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MISTAKE #37: THE EFFECT OF PREVIOUSLY ENCOUNTERED PRICES ON CURRENT HOUSING DEMAND*

Uri Simonsohn and George Loewenstein

Based on contrast effects studies from psychology, we predicted that movers arriving from more expensive cities would rent pricier apartments than those arriving from cheaper cities. We also predicted that as people stayed in their new city they would get used to the new prices and would readjust their housing expenditures countering the initial impact of previous prices. We found support for both predictions in a sample of 928 movers from the PSID. Alternative explanations based on unobserved wealth and taste, and on imperfect information are ruled out.

> 'Common Mistake #37: When moving from a high-cost area to a low-cost area, recalibrate your sights...... put the home prices of Boston and San Francisco out of your mind.' Eldred (2002, p. 89).

The median rent for a two-bedroom apartment in Pittsburgh Pennsylvania is $654. Is that cheap or expensive? The answer seems to depend on what we compare those 654 dollars to. For a mover from San Francisco, Pittsburgh may very well feel like a bargain since the typical two-bedroom in San Francisco rents for $2,124. For a mover from Gadsden, Alabama, on the other hand, Pittsburgh may seem like a rip-off; a typical two-bedroom there rents for just $433.

Does the subjective perception of nominal prices as cheap or expensive influence how much a household decides to spend? In line with *common mistake #37* from the opening quotation, and consistent with previous research in psychology, we posit that it does. We hypothesise that, because households are likely to experience some uncertainty about how much they want to spend in housing, they are prone to draw on salient, but in some cases not normatively defensible, cues when making a decision about how much to spend. Specifically, we expect households moving from more expensive cities to spend more in their destination city, holding other factors constant, because their previous exposure to high prices makes prices in the new city seem cheaper. We would expect, for example, that a household moving from Alabama to Pittsburgh would spend less on housing than an otherwise identical household moving to Pittsburgh from San Francisco.

Housing provides an ideal domain in which to test for the impact of uninformative cues, in part because of its economic importance, housing is the single

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largest item in most consumers' budgets. Yet, despite its importance to individuals and the aggregate economy, housing is a domain in which there is scope for uninformative cues to influence behaviour, because people are highly uncertain about their own housing preferences and because most people have limited opportunities to learn from experience. Indeed studies have already observed anomalous patterns in housing behaviour that have been explained in psychological terms (Genesove and Mayer, 2001; Case and Shiller, 1988).

Using data from the Panel Study of Income Dynamics (PSID), we estimate standard housing demand equations for movers between cities, modifying the equations to take account of rental prices prevailing in the city of origin. We predict, and find, that migrants coming from more expensive cities spend more in rent in their new city than those coming from cheaper cities.

Obviously, this could be caused by factors other than uncertainty about preferences for spending on housing. For example, people coming from cities with expensive housing could be wealthier, have higher taste for housing, or might systematically misestimate local housing costs. We report a series of analyses that test these alternative explanations and conclude that they cannot account for the observed patterns.

In addition, we find that when people move again within their new city, they readjust housing expenditure in a way that offsets the initial impact of previous prices, an effect not predicted by many of the alternative accounts of the phenomenon.[1] In other words, movers coming from more expensive cities initially rent more expensive units, but subsequently move to lower rent units, while movers from cheaper cities shift in the opposite direction, suggesting that they eventually forget about prices in the city they came from and become influenced, instead, by market prices in the location they move to.

In what follows, Section 1 reviews the relevant literature, Section 2 presents the statistical analysis, Section 3 presents tests of alternative explanations, Section 4 discusses possible psychological mechanisms underlying our findings and Section 5 concludes.

1. Background

Several lines of research support the notion that people are often uncertain about their own preferences. Loomes (1988), for example, argues that, contrary to the standard assumption of clearly defined indifference curves, people have *regions* of indifference; see also Butler and Loomes (1988). If so, there is no longer a unique maximising consumption bundle for any given budget, but rather a set of bundles that a consumer would 'settle' for.

When people are uncertain about their own preferences, they will naturally attempt to resolve this uncertainty. A wide range of research on what has come to be known as 'constructed preferences' (Slovic, 1995; Payne *et al.*, 1999) shows that, when people are uncertain of their own preferences, but are nevertheless called upon to make decisions, they draw upon a wide range of cues to help them resolve

[1] This effect is significant at the 10% level.

their uncertainty and arrive at a decision, including, in some cases, cues that are difficult to justify normatively.

In *anchoring* studies, for example, it has been shown that people's valuations of goods can be affected by a randomly selected number – the 'anchor'. In a recent paper by Ariely *et al.* (2003), for example, subjects were first anchored by asking them whether they valued different goods more or less than a price created from the last two digits of their social-security-number. Then, actual maximum buying prices were elicited through an incentive compatible mechanism. Subjects with above-median social security numbers stated values that were from 57% to 107% higher than those given by subjects with below-median social security numbers.

