.00134*	0.000674	0.000748
	(0.000734)	(0.00125)	(0.000848)
Ln reported Income X Follow-up X Poorer	-0.0247	-0.0140	0.0159
	(0.0231)	(0.0123)	(0.0171)
Ln reported income X Follow-up	0.00384	0.00139	-0.00924
	(0.00518)	(0.00994)	(0.00771)
Asset index X Follow-up X Poorer	-0.000505	0.00115	-0.00286
	(0.00309)	(0.00144)	(0.00347)
Asset index X Follow-up	-0.000217	-0.00106	0.00220*
	(0.000719)	(0.000911)	(0.00123)
Number of observations	24548	24548	20560
Mean outcome	0.899	0.899	0.901
Household FEs	✓	✓	✓
Province-by-Round FEs	✓	✓	✓
Cluster	Enum. Area	Enum. Area	Enum. Area

Notes: All columns pool together observations from two panels: 2002-2004 (during Phase 1), and 2012-2014 (during Phase 3). All columns implement the three-way interaction specification in Equation 13, with different definition of *Poorer*. In column (1), *Poorer* is the dummy for the bottom tercile of reported income. In column (2), it is the rural dummy. In column (3), it is the dummy for mountainous terrain. All regressions are estimated with OLS. Standard errors in parentheses are clustered at the enumeration area level. All regressions include the uninteracted terms for housing index, report income, and asset index, as well as past program status $Reject_{i,t-1}$, but the coefficients on these variables are omitted from this table for a cleaner presentation.

Table 7 reports the results for this exercise. Columns (1), (2), and (3) report the coefficients for the learning regression interacted with bottom income tercile dummy, rural dummy, and mountainous area dummy, respectively. Representing β_3 in Equation 13, the triple interaction on the first row of column (1) suggests that the effect of the housing signal over time is about six times larger for households in the bottom tercile of reported income in the previous period, compared to the same effect for those in top two terciles of the reported income distribution (capturing β_2). Similarly, the use of housing criteria is more dominant in rural area than in urban area, as it is also deemed more useful over time in mountainous regions than in the plains.

In Appendix D, I perform a similar analysis by running the specification in Equation 12 separately for subsamples defined by the same dummies above. Columns (1) and (2) in Table C3 contrasts the learning effect for households with in the bottom income tercile with the same effect for those above this bracket. Columns (3) and (4) do the same exercise, but for rural and urban areas, respectively. Columns (5) and (6) contrast the mountainous regions versus plains. Evidently, the housing signal has become a particularly important criteria to eliminate households who previously appeared poorer by these metrics and hence might have been more likely to bunch in the past. These results suggest that, in generally poorer subsamples, selection officers may particularly rely on housing conditions as an effective tool to improve the targeting process over time.

6.2.4 Can learning persist after program reforms?

The empirical exercises in subsubsection 6.2.2 and subsubsection 6.2.3 have utilized panel of households that were tracked during the same phase of the program. Within a given phase, the program rules remained unchanged, therefore I could possibly attribute the growing impact of housing conditions on the officer's decision to the learning process described in the theoretical model. However, when the program undergoes major reforms to enter a new phase, the knowledge accumulated over the preceding phase may no longer be applicable. As argued in previous subsubsection 6.2.3, while the local officers may want to rescreen households who previously report a message in the bunching region near the official cutoff level, they probably need not recheck households with high reported income. As the new phase commenced, the new official income cutoff would allow a number of higher income households to be considered for the first time. It is possible that the local officers had not gained any additional information about such households during the previous phase, because their reported income was outside the range to be rescreened.

Therefore, the program reforms present an unique opportunity to test the *obsoletion* of the previous learning effect through the housing signal. In particular, while the reforms generally change the income and asset criteria, they maintain the same housing criteria across the phases. This implies that although the official rules on housing conditions did not changed, the knowledge, which the officers had accumulated to better estimate the true type of households who reported income in the lower range, is no longer useful to determine the eligibility of higher-range income households.

In Table C5, I compare the learning effect within the same phase against the same effect between phases. For ease of comparision, I report in column (1) the same results of the within-phase learning regression as in column (3) in Table 6. This regression pools together two panels, each spreads over periods within the same phase of the program. In particular, the 2002-2004 panel tracks the same households over Phase 1, whereas the 2012-2014 panel does the same over Phase 3. In column (2), I report results of the same learning regression but over the periods that program reforms occurred. Specifically, column (2) pools together the 2004-2006 panel, which tracks households when Phase 2 superseded Phase 1, and the 2010-2012 panel, which follows households when Phase 3 replaced Phase 2. Note that only half of the households in the 2002-2004 panel reappears in the 2004-2006 panel, due to the rotating-panel structure of the survey. All regressions control for past program status $Reject_{i,t-1}$, household fixed effects, and province-by-round fixed effects. The standard errors are clustered at the enumeration area.

Within the same phase, the housing criteria had become increasingly important in the determination of eligibility for the program. Yet, when the program moved to a new phase, housing conditions had lost some predictive power of program status. For the 2010-2012 panel which spans over the transition between Phase 2 and Phase 3, the chance of rejection for a household with better housing by an additional index point even declines over time by 0.16 percentage point; however the estimate is imprecise. This result is consistent with the intuition that the previous stock of knowledge gained from the housing signal is no longer effective when the new phase started to target a new set of households.

7 Counterfactual analysis

In this section, I conduct a simple counterfactual analysis to compare the targeting performance of the *status quo* program design with an alternative targeting mechanism. In essence, I use the main results from the learning regression in subsubsection 6.2.2 to generate the *status quo* allocation of acceptance card (i.e. the officer's decision) which embeds learning over time. After that, I use the same regression results but mute the learning-over-time effect to predict a hypothetical allocation. Comparing each of these allocations to a classification of true poverty, I compute statistics, such as error rates, to evaluate how well the program targets poverty with and without learning.

There are, however, some challenges to this task. Ideally, I would like to generate the hypothetical allocation in binary values (*Reject* or *Accept*) with the learning regression estimates, then compare it with the actual allocation. However, because the learning regression utilizes a linear probability model, the predicted outcome is a continuous probability of being rejected, which could be out of bound. This makes the comparison between the actual and hypothetical outcomes impossible. To make this comparison easier, I construct two allocations, one allows for learning and the other mutes this effect, with the following steps.

First, I find s, the share of households accepted into the program in each province. Second, I use the learning regression result to predict the probability of being rejected and rank households within the province by this probability. Then, I designate the bottom $s\%$ households ranked by this probability as program participants, who would be accepted under the *status quo*. This constitutes the predicted allocation with learning, which turns out to map quite well to the actual allocation. To generate the hypothetical allocation without learning, I repeat the same steps, while setting the learning coefficient on the interaction between housing conditions and time trend to zero.

Next I establish the classification of true poverty by using consumption data available for a small random subsample of VHLSS. Following Basurto et al. (2020), I use food consumption purchased by the household to measure *true* neediness. Food consumption has a long history of being used as measurement of poverty (Deaton, 1997; Hoddinott, 1999). Like before, I rank households by food consumption and designate the bottom $s\%$ households according to this ranking as *truly* poor. In other words, these bottom $s\%$ households should be accepted to the program if targeting was perfect.

Comparing each allocation above to the classification of true poverty, I compute four statistics to gauge the targeting performance of the two mechanisms. I start by calculating the simple error rate, which is the sum of excluded poor households and included nonpoor households as a share of the total sample. It is also helpful to compute two statistics often evaluated for targeting programs: undercoverage rate and leakage rate. The former is the number of poor households who are rejected, relative to the total number of poor households. The latter is the number of nonpoor households that are accepted, relative to the total number of program beneficiaries. Finally, I compute another measure of targeting performance, known as "targeting differential". Formulated by Galasso and Ravallion (2005), this statistics is the difference between the proportion of poor and the proportion of nonpoor who are accepted to the program. The targeting differential varies between -100%, when only the nonpoor gets accepted, and 100%, when targeting is perfect targeting.

Note that this comparison is subject to some caveats. Notably, the sample size for this exercise is small.[49] The small sample size of the consumption module affects my choice of geographical unit to define true neediness. Because the certification is carried out by officers at the commune level, ideally I would prefer to use the commune-specific program participation rate to define the commune-specific poverty rate $s\%$. However, VHLSS only selects three households on average per commune for the consumption module, thus the poverty rate defined on such a small number of households is highly imprecise. Therefore, I must consider a larger geographical unit— the province —to have enough observations to define a location-specific threshold for true poverty.[50]

Table 8 reports these statistics in both years for the actual allocation, the predicted *status quo* allocation, and the hypothetical allocation assuming no learning. Panel A pools panels 2002-2004 and 2012-2014 together and utilizes the estimates from the pooled regression in Table 6 column (2) to generate the predicted allocations. Panel B focuses on only the earlier panel 2002-2004 and uses the regression estimated on this sample only (Table 6 column (3)). Panel C repeats the same but focusing on the later panel 2012-2014; the estimated coefficients come from Table 6 column (4).

To check whether my predicted *status quo* allocation reasonably emulates the actual allocation, I start Table 8 by reporting the statistics in round 1 of the panel for these two allocations in column (1) and column (2), respectively. The targeting statistics are similar across these two column, ensuring that my simulated *status quo* allocation using the learning regression result match the actual allocation quite well.

It is useful to compared these baseline statistics found here with similar studies in the literature. The simple error rate in the current context is around 13-15%, lower than what Basurto et al. (2020) find in Malawi (14-15% or 18-22% depending on the subsidy types) and Alatas et al. (2012) find in Indonesia (30-33% depending on the targeting methods). The undercoverage (also known as exclusion error) here states that 73% of the poor are excluded from the program. In this regard, the Vietnamese targeting program fares worse than the Indonesian program studied by Alatas et al. (2012) (52-54%) and the Cameroonian program studied by Stoeffler et al. (2016) (42-

[49] Although the rotating panels used for the learning regressions in subsection 6.2 have reasonable sample size (about 10,000 households for the 2002-2004 panel, and 1,800 households for the 2012-2014 panel), only a portion of them have consumption data. This results from the independence between the sampling for the rotating panel and the sampling for the expenditure module. Only about 4,000 households (40%) in the 2002-2004 panel have consumption data in 2002 and 2,000 of them (20%) have consumption data in 2004 (Table 8). The 2012-2014 panel originally has about 9,000 households, but only 1,800 of them have information on their participation in the National Anti-poverty Program, explaining the considerably smaller sample size of this sample in the learning analysis. The same 1,800 households in 2012-2014 also have consumption data with them, as noted in Table 8.

[50] At the province level, on average I observe 35-38 households (Table 8), thus true poverty defined at the province level is more accurate and reliable than at the commune level. However, one should keep in mind that the results here may not fully reflect the targeting performance at the commune level.

Table 8: Comparison between the *status quo* allocation with learning and the hypothetical allocating without learning

	Baseline Round		Follow-up Round				
	Actual	Predicted	Actual	Predicted with learning (status quo)	Predicted without learning (counterfactual)	Difference (3) - (5)	Difference (4) - (5)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Total error percentage	13.00%	13.17%	15.01%	14.80%	15.06%	-0.05 p.p.	-0.26 p.p.
Undercoverage rate	72.82%	73.79%	67.30%	66.35%	67.54%	-0.24 p.p.	-1.18 p.p.
Leakage rate	73.93%	74.86%	68.42%	67.51%	68.65%	-0.23 p.p.	-1.14 p.p.
Targeting differential	19.86%	18.80%	24.06%	25.13%	23.80%	0.27 p.p.	1.33 p.p.
N obs	5,937	5,937	3,884	3,884	3,884		
Avg. N obs have consumption data in a province	65	65	37	37	37	-	-
Avg. % obs have consumption data in a province	35.54%	35.54%	19.89%	19.89%	19.89%	-	-

Notes: The sample for this analysis is restricted to households in the 2002-2004 and 2012-2014 panels with consumption data. (1) I use per capita food consumption to measure true needs, then I define province-specific true poverty by: a) find s = the share of accepted households in each province, b) classify households in the bottom s% based on per capita food consumption as truly poor. (2) In a similar manner, I predict the selection decision following the status-quo mechanism. Using the regression results, I predict the probability of being accepted to the program, then suppose that the bottom s% of households in the province in terms of this predicted probability would be accepted to the program. (3) I construct the counterfactual selection decision as in (2) but assume there is no learning via housing conditions over time (coefficient on Housing X Follow-up = 0). Using the true poverty definition and the predicted selection rule under each mechanism, I calculate the following targeting performance metrics:
(i) Total error percentage = incorrectly excluded poors and incorrectly included nonpoors as a share of the sample.
(ii) Undercoverage rate = share of poor households incorrectly excluded.
(iii) Leakage rate = share of program participants incorrectly included as they are nonpoor.
(iv) Targeting differential = difference between proportion of poor and proportion of nonpoor who are accepted to the program; varies between -100% (only the nonpoor gets accepted) and 100% (perfect targeting).