In *context effects* studies, individual preferences are affected by the larger choice set within which alternatives are offered (McFadden, 1999). For example, adding an extreme alternative to a choice set can make other extreme, but not *as* extreme, alternatives appear to be 'compromise' options, which can increase their market share, contrary to the principle of independence of irrelevant alternatives (Huber *et al.*, 1982).

A context effect that is particularly relevant to this article is the *background contrast* effect, which occurs when previously faced options affect current decisions, even when those options are no longer available. Simonson and Tversky (1992) first documented this effect in an experiment in which subjects made two choices in sequence; the first of which constituted the experimental treatment. The treatment consisted of having half the subjects choose between two options that had a high implicit relative cost for an attribute, and the other half making a choice with an implicit low relative cost for the same attribute. The second choice was the same for all subjects. Consistent with the authors' hypothesis, subjects who were in the high cost treatment were more likely to choose the expensive option in the second choice, presumably because it *appeared* cheaper to them. For example, in one study subjects made hypothetical choices between two computers that differed in price and memory. In the 'expensive memory' condition, the price of memory was $2 per Kb, while the 'cheap memory' condition the price of memory was $0.5 per Kb. In the second stage all subjects made the same hypothetical choice between two computers that differed in memory and price with an implied cost of memory of $1 per Kb. 52% of subjects exposed to the $2/Kb background chose the computer in the target set with more memory, while only 18% of those in the $0.5/Kb background did so.

Background contrast effects have even been found in non-human animals, suggesting that they are deeply rooted in our brains. Waite (2001), for example, designed a study similar to that of Simonson and Tversky, using *grey jays* as subjects. In this experiment birds chose between obtaining three raisins with high effort or just one raisin with low effort. The experiment consisted of two stages; in the first stage, for half the birds, three raisins required much more effort than one raisin, while for the other half they required the same amount of effort. In the second stage, both groups faced the same effort/raisins trade-offs. Consistent with background contrast, birds who had previously needed to put in more effort to obtain the three raisins worked harder for raisins in the second stage, as if the effort required was perceived differently depending on the tradeoff they had faced previously.

For decision-makers' reliance on external cues to be important for economics, however, it must be documented not only in controlled experiments with small stakes, but also in real economic decisions, with the potential corrective mechanism of market forces, large enough stakes to guarantee the agents' motivation, and with opportunities for learning. That is what this article sets out to do. It is the first study we are aware of that tests for contrast effects in the field.

The parallel between background contrast experiments and our study is straightforward. In the 'first stage' individuals live in a city and observe housing prices, and in the second they make housing decision in a new city where previous prices are no longer relevant. Our first prediction is that people will behave with respect to housing decisions similarly to how they behave in contrast effect experiments. This implies that,

Prediction 1. Movers coming from more expensive cities will spend more on housing in a destination city than movers moving to the same city from cheaper cities.

A second prediction arises from the fact that, as people stay in their new city, the contrast with their previous city should have a diminishing impact on their perception of what is a reasonable amount to spend in housing, whereas prices in the market they move to should exert an increasing influence. Hence,

Prediction 2. Movers who stay in the new city will gradually readjust the amount spent on housing countering the initial impact of previous observed prices.

The logic behind Prediction 2 is that if movers are spending differently in their new city because of a contrast between new and old prices, once they get accustomed to the new prices they should become dissatisfied with their original choice and should wish to revise their housing expenditures accordingly.

Prediction 2 is particularly useful for ruling out alternative explanations for the empirical support of Prediction 1. A variety of alternative explanations involve the idea that movers leaving expensive cities differ from those leaving cheaper ones in more than the prices they faced in the past. They may, for instance, have greater wealth or differ in their taste for housing. Although such propositions are discussed in detail in Section 3, it is worth noting that *stable* differences between people moving from different cities would not lead to the pattern described by Prediction 2. For example, if people who move from more expensive cities rent more expensive apartments because they are richer, we would not expect them to systematically revise their housing consumption downwards as they remain in their new city.

2. Empirical Analyses

2.1. *The Data*

We use data from the Panel Study of Income Dynamics (PSID) between 1983 and 1993 inclusive. In 1988 and 1989, however, PSID did not collect information on amount of rent paid by survey respondents, so these years are excluded from the analysis.[2] In what follows, we standardise time periods according to the time of a

[2] Some of the data used in the analysis are derived from the Sensitive Data Files of the PSID, obtained under special contractual arrangements designed to protect the anonymity of respondents. These data are not available from the author. Persons interested in obtaining PSID Sensitive Data Files should contact PSIDHelp@isr.umich.edu

move; we use $t-1$ to refer to the year prior to a move, t for the year of the move, and $t+1$ for the year following the move. We represent the amount of money household i spends in rent in period k as $r_{i,k}$ and the median rent in the city where household i lives in period k as $\bar{r}_{i,k}$.

Observations included meet all of the following conditions:

- The household head moved from one Metropolitan Statistical Area (MSA) to a different MSA (this effectively excludes moves within a city and people moving from or to rural areas).
- Reported household income is at least \$10,000.
- The household head is 22 years old or older.
- Reported values for rent are plausible.[3]

These criteria led to a dataset with 928 observations: 650 renters and 278 buyers. Table 1 summarises the demographic characteristics of the sample as well as some key variables used in the analyses.[4]

Testing the prediction that housing costs in the origin-city will influence subsequent decisions requires a measure of the cost of housing in each city. It is not feasible to obtain these costs from the PSID because most cities have fewer than three observations. Instead, we use the median city-level prices from the 1990 Census. To estimate median rents for all other years, we adjust the 1990 Census figures by a deflator based on the HUD's Fair Market Rent index, which is available since 1983 for each of the cities in the sample.

The moves represented in the sample are geographically dispersed. There are 173 *cities of origin* and 180 different *destination cities* in the sample. The most common destination city accounted for only 41 moves (less than 5% of the sample), and these 41 movers came from 26 different origin cities. Indeed, out of the 928 moves, 617 occur between a unique city pair (i.e. only one household moved from and to those two specific cities). Such dispersion makes it unlikely that our findings are the result of idiosyncratic characteristics of people who move between one specific city and another – e.g., a 'New York to Pittsburgh effect'.

For costs of housing across cities to have measurable effects of the type we hypothesise, they must differ by sufficient magnitude. Figure 1 presents a cumulative distribution of the difference between $\bar{r}_{i,t}$ and $\bar{r}_{i,t-1}$ (median rents in the destination and origin cities, respectively). It shows that changes in cost of housing are both symmetric (i.e. a similar proportion of people are moving from expensive to cheap as are moving from cheap to expensive) and substantial. The average absolute value of the difference between $\bar{r}_{i,t}$ and $\bar{r}_{i,t-1}$ is \$130 or 20% of $\bar{r}_{i,t}$. If renters use previous prices as a point of reference, the typical mover will exhibit a disparity between the reference price and the actual market price of \$130.[5]

The PSID asks the respondent from every household that moves, their reason for having done so. Table 2 reports the distribution of answers to this question. Column 1 shows the distribution for movers between cities (i.e. those used in our

[3] Rents reported below \$80/month were excluded.

[4] All dollar amounts have been adjusted by inflation as measured by mid-year CPI index: all monetary amounts refer to US\$ of June 2000.

[5] i.e. $\sum_i |\bar{r}_{i,t} - \bar{r}_{i,t-1}|/N_{renters} = \$130$.

Table 1

Descriptive Statistics for Renters in period $t - 1$

	Age	Head's education	Household income	Rent per month	Number of children	Number of adults	Median rent city level	Distance between cities (miles)
Mean	33.05	14.02	$43,759	$567	0.72	1.55	$634	542
Std Dev	(10.57)	(2.42)	($34,168)	($285)	(1.05)	(0.50)	($136)	(609)

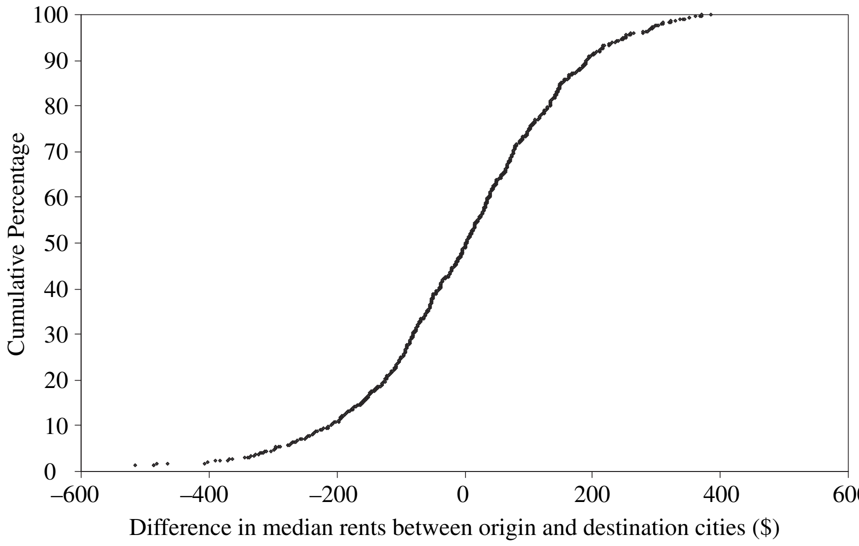

Fig. 1. *Distributions of Differences in City-level Median Cost of Housing Between Origin and Destination City,* weighted by observations in the sample

Table 2

Comparison of Reasons for Moving Between-cities vs Within-cities

	(1)	(2)
Reason for moving	Between different cities (%)	Within same city (%)
Job related	36.8	4.2
Live closer to work	5.4	4.1
Housing related (increase)	5.9	24.6
Housing related (decrease)	4.7	10.6
Become owner/got married	6.6	15.2
Neighbourhood/closer to family or friends	12.9	7.0
Outside events (evicted, divorce, job transfer)	14.5	17.9
Mixed reasons	9.3	13.0
Don't know/Refuse to answer	3.8	3.5
Total	100.0	100.0

analysis) and column 2 reports the distribution of answers given by households who moved within a city. The ideal sample would consist of households who are randomly picked up from one city and dropped into another but this kind of exogenous variation is rare. It is a good sign, nevertheless, that movers from our sample are not likely to indicate they moved for a housing related reason (just over 10% of them). As we report later, dropping movers who gave a housing related cause for their move from our analyses barely affects the results.