57%). In terms of leakage rate (inclusion error), 73% of those accepted to the Vietnamese anti-poverty program are actually not poor. [51] This is again higher than Basurto et al. (2020)'s findings of 18-23% and Stoeffler et al. (2016)'s findings of 47-49%. My findings of a relatively lower simple error rate, but higher exclusion and leakage rate, results from the lower coverage of the Vietnamese program, compared to the programs studied by other papers. Evaluated over a national representative sample, the Vietnamese program covers 11-12% of its population, while the other contexts tend to be small-scaled projects piloted in highly poor areas and covering a minimum of 25% of their sample. Finally, the last statistic reported here, targeting differential, is 31%, suggesting that the Vietnamese program performs better than Stoeffler et al. (2016)'s Cameroo-

[51] The similar magnitude of undercoverage and leakage here results from the fact that I match the the poverty rate to the share of program participants s% in a given province. A different poverty rate definition, for example, the 25th percentile of food consumption, will result in some imbalance between these two rates.

nian context (15.7-24.2%) and Galasso and Ravallion (2005)'s Bangladeshi context (5-16.7%).[52] Overall, the targeting statistics here are not far off from those reported in previous studies.

The next three columns reports the same statistics in round 2 of the panel. Besides the actual allocation in column (3) and the predicted allocation under the *status quo* in column (4), this round additionally includes the hypothetical allocation in column (5). The difference between column (4) and column (5) is that the former imbues learning, while the latter posits the counterfactual scenario without learning. This is my preferred comparison, because the allocations in these two columns are generated in the same manner while they differs only by the presence of learning. Therefore, comparing them can tell us whether learning helps improve targeting performance; column (7) reports this difference. I also compare the actual allocation with the hypothetical no-learning allocation and report the difference in column (6).

Looking at the pooled results in column (7), canceling out the learning channel slightly increases the error rates, at the same time slightly reduces the targeting differential. Without learning over time, the leakage rate would go up by 1.14 percentage point if we consider the predicted *status quo* allocation. Similarly, the undercoverage rate would go up by 1.18 percentage point. The targeting differential also falls slightly, by 1.33 percentage point, if learning is muted, indicating that targeting is less progressive. Compared to the actual allocation, the hypothetical no-learning allocation also fares worse, however the magnitude of the differences in column (6) is smaller than in those in column (7).

The same results separately for each panel, 2002-2004 and 2012-2014, are provided in Table C4. The difference between the *status quo* and the counterfactual is more pronounced for the 2002-2004 panel. For instance, canceling learning over time would result an increase in the leakage rate by 1.66 percentage point, as well as an increase in undercoverage rate by 1.75 percentage point. Intriguingly for the 2012-2014 panel, there is no difference in the performance of the predicted *status quo* allocation and the hypotherical no-learning allocation. In fact these two allocations chooses the same households to be program participants. This reflects the weak role of learning in Phase 3 of the program, which spans over the 2012-2014 panel. Compared to Phase 1 (pertaining to 2002-2004 panel), the program also utilizes a much clearer and more detailed formula to incorporate a wide range of assets, besides the housing criteria and reported income. There-

[52] These statistics depends on the classification of true poverty used in each study. Although they all use per capita consumption (not necessarily food consumption) to measure true welfare, their threshold to classify poor households are differently define across these studies. Most similar to mine, Basurto et al. (2020)'s threshold is defined at the village level, with the poverty rate defined by the threshold matching the share of program participants. Alatas et al. (2012)'s threshold is also village-specific, but is cut off at PPP$2 per day. Stoeffler et al. (2016)'s threshold is defined as the village-specific 35th percentile. Galasso and Ravallion (2005)'s threshold is defined at the national level as the 25th percentile of per capita consumption for rural areas.

fore, it is unsurprising that the coefficient on the interaction between housing conditions and the time trend for the 2012-2014 panel (column (5) Table C2) is small and statistically insignificant. As a result, muting the learning-via-housing-conditions channel brings out little difference for this panel.

Results in Table 8 shows that learning over time can lead to a modest improvement in targeting performance, but for a large-scale anti-poverty program, such a small improvement can imply a substantial monetary value. In Table C6, I compute this value with reference to different program cost estimates for the 2002-2004 period, during which the learning effect over time is strongest in the data. I focus on the reduction in leakage rate (equivalent to an increase in "correct" coverage rate in this case) in 2004 thanks to the learning accumulated since 2002. Using a conservative estimate of monthly per capita benefit receipt of around VND 48,000 (50% of the monthly per capita benefits estimate in Table A2), the total cost of the National Anti-Poverty program is estimated to be 10,178 billion VND between 2002 and 2004. From the policy decree, funds allocated for two years of the program during Phase 1 is 9,032 billion VND. These cost estimates translate to a valuation of leakage in the ballpark of 32.3-36.4 million USD (PPP). This amount was appropriately directed the poor thanks to learning effects. To the extent that the central government may place greater welfare weight on reaching the poor than on excluding the non-poor, this could imply even larger welfare gains.

8 Conclusion

The interest to improve targeting performance of redistributional programs has recently gained traction in the literature. One problem that could leads to higher error rates of targeting is the incentive to manipulate metrics of eligibility. Although several studies have documented this behavior, few has studied how it changes over time and what mechanisms in place could explain its temporal patterns.

This paper documents evidence that Vietnamese households bunch at the income cutoff to appear eligible for the National Anti-poverty Program, however this behavior disperses over time. I propose an explanation that generates predictions that are consistent with this observed bunching pattern as well as other empirical findings in the data. I show theoretically and empirically that the implementation staff on the field could play a role in countering the households' incentives. Specifically, if local program officers could learn to rely more on other selection criterion that are more difficult to manipulate, households could be discouraged from continuing to under-report income. Indeed, I find that housing conditions, a selection criteria that is more difficult to tinker with, over time have become a more decisive factor in the certification process than reported income.

The learning effect established in this paper has helped improve targeting performance over time. In particular, had there been factors that hinder the learning process, the national targeting program in Vietnam would have misallocated about 1-2% of its budget (toward the end of a five-year phase), an equivalent of $ 32.3-36.4 million dollars (PPP) for such a large-scale redistribution program. This nuanced learning effect suggests that combining a wide variety of criteria, especially those that are harder to manipulate, could be important to uphold the targeting performance of long-running programs.

A More Institutional Details and Data Notes

Table A1: Provincial Poverty Lines, 2001-2016

		Poor Household	
Year	Province/City	Urban	Rural
1992-2003	HCMC	250	208
2004-2005	HCMC	330	330
2006-2008	HCMC	500	500
2009-2013	HCMC	1000	1000
2014-2015	HCMC	1330	1330
2016-2018	HCMC	1750	1750
2006-2008	Khanh Hoa	300	250
2009-2010	Khanh Hoa	500	430
2006-2008	Dong Nai	400	250
2009-2010	Dong Nai	650	450
2011-2015	Dong Nai	850	650
2015-2020	Dong Nai	1200	1000
2010-2011	Hai Phong	390	300
2018-2020	Hai Phong	1400	1100
2005-2008	Long An	250	200
2009-2010	Long An	540	400
1997-2000	Binh Duong	150	135
2001-2003	Binh Duong	180	150
2004-2005	Binh Duong	250	200
2006-2008	Binh Duong	500	400
2009-2010	Binh Duong	780	600
2011-2013	Binh Duong	1000	800
2014-2015	Binh Duong	1100	1000
2016-2020	Binh Duong	1400	1200
2001-2005	Ha Noi	170	130
2006-2008	Ha Noi	350	270
2009-2010	Ha Noi	500	330
2011-2015	Ha Noi	750	550
2016-2020	Ha Noi	1400	1100
2005-2008	Da Nang	300	200
2009-2012	Da Nang	500	400
2013-2015	Da Nang	800	600
2016-2018	Da Nang	1500	1300
2010-2011	Binh Phuoc	390	300

Notes: These cutoffs are measured in monthly income per capita (unit: thousand VND). Local authorities can use their own poverty lines (or National poverty line, whichever is higher) under the following conditions:
(i) Average income per capita of the province/city is higher than national average income per capita
(ii) Poverty Headcount Ratio of the province/city is lower than that of the country
(iii) The province/city has enough funds to support their poor households.

Table A2: Estimate of monthly per capita monetary values of benefits

Benefit type	Amount ('000 VND)	Year	Source	Number of individual beneficiaries per household	Frequency per year	Amount for a household of 2 adult, 2 children (nominal '000 VND)	Monthly amount for a household of 2 adult, 2 children (2002 '000 VND)	Monthly amount for a household of 2 adult, 2 children (2002 '000 VND) - Selected benefits	Remarks
Free healthcare	70/person/year	2002	(1)	4	1	280	280	280	
Free health insurance	50/person/year	2002	(1)	4	1	200	200	200	
Direct transfer	120/person/month	2007	(2)	1	12	1,440	1,113	93	
Aid for cattle rearing	1,000/household	2008	(3)	1	1	1,000	580	580	
Aid for seeds	2,000/household	2008	(3)	1	1	2,000	1,160	1,160	
Housign aid	5,000/household	2004	(4)	1	1	5,000	4,495	899	(i)
Clean water aid	300/household	2004	(4)	1	1	300	270	270	
Allowance for children edu - lunch	140/child/month x 9 months/year	2007	(5)	2	9	2,520	1,948	216	
Allowance for children edu - books	70/child/month x 9 months/year	2010	(6)	2	9	1,260	627	70	
Free tuition	50/child/month	2010	(6)	2	9	900	448	50	(ii)
Farm improvement	1,000/household	2007	(5)	1	1	1,000	773	773	
Direct transfer new year 2009	200/person	2009	(7)	4	1	800	433	36	
						Total:		*4,626*	
						Per person:		*1,157*	
						Per person monthly:		*96*	

Sources:
(1) Decision No. 139/2002/QĐ-TTg
(2) Decree No. 67/2007/NĐ-CP
(3) Resolution No. 30a/2008/NQ-CP
(4) Decision No. 134/2004/QĐ-TTg
(5) Decision No. 112/2007/QĐ-TTg
(6) Decree No. 49/2010/NĐ-CP
Remarks:
(i) This tends to be a one-time transfer in a 5-year phase, so average this amount over 5 years.
(ii) This tends to be an annual transfer, so average this amount over 12 months/year.
Notes: This estimate only considers benefits that confers explicit monetary amount. There are other benefits that are difficult to quantify in terms of monetary value, such as free allotment of residential and cultivation land, preferential credits, aid through forest keeping, credits for export worker, credits for housing.

Table A3: Housing Criteria, 2001-2015

Year	Phase	Housing conditions characterizing poor households
2001-2005	1	house category (permanent, semi-permanent, temporary)
2006-2010	2	(if previously poor) show no upgrade on house, toilet, water source, eletricity connection, etc.
2011-2015	3	temporary house, no toilet, no electricity

Notes: Information on housing criteria on each phase comes from:
(i) Phase 1: Training Manual For Poverty Alleviation Staff at Commune Levels. Labor and Society Publishing House, Hanoi, 2004.
(ii) Phase 2: Circular 04/2007/TT-BLDTBXH.
(ii) Phase 3: Circular 21/2012/TT-BLDTBXH.

Table A4: Construction of Housing and Asset Indices

Variable	Building blocks	How variable is made
Asset index (accounted for current value)	air conditioner, washing machine, vaccum cleaner, water heater, fridge, camera, gas stove, microwave, food processor, rice cooker; scooter, bike, car, other means of transportation (boat and motorboat); landline phone, internet connection, computer, color TV, radio, speakers, video player; bed, couch set; sewing machine	1st component of Principal Component Analysis
Housing index	type of permanent structure of the house, toilet type, source of lighting, source of drinking water, methods of trash disposal	1st component of Principal Component Analysis

B Sample notes

Table 5: Notes on Samples for Different Analyses

Used for Section or Subsection	Topic of Analysis	Sample	N households	N observations
section 4	Bunching pattern over time How did households bunch?	Full cross-sections	75,000 in 2002; 45,000/round after 2002	369,995
subsection 6.1	Bunching heterogeneity by housing conditions (Proposition 1)	Full cross-sections	75,000 in 2002; 45,000/round after 2002	369,995
subsection 6.2	Learning effects (Proposition 3)	Two-round panels	9,000 for 2002-2004 and 2004-2006 panels 1,800 for 2010-2012 and 2012-2014 panels	18,000/panel 3,600/panel
section 7	Counterfactual	Two-round panels with available consumption data	2002-2004 panel: 4,000 in the baseline and 2,000 in the follow up 2012-2014 panel: 1,800 in the baseline and 1,800 in the follow up	6,000 3,600

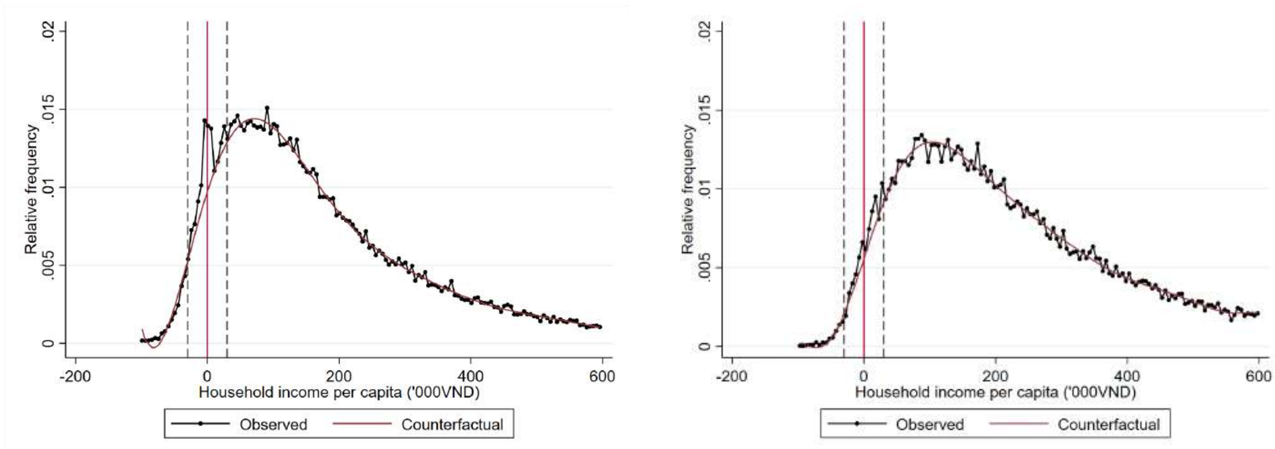
Table 5 makes notes of the samples used for various empirical analyses in this paper. Section 4 documents the bunching evidence and verifies whether such phenomenon is driven by misreporting of income or by labor supply distortion. These analyses need not to utilize the lead program status, so I employ the full cross-sectional sample, which is available for all survey rounds. Each round has 45,000 households (75,000 households for 2002), lending the necessary power to detect and quantify the bunching evidence. In a similar manner, subsection 6.1 unpacks the bunching pattern by housing conditions as predicted by Proposition 1 in the theoretical model. This analysis also utilizes the same full-sample cross-sections.

The remaining analyses in Section 6 test out Proposition 3 of the model, which predicts that over time housing has greater predictive power of the (lead) program status than other criteria. The two-round panels described above are used for these analyses. The 2002-2004 and 2004-2006 panels have about 9,000-10,000 households each. However, the 2010-2012 and 2012-2014 panels have some missing data. These two panels each originally has about 9,000 households, but only 1,800-1,900 of them have information on their status in the National Anti-poverty Program. Therefore, they are considerably smaller compared to the earlier panels.

Section 7 compares the targeting performance of the current program with a hypothetical scenario in which the learning-via-housing mechanism is turned off. This analysis focuses on the 2002-2004 and 2012-2014 panels, when learning arguably has occurred over time. To do this, I evaluate the program status generated by the *status quo* and the hypothetical mechanisms against food consumption – a measure of true neediness. Consumption data is only available for a small random subset of VHLSS (and of the said panels). As a result, this analysis is conducted for the 2002-2004 panel with 4,000 observations in 2002, and 2,000 observation in 2004. For the 2012-2014 panel, all of its 1,800 households have consumption data in both rounds, so all 1,800 observations in each round were include in the analysis.

C Additional Graphs

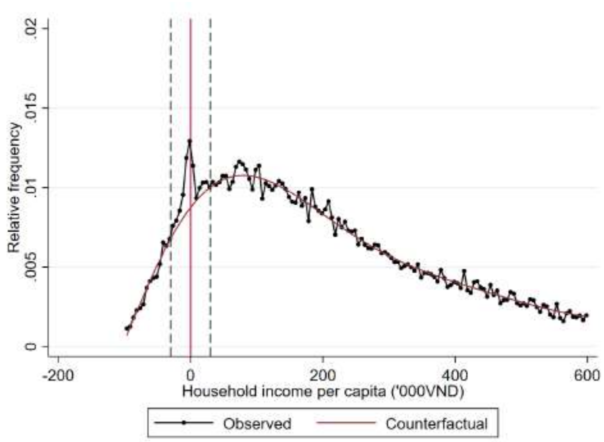
Figure B1: Counterfactual distribution of real income – Phase 1



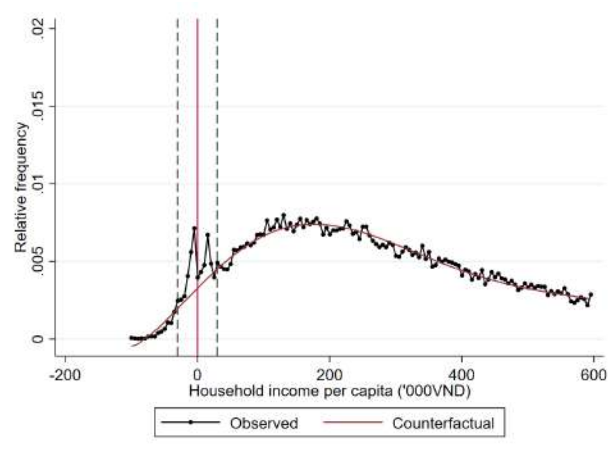
(a) 2002 (b) 2004

Notes: Figure B1a overlays the empirical density (black connected line) and estimated counterfactual density (red smooth curve) of reported income for 2002, Figure B1b does the same of 2004. On horizontal axis is reported per capita income in 2002 terms; the unit is thousand VND. Income is deflated to 2002 terms and also re-centered around the applicable cutoff. The cutoff (zero) is marked with the vertical solid line. The vertical dash lines indicates the boundaries of the bunching region, about VND 30,000. The counterfactual density distribution is estimated with Equation 1.

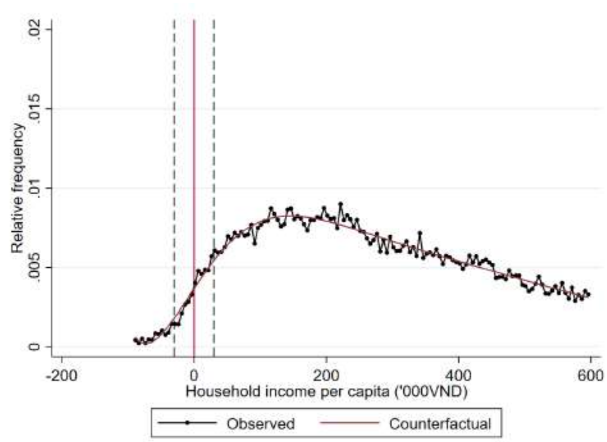
Figure B2: Counterfactual distribution of real income – Phase 2



(a) 2006



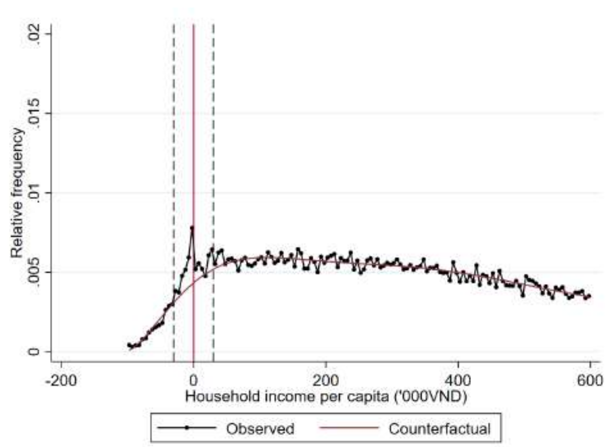
(b) 2008



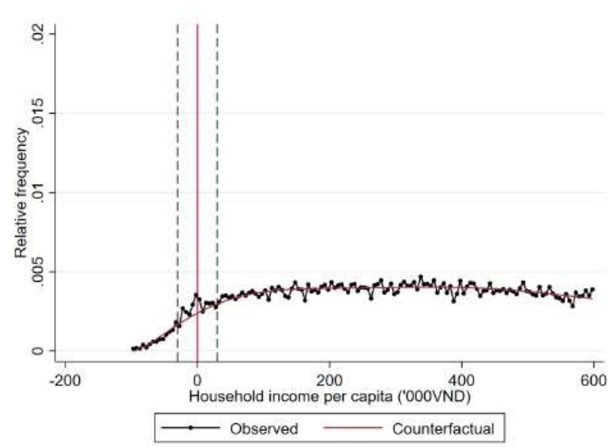
(c) 2010

Notes: Figure B2a overlays the empirical density (black connected line) and estimated counterfactual density (red smooth curve) of reported income for 2006, Figure B2b does the same of 2008, Figure B2c for 2010. On horizontal axis is reported per capita income in 2002 terms; the unit is thousand VND. Income is deflated to 2002 terms and also re-centered around the applicable cutoff. The cutoff (zero) is marked with the vertical solid line. The vertical dash lines indicates the boundaries of the bunching region, about VND 30,000. The counterfactual density distribution is estimated with Equation 1.

Figure B3: Counterfactual distribution of real income – Phase 3



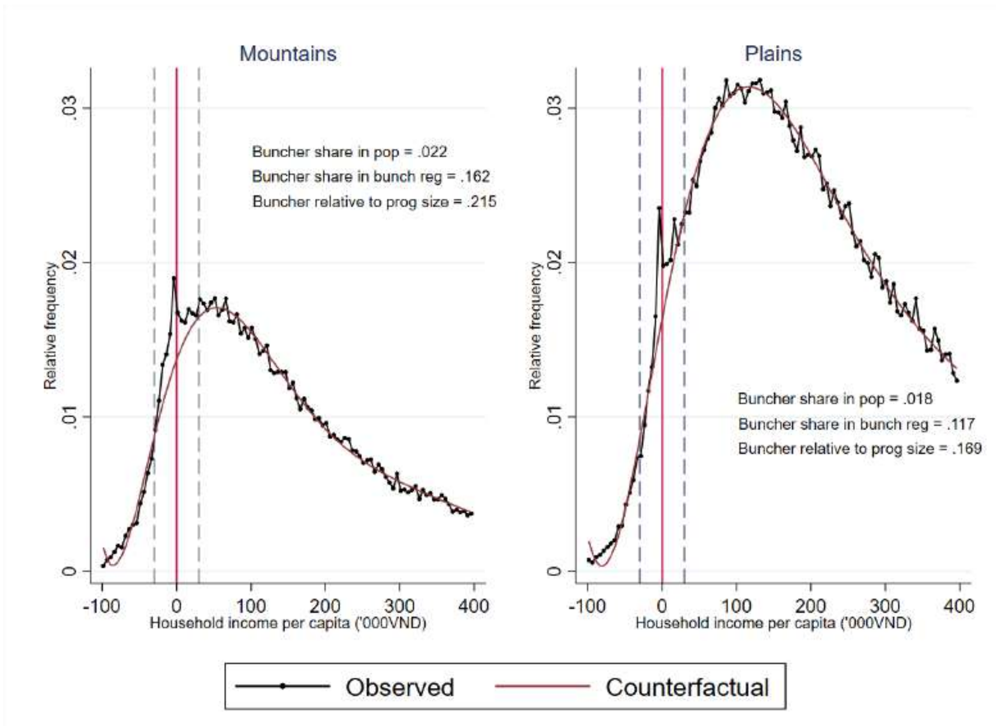
(a) 2012



(b) 2014

Notes: Figure B3a overlays the empirical density (black connected line) and estimated counterfactual density (red smooth curve) of reported income for 2012, Figure B3b does the same of 2012. On horizontal axis is reported per capita income in 2002 terms; the unit is thousand VND. Income is deflated to 2002 terms and also re-centered around the applicable cutoff. The cutoff (zero) is marked with the vertical solid line. The vertical dash lines indicates the boundaries of the bunching region, about VND 30,000. The counterfactual density distribution is estimated with Equation 1.