The disparity in reasons for moving between those relocating to a new city and those staying in the same city suggests that the process underlying the two groups' decisions is different. For this reason we conduct our analysis only on movers between cities, excluding people who did not move or who moved within a city. Conducting the analysis on the whole sample, however, does not affect the qualitative nature of the results.

2.2. *Testing Prediction 1*

To test Prediction 1 we estimate standard housing demand regressions for renters, adding the key variable of interest, $\bar{r}_{i,t-1}$, as an additional explanatory variable. The estimates of the housing demand regression are presented in Table 3. The first column excludes the main variable of interest, $\log(\bar{r}_{i,t-1})$, while the second column includes it. Column 3 adds the inverse of the Mills ratio from a first stage probit regression of the renting/owning choice on a variety of explanatory variables to correct for the selection bias that arises from the fact that people endogenously choose whether to be a renter or an owner (Heckman, 1979). This is a common procedure in the housing literature; see Rosen (1974), Henderson and Ioannides (1986), Rosenthal *et al.* (1991) and Rapaport (1997) among others. The results from the first stage regression are reported in the Appendix (Table A1).

Column 4 adds yearly fixed effect. Columns 5 and 6 attempt to control for unobserved heterogeneity across individuals by using information from previous housing choices: column 5 adds the individual's rent divided by the city-level median rent from the previous period ($r_{i,t-1}/\bar{r}_{i,t-1}$) while column 6 uses the residual from a housing demand regression from period $t-1$. Finally, column 7 excludes households who report having moved for a housing related issue. Columns 5 to 7 will be discussed in Section 3, in connection with the analysis of alternative explanations.

The parameter estimates reported in Table 3 are within the range of previous studies and in the directions that economic intuition would propose. Income, family size and education have a positive impact on housing expenditure.[6] The estimated impact of current prices (Median rent destination city) is estimated at around 0.5. Note that because this estimation is in logs, price elasticity – defined over a 'standardised unit of housing' – is simply ($\beta - 1$).

[6] We estimated regressions using both current and permanent income as controls, using as a proxy for permanent income a four-year average of yearly income (current income + next year's income + previous two years' income divided by 4). The choice between the two had no impact on the parameters of interest, so we report the results using the simpler, current income, variable.

Table 3

Housing Demand Estimates for Renters

Dependent variable: log (dollar amount of monthly rent in *t*)

	(1) Baseline	(2) Adds costs in previous city	(3) Adds selection adjustment	(4) Adds fixed effects	(5) Adds relative expenditure ($t-1$)	(6) Adds $e(t-1)$	(7) Excludes housing motivated moves
Constant	−0.631	−1.621	−1.376	−1.466	−1.260	−0.757	−1.853
	(0.606)	(0.697)	(0.705)	(0.712)	(0.908)	(1.223)	(0.785)
log(income)	0.284	0.284	0.252	0.254	0.248	0.232	0.294
	(0.029)	(0.029)	(0.030)	(0.030)	(0.045)	(0.074)	(0.039)
Number of children in household	0.044	0.045	0.040	0.040	0.053	0.062	0.056
	(0.017)	(0.017)	(0.017)	(0.018)	(0.020)	(0.021)	(0.018)
Number of adults in household	0.145	0.146	0.125	0.126	0.139	0.149	0.123
	(0.044)	(0.044)	(0.044)	(0.043)	(0.055)	(0.054)	(0.048)
Age of head of household	0.006	0.004	−0.001	0.001	0.006	0.003	0.000
	(0.007)	(0.007)	(0.007)	(0.007)	(0.011)	(0.010)	(0.009)
(Age squared)/100	−0.003	−0.002	0.001	−0.001	−0.680	−0.003	0.000
	(0.007)	(0.007)	(0.007)	(0.007)	(0.011)	(0.010)	(0.000)
Attended college (1 or 0)	0.131	0.132	0.116	0.119	0.108	0.137	0.117
	(0.036)	(0.036)	(0.036)	(0.036)	(0.042)	(0.041)	(0.039)
Head of household is female (1 or 0)	0.026	0.021	0.036	0.034	0.093	0.111	0.053
	(0.048)	(0.047)	(0.048)	(0.049)	(0.053)	(0.062)	(0.051)
log(median rent destination city)	0.536	0.494	0.527	0.537	0.421	0.427	0.550
	(0.083)	(0.087)	(0.085)	(0.085)	(0.097)	(0.103)	(0.093)
log(median rent origin city)	–	0.203	0.197	0.192	0.286	0.209	0.182
	–	(0.079)	(0.079)	(0.080)	(0.096)	(0.101)	(0.089)
Inverse of Mills ratio	–	–	0.198	0.187	−0.046	0.214	0.089
	–	–	(0.061)	(0.076)	(0.219)	(0.263)	(0.080)
Rent to median ratio in $t-1$	–	–	–	–	0.188	–	–
	–	–	–	–	(0.045)	–	–
Residual from $t-1$	–	–	–	–	–	0.136	–
	–	–	–	–	–	(0.051)	–
Yearly fixed effects	no	no	no	yes	yes	yes	yes
Number of observations	646	646	646	646	461*	461*	490
R-squared (%)	29.88	30.64	31.55	32.20	34.67	34.65	35.09

Notes. Robust standard errors are presented below parameter estimates in parenthesis.
*Some observations are lost because the taste proxy is only observed for prior renters. If prior owners are included and their taste is measured as a function of their previous house value, the results are not qualitatively affected.

In column 2 we see that the impact of median rent in origin city ($\bar{r}_{t-1}$) is positive and significant at the 1% level, consistent with Prediction 1. Correcting for selection and adding fixed effects barely affects this estimate.[7] To quantify the effect size, one may think of two individuals who are similar except in the cost of housing in the city they come from. If one of them came from a city with housing

[7] Around 14% of people who rented in period t bought a home in period $t+1$. It is possible that when they rented their apartment they knew they would buy a home on the following year and hence may have chosen their housing unit differently. Following the suggestion of a referee, we replicated the analysis excluding this 14% of the sample. The estimated elasticity of $\bar{r}_{t-1}$ is 0.17 and is also significantly different from 0 at the 5% level.

costs one standard deviation above the other's, their yearly rental expenditure would differ by 4.2%.[8]

We obtained similar results analysing the impact of previous prices on *homeowners'* behaviour. The estimated effect size for homeowners is such that two otherwise identical households coming from cities that differ in housing costs by one standard deviation are predicted to purchase homes that differ in price by around 11%. For the behaviour of homeowners, however, there are plausible alternative explanations which we cannot rule out. In particular, individuals who have owned expensive homes in the past face both liquidity and tax advantages that might provide an incentive for purchasing an expensive home in the new city.[9] Although we do not believe these causes are sufficient to explain the size of the effect (among other reasons because movers who became owners in the new city but were renters in the old one show a very similar pattern), the analysis of renters is much cleaner, so we report the results for homeowners in the Appendix (Table A2).

In summary we find evidence that $\bar{r}_{t-1}$ has a significant and important impact on how much a household chooses to spend on housing. Adding additional structure to the regressions, such as controls for selection-bias and yearly fixed effects, does not affect the qualitative nature of the estimates.

Before turning to the test of Prediction 2, it is important to explain why we could not test Prediction 1 using a regression in first differences (i.e. analysing the change in amount spent on rent as a function of the change in median rents between the origin and destination cities). Such a regression would not have tested Prediction 1. Our hypothesis is that individuals use previous prices to make future choices; the right comparison, therefore, is across individuals who have experienced different previous prices, not within individuals before and after moving (with a unique price experience). Indeed, the process we propose makes no prediction with respect to the change in housing expenditure between the old and the new city.

A mover from San Francisco is predicted to spend more in Pittsburgh than a mover from Alabama, because Pittsburgh seems cheap relative to San Francisco but expensive relative to Alabama. Whether overall expenditure increases or decreases as a result of a change in price (which is what a regression of first differences would test), in contrast, depends on the price elasticity of demand, not on the elasticity of a reference price. We return to this point in the discussion of Prediction 2, which *is* tested using analysis of changes in consumption.

2.3. *Readjusting Consumption (Testing Prediction 2)*

Prediction 2 states that as people stay in their destination city, their perception of what is appropriate to pay will adjust to local prices, leading them to become

[8] The standard deviation of the median rent is $136 (see Table 1), which corresponds to 21% of the median. Multiplying this by the elasticity of 0.2 leads to an expected difference of 4.2%.

[9] Until 1998, capital gains arising from real estate appreciation were taxable unless a new residence of at least the same value was purchased within 2 years.

dissatisfied with their current housing choice. If people moving from more expensive cities initially overspend on housing (in line with Prediction 1), they should be expected to decrease their expenditure on housing as they remain in the new city, while those coming from cheap cities are predicted to underspend originally and hence should wish to revise upwards.