Figure B4: Bunching by Mountainous versus Plain terrains



Notes: On horizontal axis is reported per capita income in 2002 terms; the unit is thousand VND. Income is deflated to 2002 terms and also re-centered around the applicable cutoff. Each graph presents the empirical and counterfactual distributions of the resulting adjusted income. The income cutoff (zero) is marked with the vertical solid line. The vertical dash lines indicates the boundaries of the bunching region, about VND 30,000. The counterfactual density distribution is estimated with Equation 1. All graphs pool observations from eight cross-sections: 2002, 2004, 2006, 2008, 2010, 2012, 2014. The graphs are plotted separately for mountainous and plain terrains. The texts in the graphs provide estimates for the number of bunchers relative to (i) the population, (ii) the bunching region within VND 30,000 around the income cutoff, and (iii) the number of program participants in the subsequent year (at $t+1$).

D Additional Tables

Table C1: Labor supply around the official income cutoff, by survey round

	Average monthly work hours - Head & Spouse						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	2002	2004	2006	2008	2010	2012	2014
Below cutoff	-0.338	7.008***	-2.095	0.739	5.489	2.507	1.663
	(1.299)	(2.712)	(1.852)	(2.421)	(4.338)	(3.176)	(3.927)
Observations	46492	27055	26660	22472	21376	18063	13959
Mean DV at cutoff	125.187	135.884	140.999	126.368	164.186	168.973	174.312
Mean of DV	133.9	146.1	147.3	143.3	175.5	172.5	171.3
Prov and Round FEs	✓	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓	✓	✓

Notes: All columns report parametric estimates for discontinuity in work hours at the income cutoff. Reported per capita income is deflated to 2002 terms and also re-centered around the applicable cutoff. The dependent variable in all columns is work hours averaged between head and spouse. All regressions control for head's education attainment, head's gender, head's age and its square, urban dummy, minority dummy, household size, household composition measured by the shares of children below 6 and elderlies above 65, province fixed effects and survey round fixed effects. Robust standard errors are in parenthesis.

Table C2: Robustness of Learning Effect via the Housing Signal, during Phase 1 and Phase 3

	Reject										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Housing Index X Follow-up	0.00286***	0.00286**	0.00169*	0.00264***	0.000909	0.00233**	0.00256**	0.00168*	0.00168*	0.00283***	0.00119
	(0.000903)	(0.00142)	(0.00101)	(0.00101)	(0.00179)	(0.000923)	(0.00101)	(0.000878)	(0.000877)	(0.000886)	(0.00178)
Housing Index	0.0000567	0.0000567	0.000840	0.000624	-0.00203	0.000446	0.000592	-0.000984	-0.000976	0.000106	-0.00240
	(0.000962)	(0.00148)	(0.000921)	(0.00106)	(0.00246)	(0.000992)	(0.00106)	(0.00148)	(0.00148)	(0.000989)	(0.00246)
Ln reported income X Follow-up	-0.00782	-0.00782	-0.0115*	-0.00183	-0.0475***	-0.00926	-0.00280	0.00764	0.00763	-0.00817	-0.0450***
	(0.00653)	(0.00977)	(0.00673)	(0.00723)	(0.0126)	(0.00666)	(0.00727)	(0.00726)	(0.00726)	(0.00636)	(0.0125)
Ln reported income	0.0262***	0.0262***	0.0237***	0.0222***	0.0459***	0.0274***	0.0222***	0.0170**	0.0174**	0.0258***	0.0446***
	(0.00618)	(0.00887)	(0.00606)	(0.00669)	(0.0133)	(0.00630)	(0.00675)	(0.00683)	(0.00685)	(0.00600)	(0.0133)
Asset index X Follow-up	-0.000571	-0.000571	0.00151	0.00109	0.000309	-0.000696	0.000956	-0.000800	-0.000702	-0.000532	0.000245
	(0.000834)	(0.00125)	(0.000936)	(0.00120)	(0.00112)	(0.000850)	(0.00118)	(0.000882)	(0.000879)	(0.000816)	(0.00111)
Asset index	0.00198**	0.00198*	0.000396	0.00134	0.000381	0.00204**	0.00119	0.000827	0.000690	0.00193**	0.000324
	(0.000965)	(0.00118)	(0.00105)	(0.00126)	(0.00138)	(0.000980)	(0.00126)	(0.000962)	(0.000956)	(0.000932)	(0.00138)
Number of observations	24548	24548	24498	20904	3644	24406	20902	13710	13708	24548	3644
Mean outcome	0.899	0.899	0.899	0.901	0.888	0.899	0.901	0.912	0.912	0.899	0.888
Household FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Flexible location-specific time trend	Prov-Round	Prov-Round	District-Round	Prov-Round	Prov-Round	Prov-Round	Prov-Round	Prov-Round	Prov-Round	Prov-Round	Prov-Round
Add. controls						Head & spouse's industry	Bad shocks		Others' housing	Lag Tuition Expt	Lag Benefits
F-test of joint significance of benefits (p-value)										0.179	0.0468
Cluster	District	Province	Enum. Area	Enum. Area	Enum. Area	Enum. Area	Enum. Area	Enum. Area	Enum. Area	Enum. Area	Enum. Area
Subsample	All	All	All	2002-2004	2012-2014	All	2002-2004	Δ Housing=0	Δ Housing=0	2002-2004	2012-2014

Notes: The dependent variable is all columns is Lead *Accept* at $t+1$. Columns (1)-(4) pool together observations from two panels: 2002-2004 (during Phase 1), and 2012-2014 (during Phase 3). Column (1) and (2) implements the base specification in **??**, but cluster standard errors at the district level and province level, respectively. Column (3) gets back to clustering standard errors at the enumeration areas, but replace province-round fixed effects with district-round fixed effects. Columns (4)-(6) estimate the base specification, but further controls for changes in other dimensions that may contributes to changes in the officer's decision over time. Column (4) adds the share of self-employed adults and its interaction with the follow-up round dummy. Column (5) adds the head's and spouse's industry fixed effect and their interactions with the follow-up round dummy. Column (6) adds a dummy for experiencing bad shocks in the last 12 months (available only for the 2002-2004 panel), and interaction with the follow-up round dummy. All three columns (4)-(6) also control for urban dummy, minority dummy, education and age of household's head, female head dummy, household composition measured by the shares of children below 6 and elderlies above 65, and household size. Column (7) gets back to the base specification, but restricts the sample to households who experienced no change in housing conditions after two years. Columns (8)-(9) adds measures of past benefit amounts at $t-1$ to better proxy for the officer's past belief. Column (8) adds the discount percentages on tuition and other education fees for children of poor households, which is available only for the 2002-2004 panel. Column (9) adds the count of benefits and the amount of electricity subsidy the household received, available only for the 2012-2014 panel. The p-value of the F-test for the joint significance of past-benefit variables are provided for these two columns. All regressions are estimated with OLS. In all but columns (1) and (2), standard errors are clustered at the enumeration area level.

Table C3: Impact of Selection Criteria on Program Status over Time by Predictor of Bunching, within Phase 1 and Phase 3

	Reject					
	(1)	(2)	(3)	(4)	(5)	(6)
Housing Index X Follow-up	0.00815***	0.000875	0.00389***	0.000550	0.00608**	0.000600
	(0.00298)	(0.000650)	(0.00144)	(0.00120)	(0.00254)	(0.000828)
Housing Index	-0.000277	0.000199	-0.000883	0.00204*	-0.00104	0.00171*
	(0.00292)	(0.000822)	(0.00139)	(0.00117)	(0.00292)	(0.000904)
Ln reported income X Follow-up	-0.0640**	0.0165***	-0.0160**	0.00499	0.00913	-0.00860
	(0.0259)	(0.00481)	(0.00767)	(0.0105)	(0.0161)	(0.00764)
Ln reported income	0.0887***	-0.00232	0.0365***	0.000331	0.0326*	0.0180***
	(0.0263)	(0.00441)	(0.00767)	(0.00778)	(0.0182)	(0.00658)
Asset index X Follow-up	-0.00625*	0.000994	-0.000383	0.000461	0.000161	0.00217*
	(0.00339)	(0.000681)	(0.00126)	(0.000973)	(0.00335)	(0.00120)
Asset index	0.0105***	0.0000120	0.00323**	-0.00108	0.00626*	-0.000732
	(0.00386)	(0.000721)	(0.00142)	(0.000964)	(0.00371)	(0.00120)
Number of observations	8016	16528	18730	5818	5884	14676
Mean outcome	0.779	0.957	0.882	0.952	0.857	0.919
Household FEs	✓	✓	✓	✓	✓	✓
Province-by-Round FEs	✓	✓	✓	✓	✓	✓
Cluster	Enum. Area	Enum. Area	Enum. Area	Enum. Area	Enum. Area	Enum. Area
Subsample	Bottom tercile reported income	Top two terciles reported income	Rural	Urban	Mountainous	Plain

Notes: All columns implement the base specification in **??**, pooling observations from two panels: 2002-2004 (during Phase 1) and 2012-2014 (during Phase 3). Columns (1) and (2) estimate the base specification separately for two subsamples: the bottom tercile and the top two terciles of reported income, respectively. Columns (3) and (4) do the same for rural and urban areas, respectively. Columns (5) and (6) for mountainous terrains and the plains, respectively. All regressions are estimated with OLS. Standard errors in parentheses are clustered at the enumeration area level.

Table C4: Comparison between the *status quo* allocation with learning and the hypothetical allocating without learning

	Baseline Round		Follow-up Round				
	Actual	Predicted	Actual	Predicted with learning (status quo)	Predicted without learning (counterfactual)	Difference (3) - (5)	Difference (4) - (5)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				A. 2002-2004 Panel			
Total error percentage	12.67%	12.87%	15.41%	15.32%	15.71%	-0.29 p.p.	-0.39 p.p
Undercoverage rate	81.41%	82.69%	66.81%	66.38%	68.12%	-1.31 p.p	-1.75 p.p
Leakage rate	82.26%	83.49%	68.46%	68.05%	69.71%	-1.24 p.p	-1.66 p.p
Targeting differential	11.54%	10.15%	24.19%	24.68%	22.72%	1.47 p.p	1.96 p.p
N obs	4,127	4,127	2,063	2,063	2,063		
Avg. N obs have consumption data in a province	78	78	38	38	38	-	-
Avg. % obs have consumption data in a province	42.24%	42.24%	19.72%	19.72%	19.72%	-	-
				B. 2012-2014 Panel			
Total error percentage	13.76%	14.55%	14.55%	14.33%	14.22%	0.33 p.p	0.11 p.p
Undercoverage rate	59.61%	67.88%	67.88%	66.84%	66.32%	1.55 p.p	0.52 p.p
Leakage rate	60.95%	68.37%	68.37%	67.35%	66.84%	1.53 p.p	0.51 p.p
Targeting differential	32.43%	23.89%	23.89%	25.05%	25.63%	-1.74 p.p	-0.58 p.p
N obs	1,810	1,821	1,821	1,821	1,821		
Avg. N obs have consumption data in a province	35	35	35	35	35	-	-
Avg. % obs have consumption data in a province	20.16%	20.08%	20.08%	20.08%	20.08%	-	-

Notes: The sample for this analysis is restricted to households in the 2002-2004 and 2012-2014 panels with consumption data. (1) I use per capita food consumption to measure true needs, then I define province-specific true poverty by: a) find s = the share of accepted households in each province, b) classify households in the bottom s% based on per capita food consumption as truly poor. (2) In a similar manner, I predict the selection decision following the status-quo mechanism. Using the regression results, I predict the probability of being accepted to the program, then suppose that the bottom s% of households in the province in terms of this predicted probability would be accepted to the program. (3) I construct the counterfactual selection decision as in (2) but assume there is no learning via housing conditions over time (coefficient on Housing X Follow-up = 0). Using the true poverty definition and the predicted selection rule under each mechanism, I calculate the following targeting performance metrics:
(i) Total error percentage = incorrectly excluded poors and incorrectly included nonpoors as a share of the sample.
(ii) Undercoverage rate = share of poor households incorrectly excluded.
(iii) Leakage rate = share of program participants incorrectly included as they are nonpoor.
(iv) Targeting differential = difference between proportion of poor and proportion of nonpoor who are accepted to the program; varies between -100% (only the nonpoor gets accepted) and 100% (perfect targeting).