To test Prediction 2 we need to confine our analysis to the subset of people who move *again* after arriving at their new city and to analyse *changes* in housing consumption rather than levels. In particular consider the housing expenditure equation for household i in time period t:

$$\begin{aligned}\log(r_{i,t}) = \beta_0 + \beta_1 \log(\text{income}_{i,t}) + \beta_2 \text{Number of Adults}_{i,t} \\ + \beta_3 \text{Number of children}_{i,t} + \mathbf{X}_i \boldsymbol{\beta}_4 \\ + \beta_5 \log(\bar{r}_{i,t}) + \beta_6 \log(\bar{r}_{t-1}) + \varepsilon_{i,k,}\end{aligned} \tag{1}$$

where $\mathbf{X}_i$ represent characteristics of the household that do not change with time (e.g. education and gender).

From (1) we see that the change in housing expenditure in period $t + 1$ would correspond to:

$$\begin{aligned}\Delta \log(r_{i,t+1}) = \beta_1\ \Delta\ \log(\text{income}_{i,t+1}) + \beta_2\ \Delta\ (\text{Number of Adults}_{i,t+1}) \\ + \beta_3\ \Delta\ (\text{Number of children}_{i,t+1}) + \beta_6[\log(\bar{r}_t) - \log(\bar{r}_{t-1})] \\ + \varepsilon_{i,t+1} - \varepsilon_{i,t.}\end{aligned} \tag{2}$$

Note that the specification of the reference point term, $[\log(\bar{r}_{i,t}) - \log(\bar{r}_{i,t-1})]$, implicitly assumes that after one year people's perceptions have *fully* adjusted (at least among those who chose to move again). To the extent that households do not fully adjust within one year, our estimate of β_6 is biased towards zero.

The regression estimates of (2) are presented in Table 4. Column 1 is the baseline specification. Columns 2 adds $[\log(\bar{r}_{i,t}) - \log(\bar{r}_{i,t-1})]$ as a predictor, column 3 incorporates yearly fixed effects and column 4 adds the inverse of the Mills ratio from a first stage probit regression for the decision to move again in the new city.[10] The results from the first stage are presented in Table A3 in the Appendix.

All four columns report nearly identical results so they will be discussed jointly. Change in income and in family size both have the expected positive impact on the change in rent. More importantly, the impact of $[\log(\bar{r}_t) - \log(\bar{r}_{t-1})]$ is positive and significant (at the 10% level) in all three specifications, indicating that as people stay in their destination city, they tend to adjust housing expenditure in a direction countering the impact of previous prices. β_6 is estimated at around 0.29, which means that when household move again within the same city, they completely reverse the original impact of $\bar{r}_{t-1}$ (that was estimated at 0.20 in our previous analysis). Because the adjustment is estimated using households who *chose* to relocate, the fact that the point estimate of adjustment is bigger than that of the original impact of $\bar{r}_{t-1}$ (0.29 vs. 0.20) is not particularly worrisome.

[10] None of the variables in the second stage are included in the first stage.

Table 4

Readjustment of Consumption on Year Following Inter-city Move

Dependent variable: Δlog[rent $(t + 1)$]

	(1) Baseline	(2) Adds key variable	(3) Adds year fixed effects	(4)* Adds selection adj.
Intercept	0.072	0.057	0.101	0.932
	(0.040)	(0.040)	(0.101)	(0.596)
Change in log (income)	0.199	0.170	0.157	0.138
	(0.075)	(0.076)	(0.081)	(0.082)
Change in no. of Adults	0.206	0.231	0.253	0.275
	(0.140)	(0.140)	(0.144)	(0.144)
Change in no. of Children	0.047	0.064	0.059	0.066
	(0.071)	(0.072)	(0.073)	(0.073)
Log [median rent (t)] −	–	0.287	0.286	0.295
log [median rent $(t-1)$]	–	(0.163)	(0.171)	(0.171)
Inverse of Mill's ratio	–	–	–	−1.082
	–	–	–	(0.766)
Number of Observations	140	140	140	140
Year fixed effects	No	No	Yes	Yes
R-squared (%)	9.50	11.54	12.87	14.21

Notes. Robust standard errors are presented below parameter estimates, in parenthesis.
*Inverse of Mill's ratio is obtained from a first stage estimating probability of moving again in new city

One of the reviewers pointed out than an additional prediction arising from the mechanism we are proposing is that individuals moving between cities with the largest price differentials should initially choose housing units that differ the most from their long-term ideal, and hence should be the most likely to move again as they remain in the destination city. To test this prediction we incorporated the absolute value of the difference in median rents between origin and destination city as a predictor in the logistic regression assessing which households are more likely to move once in the new city, i.e. into the first stage used to test Prediction 2 (see Table A3 in the Appendix).

Although the estimated impact of the absolute difference in median rents between origin and destination city was positive, it did not approach significance. One possible explanation for the lack of support of this prediction is moving costs. The average effect size we document is around 4% of the monthly rent, an amount probably not large enough to compensate for both the monetary and non-monetary costs involved in moving to a new apartment. In other words, conditional on moving to a new apartment, households do adjust expenditure in the predicted direction, but the costs involved in moving may be too large to justify moving *because* of the difference between actual and optimal expenditures arising from *mistake #37*.