Table C5: Impact of Selection Criteria on Program Status over Time, Within Phase versus Between Phases

	Reject			
	(1) Within Phase	(2) Between Phases	(3) Phase 1 to Phase 2	(4) Phase 2 to Phase 3
Housing Index X Follow-up	0.00286***	0.000388	0.000391	-0.00155
	(0.000896)	(0.000790)	(0.000877)	(0.00177)
Housing Index	0.0000567	-0.000349	-0.000686	0.00298
	(0.000990)	(0.00106)	(0.00119)	(0.00229)
Ln reported income X Follow-up	-0.00782	0.00266	0.00829	-0.0299**
	(0.00639)	(0.00637)	(0.00725)	(0.0122)
Ln reported income	0.0262***	0.00768	0.00772	0.00987
	(0.00605)	(0.00628)	(0.00696)	(0.0145)
Asset index X Follow-up	-0.000571	0.00185**	0.00254***	0.00195*
	(0.000819)	(0.000725)	(0.000972)	(0.00112)
Asset index	0.00198**	-0.000621	-0.00101	-0.000202
	(0.000934)	(0.000947)	(0.00119)	(0.00160)
Number of observations	24548	22778	19058	3720
Mean outcome	0.899	0.876	0.878	0.866
Household FEs	✓	✓	✓	✓
Province-by-Round FEs	✓	✓	✓	✓
Cluster	Enum. Area	Enum. Area	Enum. Area	Enum. Area
Subsample	2002-2004 and 2012-2014	2004-2006 and 2010-2012	2004-2006	2010-2012

Notes: All columns implement the base specification in Equation 12. Column (1) reposts the main results from column (2) in Table 6; its sample includes observations from two panels: 2002-2004 (during Phase 1) and 2012-2014 (during Phase 3). Column (2) pools observations from two other panels: 2004-2006 (transitioning between Phase 1 and Phase 2) and 2010-2012 (transitioning between Phase 2 and Phase 3). Columns (3) and (4) reestimates the base specification separately for the 2004-2006 panel and the 2010-2012 panel, respectively. All regressions are estimated with OLS. Standard errors in parentheses are clustered at the enumeration area level.

Table C6: Monetary value of reduction on leakage/undercoverage due to learning, 2004

Row no.	Field	Source	Estimated value
(1)	Total number of households in the population	VHLSS, 2004	17,503,087
(2)	Program participation rate	VHLSS, 2004	11.59%
(3)	Number of participating households	implied calculation	2,028,608
(4)	Mean household size	VHLSS, 2004	4.36
(5)	Monthly benefits per person	self-estimated from various policy documents	₫ 48,000
(6)	Total program cost for 2 years (A)	implied calculation from (4), (5), (6)	B ₫ 10,178.60
(7)	Total program cost for 2 years (B)	2001 policy decree (No: 143/2001/QĐ-TTg): B₫ 22,580 for 5 years	B ₫ 9,032.00
(8)	Reduction in leakage	Table 9	1.66%
(9)	Saving on leakage (A)	implied calculation	B ₫ 168.96
(10)	Saving on leakage (B)	implied calculation	B ₫ 149.93
(11)	PPP conversion rate USD to VND	World Bank, 2004 average	B ₫ 3,191
(12)	Saving on leakage in USD (A)	implied calculation	M $ 36.36
(13)	Saving on leakage in USD (B)	implied calculation	M $ 32.27

Notes: The implied calculations in rows (3), (5)-(7), (9), (10), (12), (13) are carried out as followed:
Row (3) = (1) x (2)
Row (5) = 50% of monthly per capita benefits estimate in Table A2
Row (6) = (3) x (4) x (5) x 24 months
Row (7) = (22,580 / 5) x 2
Row (9) = (6) x (8)
Row (10) = (7) x (8)
Row (12) = (9) / (11)
Row (13) = (10) / (11)

E Derivations and Proofs

E.1 Proof of Proposition 2

Proposition 2 states if ρ_{ϵ_t} increases over time, the fraction of bunching households is decreasing over time.

Proof. According to the theoretical model, only households with true income in the range $[\bar{\theta}, \theta_{h_t^*}]$ with physical housing condition below h_t^* will bunch. Thus the fraction of bunching households according to the theoretical model can be defined as $\zeta_t = \int_{\bar{\theta}}^{\theta_{h_t^*}} \int_{-\infty}^{h_t^*} f(\theta, h_t) dh_t d\theta$.

Consider first the probability of bunching for a given type θ, ζ_t^θ. This is defined as the probability that the realization of the housing signal h_t falls below some "conceivable" threshold h_t^*, so $\zeta_t^\theta = \int_{-\infty}^{h_t^*} f(h_t|\theta) dh_t = F(h_t^*|\theta_i)$. The housing signal h_t is generated by $h_{it} = \theta_i + \epsilon_{it}$. Since both θ_i and ϵ_{it} are normal, and ϵ_{it} is independent of θ_i, the conditional distribution of $h_{it}|\theta_i$ is also normal: $h_{it}|\theta_i \sim \mathcal{N}(\theta_i, \sigma_{\epsilon_t}^2)$. The cumulative (conditional) probability $\zeta_t^\theta = F(h_t^*|\theta_i)$ thus has the functional form:

$$\zeta_t^\theta = \Phi(\frac{h_t^* - \theta_i}{\sigma_{\epsilon_t}}) = \frac{1}{\sqrt{2\pi}} \int_{-\infty}^{\frac{h_t^* - \theta_i}{\sigma_{\epsilon_t}}} e^{-s^2/2} ds. \tag{14}$$

Define the "conceivable" threshold h_t^* as the 99th percentile of housing conditions that the officer believes a household of type $\bar{\theta}$ could live in:

$$h_t^* = F^{-1}_{h_{it}|\bar{\theta}}(0.99) = \mu_{h_{it}|\bar{\theta}} + \sigma_{h_{it}|\bar{\theta}} \Phi^{-1}_{h_t|\bar{\theta}}(0.99) = \bar{\theta} + \sigma_{\epsilon_t} \Phi^{-1}_{h_t|\bar{\theta}}(0.99) \approx \bar{\theta} + \sigma_{\epsilon_t} 2.58 = \bar{\theta} + 2.58 \rho_{\epsilon_t}^{-1/2},$$

where $\Phi^{-1}(p)$ for $p \in (0,1)$ is the quantile function of the standard normal distribution. From this definition, it is straightforward that $\frac{\partial h_t^*}{\partial \rho_{\epsilon_t}} = -1.29 \rho_{\epsilon_t}^{-3/2} < 0$, since ρ_{ϵ_t} is non-negative. A more precise signal lowers the highest "conceivable" housing conditions a household of type $\bar{\theta}$ could possibly live in.

Substitute in $\frac{1}{\sigma_{\epsilon_t}} = \sqrt{\rho_{\epsilon_t}}$ and differentiate ζ_t^θ in Equation 14 with respect to ρ_{ϵ_t} by Leibniz's rule we have:

$$\frac{\partial \omega_t^\theta}{\partial \rho_{\epsilon_t}} = \frac{1}{\sqrt{2\pi}} \frac{\partial}{\partial \rho_{\epsilon_t}} \left(\int_{-\infty}^{(h_t^* - \theta_i)\sqrt{\rho_{\epsilon_t}}} e^{-s^2/2} ds \right) = \underbrace{e^{-\left(\rho_{\epsilon_t}(h_t^* - \theta_i)\right)^2/2}}_{>0} \frac{\partial}{\partial \rho_{\epsilon_t}} \left((h_t^*(\rho_{\epsilon_t}) - \theta_i)\sqrt{\rho_{\epsilon_t}} \right)$$

Continue expanding $\frac{\partial}{\partial \rho_{\epsilon_t}}\left((h_t^*(\rho_{\epsilon_t})-\theta_i)\sqrt{\rho_{\epsilon_t}}\right)$ and substitute in $\frac{\partial h_t^*}{\partial \rho_{\epsilon_t}} \approx -1.29\rho_{\epsilon_t}^{-3/2}$ and $\rho_{\epsilon_t} = \sigma_{\epsilon_t}^{-2}$:

$$\frac{\partial}{\partial \rho_{\epsilon_t}}\left((h_t^*(\rho_{\epsilon_t})-\theta_i)\sqrt{\rho_{\epsilon_t}}\right) = \rho_{\epsilon_t}^{-1/2}\left(\frac{h_t^*-\theta_i}{2} - 1.29\rho_{\epsilon_t}^{-1/2}\right) = \sigma_{\epsilon_t}\left(\frac{h_t^*-\theta_i}{2} - 1.29\sigma_{\epsilon_t}\right)$$

Therefore $\frac{\partial \zeta_t^\theta}{\partial \rho_{\epsilon_t}} < 0$ if $h_t^* < \theta_i + 2.58\sigma_{\epsilon_t}$, which is true for $\forall \theta_i > \bar{\theta}$.

Finally, since $\zeta_t = \int_{\bar{\theta}}^{\theta_{h_t^*}} \int_{-\infty}^{h_t^*} f(\theta, h_t) dh_t d\theta = \int_{\bar{\theta}}^{\theta_{h_t^*}} \theta \left(\int_{-\infty}^{h_t^*} f(h_t|\theta) dh_t\right) f(\theta) d\theta = \int_{\bar{\theta}}^{\theta_{h_t^*}} \theta \zeta_t^\theta f(\theta) d\theta$, $\frac{\partial \zeta_t^\theta}{\partial \rho_{\epsilon_t}} < 0$ results in $\frac{\partial \zeta_t}{\partial \rho_{\epsilon_t}} < 0$. If $\frac{\partial \rho_{\epsilon_t}}{\partial t} > 0$, then $\frac{\partial \zeta_t}{\partial t} = \frac{\partial \zeta_t}{\partial \rho_{\epsilon_t}} \frac{\partial \rho_{\epsilon_t}}{\partial t} < 0$. □

E.2 Derivation of Bayesian updating by housing segment (above or below h_t^*)

Proof. **If the officer observes** $h_{it} > h_t^*$, she is confident that all these households will tell the truth. Thus she utilizes information from both housing conditions and reported income:

$$f(\theta_i|h_{it}, \tilde{\theta}_{it})\mathbb{I}\{h_i > h_t^*\} = \frac{f(\tilde{\theta}_{it}|\theta_i, h_{it}) f(\theta_i|h_{it})}{f(\tilde{\theta}_{it}|h_{it})}\mathbb{I}\{h_{it} > h_t^*\}$$

Consider first the Bayesian updating process with a truncated housing distribution $f(\theta_i|h_{it})\mathbb{I}\{h_{it} > h_t^*\}$, which can be modified from the updating formula with the full support $f(\theta_i|h_{it}) = \frac{f(h_{it}|\theta_i)f(\theta_i)}{f(h_{it})}$. First, the unconditional distribution of housing conditions $f(h_{it})$ is truncated below at h_t^*. Second, when the officer observes a housing realization above h_t^*, she can infer that the true income of such households must be greater than $\bar{\theta}$. Due to their high draw of housing conditions, these households will surely tell the truth. Note that, these households include the types above the marginal value $\theta_{h_t^*}$ who would surely receive a housing realization higher than h_t^* (and surely cannot bunch), and the types in the bunching range $[\bar{\theta}, \theta_{h_t^*}]$ who potentially could bunch, but actually cannot do so due to a high housing draw above h_t^*. Consequently, when updating her belief with housing values truncated below at h_t^*, her prior is also truncated below at $\bar{\theta}$ (which is lower than $\theta_{h_t^*}$). Finally, the likelihood function $f(h_{it}|\theta_i)$ is also truncated below and above at the 1st and 99th percentiles ($\theta_i - \epsilon_t^*$ and $\theta_i + \epsilon_t^*$, respectively) to reflect the boundedness of $h_{it}|\theta_i$.

Bayesian updating only for households with their physical house scoring above h_t^* is given by:

$$
\begin{aligned}
f(\theta_i|h_{it})\mathbb{I}\{h_{it} > h_t^*\} &= \frac{f_{\theta_i \in [\theta_i - \epsilon_t^*, \theta_i + \epsilon_t^*]}(h_{it}|\theta_i) f_{\theta_i > \bar{\theta}}(\theta_i)}{f_{h_{it} > h_t^*}(h_{it})} \mathbb{I}\{\theta_i > \bar{\theta}\} \\
&= \frac{\frac{f(h_{it}|\theta_i)}{\sigma_{\epsilon_t}\left[\Phi\left(\frac{\theta_i + \epsilon_t^* - \theta_i}{\sigma_{\epsilon_t}}\right) - \Phi\left(\frac{\theta_i - \epsilon_t^* - \theta_i}{\sigma_{\epsilon_t}}\right)\right]} \frac{f(\theta_i)}{\sigma_\theta\left[1 - \Phi\left(\frac{\bar{\theta} - \mu_\theta}{\sigma_\theta}\right)\right]}}{\frac{f(h_t)}{\sqrt{\sigma_\theta^2 + \sigma_{\epsilon_t}^2}\left[1 - \Phi\left(\frac{h_t^* - \mu_\theta}{\sqrt{\sigma_\theta^2 + \sigma_{\epsilon_t}^2}}\right)\right]}} \mathbb{I}\{\theta_i > \bar{\theta}\}
\end{aligned}
$$

where $\phi(.)$ is the standardized normal density, $f(h_{it}|\theta_i)$, $f(\theta_i)$ and $f(h_{it})$ are untruncated distributions.[53] Note that the indicator $\mathbb{I}\{h_{it} > h_t^*\}$ is superseded by the indicator $\mathbb{I}\{\theta_i > \bar{\theta}\}$ because the officer can infer the appropriate types who possibly have housing values above h_t^*. Let $k_t = \frac{\sqrt{\sigma_\theta^2 + \sigma_{\epsilon_t}^2}\left[1 - \Phi\left(\frac{h_t^* - \mu_\theta}{\sqrt{\sigma_\theta^2 + \sigma_{\epsilon_t}^2}}\right)\right]}{\sqrt{\sigma_{\epsilon_t}^2 \sigma_\theta^2}\left[\Phi\left(\frac{\epsilon_t^*}{\sigma_{\epsilon_t}}\right) - \Phi\left(\frac{-\epsilon_t^*}{\sigma_{\epsilon_t}}\right)\right]\left[1 - \Phi\left(\frac{\theta_{h_t^*} - \mu_\theta}{\sigma_\theta}\right)\right]}$ be a normalizing constant. The expression above works out as:

$$
\begin{aligned}
f(\theta_i|h_{it})\mathbb{I}\{h_{it} > h_t^*\} &= k_t \frac{f(h_{it}|\theta_i) f(\theta_i)}{f(h_{it})} \mathbb{I}\{\theta_i > \bar{\theta}\} \\
&\propto k_t \phi\left(\frac{h_{it} - \theta_i}{\sigma_{\epsilon_t}}\right) \phi\left(\frac{\theta_i - \mu_\theta}{\sigma_\theta}\right) \mathbb{I}\{\theta_i > \bar{\theta}\}
\end{aligned} \tag{15}
$$

Note that the term $f(h_{it})$ in the denominator can be ignored because it is just a constant assuring that the posterior will integrate to 1. Simplifying the multiplication of the two normal distributions yields:

$$
\begin{aligned}
\phi\left(\frac{h_{it} - \theta_i}{\sigma_{\epsilon_t}}\right) \phi\left(\frac{\theta_i - \mu_\theta}{\sigma_\theta}\right) &= \frac{1}{\sqrt{2\pi\sigma_{\epsilon_t}^2}} e^{-\frac{(h_{it} - \theta_i)^2}{2\sigma_{\epsilon_t}^2}} \frac{1}{\sqrt{2\pi\sigma_\theta^2}} e^{-\frac{(\theta_i - \mu_\theta)^2}{2\sigma_\theta^2}} \\
&\propto e^{\frac{-\theta_i^2 + 2\mu_\theta\theta_i - \mu_\theta^2}{2\sigma_\theta^2} + \frac{-h_{it}^2 + 2h_{it}\theta_i - \theta_i^2}{2\sigma_{\epsilon_t}^2}} \\
&= e^{\frac{-\theta_i^2(\sigma_{\epsilon_t}^2 + \sigma_\theta^2) + 2\theta_i(\sigma_{\epsilon_t}^2\mu_\theta + \sigma_\theta^2 h_{it}) - (\sigma_{\epsilon_t}^2\mu_\theta^2 + \sigma_\theta^2 h_{it}^2)}{2\sigma_\theta^2\sigma_{\epsilon_t}^2}} \\
&= e^{\frac{-\theta_i^2 + 2\theta_i\frac{\sigma_{\epsilon_t}^2\mu_\theta + \sigma_\theta^2 h_{it}}{\sigma_{\epsilon_t}^2 + \sigma_\theta^2} - \left(\frac{\sigma_{\epsilon_t}^2\mu_\theta + \sigma_\theta^2 h_{it}}{\sigma_{\epsilon_t}^2 + \sigma_\theta^2}\right)^2}{2\frac{\sigma_\theta^2\sigma_{\epsilon_t}^2}{\sigma_{\epsilon_t}^2 + \sigma_\theta^2}}} e^{\frac{(h_{it} - \mu_\theta)^2}{2(\sigma_{\epsilon_t}^2 + \sigma_\theta^2)}}
\end{aligned}
$$

[53]Unconditionally, h_{it} is normal with $\mathcal{N}(\mu_\theta, \sigma_\theta^2 + \sigma_{\epsilon_t}^2)$. However, conditioning on true income θ_i, the housing signal $h_{it}|\theta_i$ follows: $h_{it}|\theta_i \sim \mathcal{N}(\theta_i, \sigma_{\epsilon_t}^2)$.

$$\propto \frac{1}{\sqrt{2\pi\frac{\sigma_\theta^2\sigma_{\epsilon_t}^2}{\sigma_{\epsilon_t}^2+\sigma_\theta^2}}} e^{\frac{-\left(\theta_i - \frac{\sigma_{\epsilon_t}^2\mu_\theta+\sigma_\theta^2 h_{it}}{\sigma_{\epsilon_t}^2+\sigma_\theta^2}\right)^2}{2\frac{\sigma_\theta^2\sigma_{\epsilon_t}^2}{\sigma_{\epsilon_t}^2+\sigma_\theta^2}}}$$

Thus the Bayesian updating formula for $h_{it} > h_t^*$ works out to:

$$f(\theta_i|h_{it})\mathbb{I}\{h_{it} > h_t^*\} = k_t\phi\left(\frac{\theta_i - \mu_{\theta_i|h_{it}}}{\sigma_{\theta_i|h_{it}}}\right)\mathbb{I}\{\theta_i > \bar{\theta}\} \tag{16}$$

where $\mu_{\theta_i|h_{it}} = \frac{\sigma_{\epsilon_t}^2\mu_\theta+\sigma_\theta^2 h_{it}}{\sigma_{\epsilon_t}^2+\sigma_\theta^2}$, $\sigma_{\theta_i|h_{it}}^2 = \frac{\sigma_\theta^2\sigma_{\epsilon_1}^2}{\sigma_{\epsilon_1}^2+\sigma_\theta^2}$ and k_t now simplifies to $k_t \approx \frac{1-\Phi\left(\frac{h_t^*-\mu_\theta}{\sqrt{\sigma_\theta^2+\sigma_{\epsilon_t}^2}}\right)}{1-\Phi\left(\frac{\theta-\mu_\theta}{\sigma_\theta}\right)} \equiv \frac{1-F_{h_{it}}(h_t^*)}{1-F_{\theta_i}(\theta)}$.[54] Denote these parameters with the precision (instead of variance) of distributions, we have: $\mu_{\theta_i|h_{it}} = (1-a_t)\mu_\theta + a_t h_{it}$ and $\rho_{\theta_i|h_{it}} = \rho_\theta + \rho_{\epsilon_t}$ where $a_t = \frac{\rho_{\epsilon_t}}{\rho_\theta+\rho_{\epsilon_t}}$.[55]

Next the officer updates her belief with reported income, given the household has its physical housing above h_t^*.

$$\begin{aligned} f(\theta_i|h_{it},\tilde{\theta}_{it})\mathbb{I}\{h_i > h_t^*\} &= \frac{f(\tilde{\theta}_{it}|\theta_i,h_{it})f(\theta_i|h_{it})}{f(\tilde{\theta}_{it}|h_{it})}\mathbb{I}\{\theta_i > \bar{\theta}\} \\ &\propto f(\tilde{\theta}_{it}|\theta_i,h_{it})f(\theta_i|h_{it})\mathbb{I}\{\theta_i > \bar{\theta}\} \\ &= \phi\left(\frac{\tilde{\theta}_{it}-\theta_i}{\sigma_\eta}\right)k_t\phi\left(\frac{\theta_i-\mu_{\theta_i|h_{it}}}{\sigma_{\theta_i|h_{it}}}\right)\mathbb{I}\{\theta_i > \bar{\theta}\} \end{aligned}$$

Again the denominator can be ignored as it only acts as a scaling constant. These households tell the truth, so the functional form for the conditional density distribution of messages take the form: $f(\tilde{\theta}_{it}|\theta_i,h_{it})\mathbb{I}\{\theta_i > \bar{\theta}\} = \phi\left(\frac{\tilde{\theta}_{it}-\theta_i}{\sigma_\eta}\right)$. Substitute the result for $f(\theta_i|h_{it})$ from above, then reiterate a similar process simplifying Equation 15 to evaluate these two normal densities:

$$\begin{aligned} \phi\left(\frac{\tilde{\theta}_{it}-\theta_i}{\sigma_\eta}\right)\phi\left(\frac{\theta_i-\mu_{\theta_i|h_{it}}}{\sigma_{\theta_i|h_{it}}}\right) &= \frac{1}{\sqrt{2\pi\sigma_{\theta_i|h_{it}}^2}}e^{-\frac{(\theta_i-\mu_{\theta_i|h_{it}})^2}{2\sigma_{\theta_i|h_{it}}^2}}\frac{1}{\sqrt{2\pi\sigma_\eta^2}}e^{-\frac{(\tilde{\theta}_{i1}-\theta_i)^2}{2\sigma_\eta^2}} \\ &\propto e^{\frac{-\theta_i^2+2\theta_i\mu_{\theta_i|h_{it}}-\mu_{\theta_i|h_{it}}^2}{2\sigma_{\theta_i|h_{it}}^2}+\frac{-\tilde{\theta}_{i1}^2+2\tilde{\theta}_{i1}\theta_i-\theta_i^2}{2\sigma_\eta^2}} \\ &= e^{\frac{-\theta_i^2\left(\sigma_\eta^2+\sigma_{\theta_{i1}|h_i}^2\right)+2\theta_i\left(\sigma_\eta^2\mu_{\theta_i|h_{it}}+\sigma_{\theta_i|h_{it}}^2\tilde{\theta}_{i1}\right)-\left(\sigma_\eta^2\mu_{\theta_i|h_{it}}^2+\sigma_{\theta_i|h_{it}}^2\tilde{\theta}_{i1}^2\right)}{2\sigma_{\theta_i|h_{it}}^2\sigma_\eta^2}} \end{aligned}$$

[54]The term $\left[\Phi\left(\frac{\epsilon_t^*}{\sigma_{\epsilon_t}}\right) - \Phi\left(\frac{-\epsilon_t^*}{\sigma_{\epsilon_t}}\right)\right] \approx 1$ when $-\epsilon_t^*$ and ϵ_t^* are the 1st and 99th percentile of ϵ_t.