3. Alternative Explanations

In the previous Section we document that movers from expensive cities spend more money on housing in their new city than movers from cheaper cities. We believe that this results from consumers from different markets choosing different units deliberately, and with full knowledge of housing prices, because they seem cheap or expensive depending on the price of housing in the city they moved from. This Section discusses three alternative explanations: unobserved differences in

(1) wealth, and/or
(2) in taste, and
(3) imperfect information.

3.1. *Unobserved Wealth Differences*

The estimation of regression (1) assumes that $\bar{r}_{t-1}$ is not correlated with any omitted variable that is itself correlated with housing choice. The most obvious candidates for violating this assumption are wealth or future income and taste for housing.

In our main analysis we control only for current income. To the extent that *future* income or wealth also influences housing decisions, our results may be affected by omitted variable bias. The measure of income used in our analyses ('total family money' in the PSID), however, incorporates not only wages and salaries but all sources of income for the household, such as dividends, interests, alimony and money transfers. Hence, to some degree, we are already controlling for wealth differences. In addition, as mentioned earlier (footnote 6), using a 4 year average of total family income instead of only current income barely affects our estimates. Moreover, renters do not have much wealth, so differences in wealth among them are unlikely to be consequential (the median wealth for renters in the PSID in 1984 was of just $1,000). It is also unlikely that renters would be able to borrow against future income to finance their current rental expenditures.

Nevertheless, if there is a residual of relevant unobserved income, and this residual correlates positively with the cost of housing in cities households move from, then our results may be biased upwards due to such omission. The analyses that follow, therefore, seek to assess the potential problem that unobserved wealth or income differences across renters moving from different cities may pose to our interpretation of the results.

The PSID collects information about households' wealth only every 5 years, so we cannot simply add wealth as a control into our regressions. Instead, we conduct indirect analyses to evaluate whether it is plausible that the omission of wealth or future income may be driving the relationship between $\bar{r}_{i,t-1}$ and $r_{i,t}$. In particular, as we describe in detail below, we find that

(*i*) omitting all *observable* income variables barely affects the point estimate of our key estimate variables,
(*ii*) that adding more controls for income also leaves the estimated impact of $\bar{r}_{i,t-1}$ on $r_{i,t}$ practically unchanged, and finally,

(*iii*) that $\bar{r}_{i,t-1}$ does not predict the consumption of goods other than housing, contrary to what would be expected if the relationship between $\bar{r}_{i,t-1}$ and $r_{i,t}$ was mediated by income.

(*i*) *Eliminating observable controls for income.* One way to asses the extent to which the estimated impact of $\bar{r}_{i,t-1}$ may be picking up variation in *unobserved* differences in income is to evaluate how much variation in *observed* income it picks up. To do so, we replicate the regression analysis presented in Table 3 without any covariates, and then progressively add current income and the rest of the covariates. The results are presented in Table 5, in which column 1 reports the results for the most basic model, column 2 adds current income, and column 3 adds the rest of the covariates used in our main analysis (the same specification as column 2 in Table 3).

Comparing the point estimate for $\bar{r}_{i,t-1}$ (log of median rent in $t-1$) between columns 1 and 2 in Table 5 one can see that it is barely affected by the exclusion of current income from the regression (it changes from 0.221 to 0.206) and is practically unchanged when all other covariates are added. Given that omitting (all) controls for observable income leaves our estimate of $\bar{r}_{i,t-1}$ nearly unchanged, there is little reason to suspect that the omission of unobservable income is producing significant bias.

(*ii*) *Incorporating additional proxies for wealth.* Another way to assess the potential threat of unobserved income to our identifying assumption is to add proxies for wealth to the main regression and to examine, again, what happens to the coefficient of $\bar{r}_{i,t-1}$. A commonly used proxy is consumption; the PSID only has food consumption data, so that is what we use.[11]

Table 6 presents regressions in which food expenditures are added to the specification presented in Table 3, column 4. Although all covariates from Table 3 were included in the regression, Table 6 reports only the most relevant ones. Column 1 presents the key coefficients from column 4 in Table 3, column 2 adds money spent on food at home and column 3 away from home. There is a significant positive relationship between food consumption and housing expenditure, consistent with the proposition that uncontrolled for wealth or income differences have an impact on housing expenditure. The inclusion of food in the regression, however, has practically no effect on the estimate or standard error of the coefficient of $\bar{r}_{i,t-1}$.

(*iii*) *Predicting food consumption with previous cost of housing.* A third prediction that arises from the hypothesis that movers from expensive cities spend more on housing because they are richer is that they will spend more on non-housing goods as well. To test this prediction we ran regressions analogous to the housing demand ones but with food expenditure as the dependent variable. Table 7 reports the results of these regressions. The dependent variable in columns 1 and 2 is the log of food at home, and in columns 3 and 4 it is the log of food

[11] To assess whether food consumption is a valid wealth proxy, we ran a regression with wealth reported in 1984 as the dependent variable, and food consumption in 1984 as the key predictor. We found that food consumption *is* a significant predictor of wealth, even after controlling for the measures of wealth which we do include in the main analysis. The results from this regression are available from the authors upon request.