[55]For a given random variable X, σ_X^2 denotes its variance and $\rho_X = \frac{1}{\sigma_X^2}$ denotes its precision.

$$= e^{\frac{-\theta_i^2+2\theta_i\frac{\sigma_\eta^2\mu_{\theta_i|h_{it}}+\sigma^2_{\theta_i|h_{it}}\tilde{\theta}_{i1}}{\sigma_\eta^2+\sigma^2_{\theta_i|h_{it}}}-\left(\frac{\sigma_\eta^2\mu_{\theta_i|h_{it}}+\sigma^2_{\theta_i|h_{it}}\tilde{\theta}_{i1}}{\sigma_\eta^2+\sigma^2_{\theta_i|h_{it}}}\right)^2}{2\frac{\sigma^2_{\theta_i|h_{it}}\sigma_\eta^2}{\sigma_\eta^2+\sigma^2_{\theta_i|h_{it}}}}} e^{\frac{\left(\tilde{\theta}_{i1}-\mu_{\theta_i|h_{it}}\right)^2}{2\left(\sigma_\eta^2+\sigma^2_{\theta_i|h_{it}}\right)}}$$

$$\propto \frac{1}{\sqrt{2\pi\frac{\sigma^2_{\theta_i|h_{it}}\sigma_\eta^2}{\sigma_\eta^2+\sigma^2_{\theta_i|h_{it}}}}} e^{\frac{-\left(\theta_i-\frac{\sigma_\eta^2\mu_{\theta_i|h_{it}}+\sigma^2_{\theta_i|h_{it}}\tilde{\theta}_{i1}}{\sigma_\eta^2+\sigma^2_{\theta_i|h_{it}}}\right)^2}{2\frac{\sigma^2_{\theta_i|h_{it}}\sigma_\eta^2}{\sigma_\eta^2+\sigma^2_{\theta_i|h_{it}}}}}$$

Therefore we have the result:

$$f(\theta_i|h_{it},\tilde{\theta}_{it})\mathbb{I}\{h_i > h_t^*\} = k_t\phi\left(\frac{\theta_i-\mu_{\theta_i|(h_{it},\tilde{\theta}_{it})}}{\sigma_{\theta_i|(h_{it},\tilde{\theta}_{it})}}\right)\mathbb{I}\{\theta_i > \theta_{h_t^*}\}$$

where $k_t = \frac{1-F_{h_{it}}(h_t^*)}{1-F_{\theta_i}(\theta_{h_t^*})}$, $\mu_{\theta_i|h_{it},\tilde{\theta}_{it}} = \frac{\sigma_\eta^2\mu_{\theta_i|h_{it}}+\sigma^2_{\theta_i|h_{it}}\tilde{\theta}_{it}}{\sigma_\eta^2+\sigma^2_{\theta_i|h_{it}}}$ and $\sigma^2_{\theta_i|h_{it},\tilde{\theta}_{it}} = \frac{\sigma^2_{\theta_i|h_{it}}\sigma_\eta^2}{\sigma_\eta^2+\sigma^2_{\theta_i|h_{it}}}$. Again rewrite the mean and variance with precision notations, then

$$\mu_{\theta_i|h_{it},\tilde{\theta}_{it}} = (1-b_t)(1-a_t)\mu_\theta + (1-b_t)a_t h_{it} + b_t\tilde{\theta}_{it}$$

and

$$\rho_{\theta_i|h_{it},\tilde{\theta}_{i1}} = \rho_{\theta_i|h_{it}} + \rho_\eta = \rho_{\theta_i} + \rho_{\epsilon_t} + \rho_\eta,$$

where

$$a_t = \frac{\rho_{\epsilon_t}}{\rho_\theta+\rho_{\epsilon_t}} \text{ and } b_t = \frac{\rho_\eta}{\rho_\theta+\rho_{\epsilon_t}+\rho_\eta}.$$

Denote $F_{h_i}(h_t^*) = \omega_t$. Pooling across types, by LIE, the officer's estimation of the household's true income when she observes housing conditions $h_i > h_t^*$ is given by:

$$\begin{aligned}
E(\theta_i|h_{it} > h_t^*,\tilde{\theta}_{it}) &= E(\theta_i|\theta_i > \bar{\theta}, h_{it} > h_t^*,\tilde{\theta}_{it})\left(1-F_{\theta_i}(\bar{\theta})\right) + \underbrace{E(\theta_i|\theta_i \le \bar{\theta}, h_{it} > h_t^*,\tilde{\theta}_{it})}_{0} F_{\theta_i}(\bar{\theta}) \\
&= k_t\left[(1-b_t)(1-a_t)\mu_\theta + (1-b_t)a_t h_{it} + b_t\tilde{\theta}_{it}\right]\left(1-F_{\theta_i}(\bar{\theta})\right) \\
&= \frac{1-F_{h_{it}}(h_t^*)}{1-F_{\theta_i}(\bar{\theta})}\left[(1-b_t)(1-a_t)\mu_\theta + (1-b_t)a_t h_{it} + b_t\tilde{\theta}_{it}\right]\left(1-F_{\theta_i}(\bar{\theta})\right) \\
&= \left[(1-b_t)(1-a_t)\mu_\theta + (1-b_t)a_t h_{it} + b_t\tilde{\theta}_{it}\right](1-\omega_t)
\end{aligned} \tag{17}$$

Note here, $E(\theta_i|\theta_i \le \bar{\theta}, h_{it} > h_t^*,\tilde{\theta}_{it}) = 0$ because the case of $[-\infty,\bar{\theta}] \cap [h_t^*,+\infty]$ does not exist in the data generation process.

If the officer observes $h_{it} \leq h_t^*$, she can infer the true income of such households is no more than $\theta_{h_t^*}$. Additionally, she expects some of these households – types in the range $[\bar{\theta}, \theta_{h_t^*}]$ (who will surely have $h_{it} \leq h_t^*$) – will bunch. For such households, the density of message takes the same form regardless of their actual income: $f(\tilde{\theta}_{it}|\theta_i, h_{it}) = f(\tilde{\theta}_{it}|h_{it}) = \phi(\frac{\tilde{\theta}_{it}-\bar{\theta}}{\sigma_\eta})$, thus her updating for them is:

$$
\begin{aligned}
f(\theta_i|\theta_i, h_{it}, \tilde{\theta}_{it})\mathbb{I}\{\theta_i \in [\bar{\theta}, \theta_{h_t^*}]\} &= \frac{f(\tilde{\theta}_{it}|\theta_i, h_{it})f(\theta_i|h_{it})}{f(\tilde{\theta}_{it}|h_{it})}\mathbb{I}\{\theta_i \in [\bar{\theta}, \theta_{h_t^*}]\} \\
&= f(\theta_i|h_{it})\mathbb{I}\{\theta_i \in [\bar{\theta}, \theta_{h_t^*}]\}
\end{aligned}
$$

This result reflects the equilibrium behavior of bunching households: regardless of their true income, as long as its is in the bunching range, they will shade it to the cutoff level $\bar{\theta}$. Therefore, their message is completely unreliable to the officer. Like before, the densities in this formula needs appropriate truncation. In particular, the prior on types $f(\theta_{it})$ is truncated to the range $[\bar{\theta}, \theta_{h_t^*}]$, and $f(h_{it})$ is truncated above at h_t^* to reflect to bounds of the information the officer expects from these types. Then applying a similar updating process as in **??**, we have:

$$
f(\theta_i|\theta_i, h_{it}, \tilde{\theta}_{it})\mathbb{I}\{\theta_i \in [\bar{\theta}, \theta_{h_t^*}]\} = k_t^{'}\phi\left(\frac{\theta_i - \mu_{\theta_i|h_{it}}}{\sigma_{\theta_i|h_{it}}}\right)\mathbb{I}\{\theta_i \in [\bar{\theta}, \theta_{h_t^*}]\}
$$

where $k_t^{'} = \frac{F_{h_{it}}(h_t^*)}{F_{\theta_i}(\theta_{h_t^*}) - F_{\theta_i}(\bar{\theta})}$. Notice that $k_t^{'}$ encompasses the truncation bounds mentioned above.

For other types with $h_{it} \leq h_t^*$, those with true income below the official cutoff $\theta_i < \bar{\theta}$, still report the truth.[56] Applying the appropriate truncation bounds, her updating process for such types will be:

$$
\begin{aligned}
f(\theta_i|h_{it}, \tilde{\theta}_{it})\mathbb{I}\{\theta_i < \bar{\theta}\} &= \frac{f(\tilde{\theta}_{it}|\theta_i, h_{it})f(\theta_i|h_{it})}{f(\tilde{\theta}_{it}|h_{it})}\mathbb{I}\{\theta_i < \bar{\theta}\} \\
&= k_t^{''}\phi\left(\frac{\theta_i - \mu_{\theta_i|(h_{it},\tilde{\theta}_{it})}}{\sigma_{\theta_i|(h_{it},\tilde{\theta}_{it})}}\right)\mathbb{I}\{\theta_i < \bar{\theta}\}
\end{aligned}
$$

where $k_t^{''} = \frac{F_{h_{it}}(h_t^*)}{F_{\theta_i}(\bar{\theta})}$. To summarize, when the officer observes $h_{it} \leq h_t^*$, her posterior differs for

[56]This implies that for these households, the unconditional density of message takes the form: $f(\tilde{\theta}_{it}|\theta_i, h_{it}) = \phi\left(\frac{\tilde{\theta}_{it}-\theta_i}{\sigma_\eta}\right)$, while the density of message conditioning on type takes the form: $f(\tilde{\theta}_{it}|h_{it}) = \phi\left(\frac{\tilde{\theta}_{it}-\mu_\theta}{\sqrt{\sigma_\theta^2+\sigma_\eta^2}}\right)$. These functional forms are the same as in the case for households with $h_{it} > h_t^*$, who also tell the truth.

different ranges of types:

$$f(\theta_i|h_{it},\tilde{\theta}_{it})\mathbb{I}\{h_{it}\leq h_t^*\} = \begin{cases} = k_t^{'}\phi\left(\frac{\theta_i-\mu_{\theta_i|h_{it}}}{\sigma_{\theta_i|h_{it}}}\right)\mathbb{I}\{\theta_i\in[\bar{\theta},\theta_{h_t^*}]\} \\ = k_t^{''}\phi\left(\frac{\theta_i-\mu_{\theta_i|(h_{it},\tilde{\theta}_{it})}}{\sigma_{\theta_i|(h_{it},\tilde{\theta}_{it})}}\right)\mathbb{I}\{\theta_i<\bar{\theta}\} \end{cases} \tag{18}$$

Pooling across types, by LIE, the officer's estimation of the household's true income when she observes housing conditions $h_i \leq h_t^*$ is given by:

$$\begin{aligned} E(\theta_i|h_{it}\leq h_t^*,\tilde{\theta}_{it}) &= E(\theta_i|\theta_i\leq\bar{\theta},h_{it}\leq h_t^*,\tilde{\theta}_{it})F_{\theta_i}(\bar{\theta}) \\ &+ E(\theta_i|\bar{\theta}<\theta_i\leq\theta_{h_t^*},h_{it}\leq h_t^*,\tilde{\theta}_{it})\left(F_{\theta_i}(\theta_{h_t^*})-F_{\theta_i}(\bar{\theta})\right) \\ &+ \underbrace{E(\theta_i|\theta_i>\theta_{h_t^*},h_{it}\leq h_t^*,\tilde{\theta}_{it})}_{0}\left(1-F_{\theta_i}(\theta_{h_t^*})\right) \\ &= k_t^{''}\left[(1-b_t)(1-a_t)\mu_\theta+(1-b_t)a_th_{it}+b_t\tilde{\theta}_{it}\right]F_{\theta_i}(\bar{\theta}) \\ &+ k_t^{'}\left[(1-a_t)\mu_\theta+a_th_{it}\right]\left(F_{\theta_i}(\theta_{h_t^*})-F_{\theta_i}(\bar{\theta})\right) \end{aligned}$$

In this computation, $E(\theta_i|\theta_i > \theta_{h_t^*}, h_{it} \leq h_t^*, \tilde{\theta}_{it}) = 0$ because the case of $[\theta_{h_t^*}, +\infty] \cap [-\infty, h_t^*]$ again does not exist. The remaining two terms relate to the two range of true income associated with low housing conditions – the bunching range and the truth telling range below $\bar{\theta}$. The expectation on each of them are integrated over the corresponding density function summarized in Equation 18.

Substitute in the expressions for $k_t^{''}, k_t^{'}$, and denote $\omega_t = F_{h_{it}}(h_t^*)$, then simplify the above, we have:

$$E(\theta_i|h_{it}\leq h_t^*,\tilde{\theta}_{it}) = \left[(2-b_t)(1-a_t)\mu_\theta+(2-b_t)a_th_{it}+b_t\tilde{\theta}_{it}\right]\omega_t \tag{19}$$

Pooling cross the two housing segments, we have:

$$\begin{aligned}
E(\theta_i|h_{it},\tilde{\theta}_{it}) &= E(\theta_i|h_{it}\le h_t^*,\tilde{\theta}_{it})\omega_t + E(\theta_i|h_{it}>h_t^*,\tilde{\theta}_{it})(1-\omega_t)\\
&= \underbrace{\left[(1-b_t)(1-a_t)(1-2\omega_t+2\omega_t^2)+(1-a_t)\omega_t^2\right]}_{A_t}\mu_\theta\\
&+ \underbrace{\left[(1-b_t)a_t(1-2\omega_t+2\omega_t^2)+a_t\omega_t^2\right]}_{B_t}h_{it}\\
&+ \underbrace{\left[b_t(1-2\omega_t+2\omega_t^2)\right]}_{C_t}\tilde{\theta}_{it}
\end{aligned} \tag{20}$$

Tracing out the effects of learning over time Let $\iota_t = 1-2\omega_t+2\omega_t^2$. Tracing out the difference in the coefficient on housing condition, we will see it increases if:

$$B_t > B_{t-1}$$

$$\Leftrightarrow \underbrace{\frac{\rho_{\epsilon_t}}{\rho_{\theta_{i,t-1}}+\rho_{\epsilon_t}+\rho_\eta}\iota_t + \frac{\rho_{\epsilon_t}}{\rho_{\theta_{i,t-1}}+\rho_{\epsilon_t}}\omega_t^2}_{\longrightarrow 1 \text{ if } \rho_{\epsilon_t} \text{ large}} > \underbrace{\frac{\rho_{\epsilon_{t-1}}}{\rho_{\theta_{i,t-2}}+\rho_{\epsilon_{t-1}}+\rho_\eta}\iota_{t-1} + \frac{\rho_{\epsilon_{t-1}}}{\rho_{\theta_{i,t-2}}+\rho_{\epsilon_{t-1}}}\omega_{t-1}^2}_{<1 \text{ if } \rho_{\epsilon_{t-1}} \text{ and } \omega_{t-1} \text{ small}}$$

Intuitively, there are two opposing effects on B_t as the housing signal gets more precise (ρ_{ϵ_t} grows larger than $\rho_{\epsilon_{t-1}}$). If ρ_{ϵ_t} grows large enough, it dominates the officer's current stock of knowledge, thus brings B_t closer to one.[57] However, the term $\frac{\rho_{\epsilon_t}}{\rho_{\theta_{i,t-1}}+\rho_{\epsilon_t}}\omega_t$ in this coefficient is present due bunching. As the housing signal gets more precise, it drive down bunching, thereby shrink thinks term to zero. Therefore, if the positive effect of more precise housing signal is sufficiently larger the negative effect from bunching reduction, then B_t may be larger than B_{t-1}.

Similarly, we can also trace out the difference in the coefficient on reported income over time:

$$C_t < C_{t-1}$$

$$\Longleftrightarrow \frac{\rho_\eta}{\rho_{\theta_{i,t-1}}+\rho_{\epsilon_t}+\rho_\eta}\iota_t < \frac{\rho_\eta}{\rho_{\theta_{i,t-2}}+\rho_{\epsilon_{t-1}}+\rho_\eta}\iota_{t-1}$$

Again, there are also two opposing effects on C_t as ρ_{ϵ_t} grows larger than $\rho_{\epsilon_{t-1}}$. Larger ρ_{ϵ_t} in the denominator, bringing down this coefficient. However, growing ρ_{ϵ_t} also raises ι_t the numerator.[58] If the former sufficiently dominates the latter, the coefficient on reported income will fall over time.

[57] Larger ρ_{ϵ_t} raises $\frac{\rho_{\epsilon_t}}{\rho_{\theta_{i,t-1}}+\rho_{\epsilon_t}+\rho_\eta}$ and $\frac{\rho_{\epsilon_t}}{\rho_{\theta_{i,t-1}}+\rho_{\epsilon_t}}$. Additionally, it reduces $\omega_t = F(h_t^*)$, thus most likely increases ι_t; this is very plausible because for $\omega_t < 0.5$, $\frac{\partial \iota_t}{\partial \omega_t} < 0$.

[58] See footnote 7.

$\square$

F Model Extension

G Model extension: allowing for economic growth

The model in Section 5 assumes the true income of households is time-invariant in order to highlight the effect of learning over time. This, of course, is untrue in reality. Between 2002 and 2020, GDP per capita increased by 3.6 fold in Vietnam (World Bank, 2022). Dramatic economic growth could well explain the observed "on-then-off" bunching pattern, because many households could have moved out of poverty and no longer have the incentive to bunch. In this extension, I show that economic growth alone may yield similar predictions to the main model with learning mechanism, but the magnitude of the effects over time are likely to be much more modest compared to those predicted my the learning model. This analysis thus provides additional theoretical base for officer's learning as a critical mechanism to improve targeting performance over time.

I model growth with a simple mean shift in the distribution of true log income over time: $\theta_{it} = \theta_{i,t-1} + g_t$, where θ_{it} represent the true log income in period t, and $g_t > 0$ is a positive constant that approximately equals the growth rate.[59] The current period's true log income is normally distributed with a deterministic drift $\theta_{it} \sim \mathcal{N}(\mu_{\theta_{t-1}} + g_t, \sigma_\theta)$. Note $f(\theta_0)$ represents the prior distribution of true log income at the beginning of a phase (equivalent to $f(\theta)$ in subsection 5.1). Additionally, the mean of this distribution shifts rightward over time, but its variance remains time invariant.

An increase in true income prompts a similar rightward shift in the housing signal: $h_{it} = \theta_{it} + \epsilon_{it} = g_t + \theta_{i,t-1} + \epsilon_{it}$. Here ϵ_{it} is still the driver of the officer's mapping between true income and housing conditions. The message sent by households also changes accordingly if they tell the truth $\tilde{\theta}_{it} = \theta_{it} + \eta_{it} = g_t + \theta_{i,t-1} + \eta_{it}$, but will remain $\tilde{\theta}_{it} = \bar{\theta} + \eta_{it}$ if they bunch at the official cutoff.

The information structure remains operationally the same. The only change is that at the beginning of period t, the officer is aware that her belief about the household's true (log) income (up to $t-1$) should be adjusted upwards by a constant g_t, so her prior is $f(\theta_{it}|I_{i,t-1}, t) = f(\theta_{i,t-1} + g_t|I_{i,t-1}, t)$. After receiving information in the current period, her posterior is $f(\theta_{it}|I_{i,t-1}, t, h_{it}, \tilde{\theta}_{it})$. The housing signal in t, being independent from everything else, still has the potential to drive the officer decision if it is highly accurate.

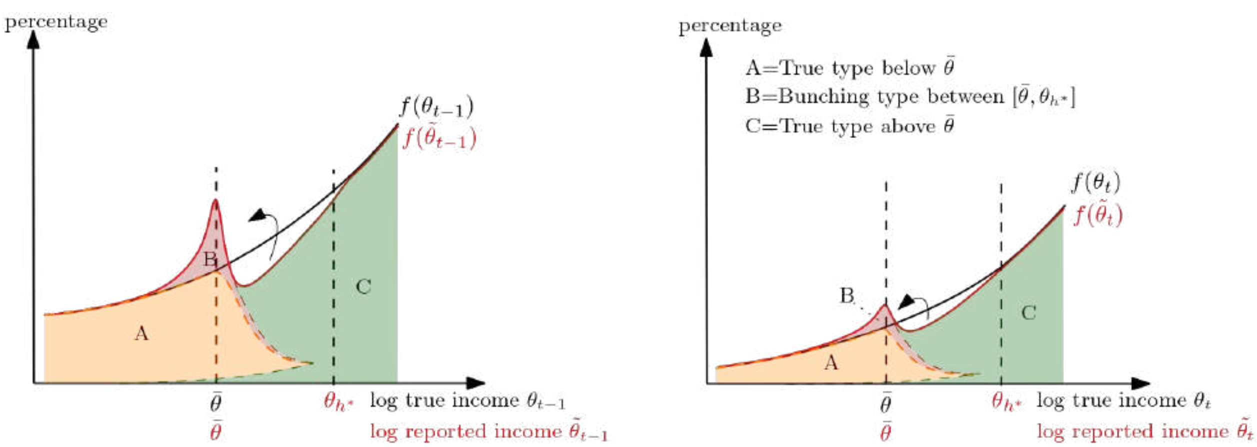
[59] This formulation of growth implies that economic growth imbues inequality, because, at the same growth rate, the income increase earned by an initially high-income earner is larger than the same amount earned by an initially low-income earner. This phenomenon is typical in most countries, yet the extent of rise in inequality in Vietnam since 1991 has been fairly small compared to other countries (World Bank, 2016).

Economic growth without learning To distinguish the two competing theories that can explain the observed temporal pattern of bunching, I now discuss the implications of the model if we allow for economic growth but turn off the learning channel.

Because the official income cutoff remains the same within a phase and the housing-income mapping retains the same precision over time, there is no change in the officer's inference about the "acceptable" housing conditions for a household with a true income right at the cutoff. In other words, since the distribution of ϵ_{it} has time-invariant parameters, the low housing threshold $h^* = \bar{\theta} + \epsilon^*$ stays the same over time.

Figure 5 illustrates the temporal change in households' bunching behavior under this premise. Compare to period $t-1$, the rightward mean shift in θ_{it} results in a smaller fraction of households that would be susceptible to bunching. As a result, the bunching mass in red area (mass B) in t (left panel) is smaller than in $t-1$ (right panel). One can also see this by inspecting the probability of bunching, defined as $\zeta_t = \int_{\bar{\theta}}^{\theta_{h^*}} \int_{-\infty}^{h^*} f(\theta_{it}, h_{it}) dh_{it} d\theta_{it} = \int_{\bar{\theta}}^{\theta_{h^*}} \int_{-\infty}^{h^*} f(h_{it}|\theta_{it}) f(\theta_{it}) dh_{it} d\theta_{it}$. While $f(h_{it}|\theta_{it})$ is unchanged because the signal is not improving, $f(\theta_{it})$ decreases (for lower range of income), thus the mass of bunching becomes smaller over time in the presence of income growth.

Figure 5: Bunching equilibrium with growth but no learning



(a) Period $t-1$

(b) Period t

With regards to the officer's decision, we derive a similar estimation of household's true income as in subsection 5.4. The information structure and Bayesian updating process remain operationally the same as in the case with learning. However, the prior mean in this case will be $\mu_{\theta_{i,t-1}} + g_t$, thus it can be shown that the officer's estimate of the household true income in the current period is given by:

$$E(\theta_i|I_{i,t-1},t,h_{it},\tilde{\theta}_{it}) = \underbrace{\left[(1-b_t)(1-a_t)(1-2\omega_t+2\omega_t^2)+(1-a_t)\omega_t^2\right]}_{A_t}(\mu_{\theta_{i,t-1}}+g_t)$$
$$+\underbrace{\left[(1-b_t)a_t(1-2\omega_t+2\omega_t^2)+a_t\omega_t^2\right]}_{B_t}h_{it}$$
$$+\underbrace{\left[b_t(1-2\omega_t+2\omega_t^2)\right]}_{C_t}\tilde{\theta}_{it}$$

where $a_t = \frac{\rho_\epsilon}{\rho_{\theta_{it}}+\rho_\epsilon}$ and $b_t = \frac{\rho_\eta}{\rho_{\theta_{it}}+\rho_\epsilon+\rho_\eta}$ and $\omega_t = F_{h_{it}}(h^*)$.

We can again compare the coefficient on housing conditions over time:

$$B_t < B_{t-1}$$
$$\iff \frac{\rho_\epsilon}{\rho_{\theta_{i,t-1}}+\rho_\epsilon+\rho_\eta}\iota_t + \frac{\rho_\epsilon}{\rho_{\theta_{i,t-1}}+\rho_\epsilon}\omega_t^2 < \frac{\rho_\epsilon}{\rho_{\theta_{i,t-2}}+\rho_\epsilon+\rho_\eta}\iota_{t-1} + \frac{\rho_\epsilon}{\rho_{\theta_{i,t-2}}+\rho_\epsilon}\omega_{t-1}^2.$$

where $\iota_t = (1-2\omega_t+2\omega_t^2)$. With the distribution of h_{it} shifting rightward over time and h^* is unchanged, $\omega_t < \omega_{t-1}$. The same signal precision ρ_ϵ implies that the fractions on the left hand side are strictly smaller than the fractions on the right hand side. However, $\iota_t > \iota_{t-1}$ as $\omega_t < \omega_{t-1}$ (for $\omega_{t-1} < 0.5$). Therefore, it is plausible that the coefficient on housing conditions B_t may still rise over time in the absence of learning. The increase of B_t over time in this scenario, however, is likely to be more much smaller than in the case with learning. This is because ι_t is the only component that could raise B_t, while with learning effect, rising ρ_ϵ over time add an addition channel through which B_t can rise over time. If the ω_t falls by the same amount under both scenarios, then B_t will increase by a greater extent with learning than without this process.

In summary, without the learning channel, economic growth alone can yield the similar predictions as before, but the magnitude of the effects over time are likely to be much more modest.

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