Table 5

Comparison of the Main Regression With and Without Income Covariates

	(1) No controls	(2) Only income	(3) All observables*
Intercept	1.877	−1.757	−1.621
	(0.735)	(0.724)	(0.697)
log(Median Rent in *t*)	0.462	0.478	0.494
	(0.098)	(0.088)	(0.087)
log(Median Rent in $t-1$)	0.221	0.206	0.203
	(0.092)	(0.081)	(0.079)
log(Income)	–	0.348	0.284
	–	(0.027)	(0.029)
Child	–	–	0.045
	–	–	(0.017)
Adult	–	–	0.146
	–	–	(0.044)
Age	–	–	0.004
	–	–	(0.007)
Age squared × (100)	–	–	−0.002
	–	–	(0.007)
College	–	–	0.132
	–	–	(0.036)
Female	–	–	0.021
	–	–	(0.047)
Number of observations	646	646	646

Notes. Robust standard errors below parameter estimates.
Column 3 is the same regression as Column 2 in Table 3.

Table 6

Subset of Parameters Estimates From Housing Demand With Food as Proxy for Wealth

Dependent variable: Log [rent (*t*)]

	(1) Baseline Table 3 (4)	(2) Food at home	(3) Food away from home
log(median rent origin city)	0.192	0.181	0.190
	(0.080)	(0.077)	(0.081)
log(food consumption)	–	0.102	0.051
	–	(0.004)	(0.019)
Number of observations (*N*)	646	637	591
Number of covariates (*K*)	18	19	19
R-Squared (%)	32.20	33.95	34.58

Robust standard errors below parameter estimates.

away-from-home. In all specifications the variables that we would expect to predict food consumption (income and family size) are indeed significant but, contrary to the story where housing prices proxy for unobserved wealth, the coefficient of $\bar{r}_{i,t-1}$ is not significant (p-values > 0.4).[12]

[12] The regressions are run using the 2-stage Heckman procedure in order to emulate as close as possible the rent regressions. Excluding the Mills ratio from the OLS regression has no real effect on any of the coefficients.

Table 7

Food Demand Estimations for Renters

Dependent variable:	(1) Food at home Without cost of housing $t-1$	(2) Food at home With cost of housing $t-1$	(3) Food away Without cost of housing $t-1$	(4) Food away With cost of housing $t-1$
Constant	5.449	5.033	2.704	2.021
	(0.805)	(0.937)	(1.330)	(1.636)
log (income)	0.160	0.160	0.353	0.352
	(0.051)	(0.051)	(0.083)	(0.083)
Number of children in household	0.179	0.180	−0.020	−0.019
	(0.02)	(0.021)	(0.040)	(0.040)
Number of adults in household	0.265	0.266	−0.245	−0.245
	(0.050)	(0.049)	(0.095)	(0.095)
Age of head of household	0.014	0.013	0.025	0.024
	(0.010)	(0.010)	(0.018)	(0.018)
(Age squared)/100	−0.007	−0.006	−0.037	−0.036
	(0.011)	(0.011)	(0.020)	(0.020)
Attended college (1 or 0)	0.059	0.060	0.066	0.068
	(0.045)	(0.045)	(0.080)	(0.080)
Head of household is female (1 or 0)	−0.188	−0.190	−0.388	−0.392
	(0.064)	(0.064)	(0.098)	(0.098)
log (median rent destination city)	0.017	0.001	0.179	0.150
	(0.100)	(0.102)	(0.182)	(0.183)
log (median rent origin city)	–	0.083	–	0.139
	–	(0.094)	–	(0.177)
Inverse of Mill's Ratio	−0.057	−0.056	0.050	0.050
	(0.098)	(0.098)	(0.172)	(0.172)
Yearly fixed effects	Yes	Yes	yes	Yes
Number of observations	634	634	594	594
R-squared (%)	31.29	31.37	16.76	16.85

Robust standard errors below parameter estimates.

In sum we find no evidence that the relationship between current housing expenditure and previous cost of housing is caused by unobserved differences in income or wealth across individuals.

In addition to the evidence presented above, it is worth reiterating that Prediction 2 cannot be accounted for by unobserved income differences. Although richer households rent more expensive apartments, on average, there is no reason for them to systematically revise consumption as they stay in a new city.

3.2. *Taste for Housing*

If households in cities with different housing costs have systematically different unobserved 'taste for housing', then the coefficient on $\bar{r}_{i,t-1}$ in the housing demand regression may be picking up stable taste differences across households.[13]

[13] We are using the term 'taste' to mean any unobserved characteristic of a household which may lead it to desire to consume more (or less) housing *systematically*: from enjoying large crowds of friends over for dinner, to a preference for neighbourhoods with good public schools to – again - unobserved wealth differences.

The *average* household in an expensive city should have a lower preference for housing than the *average* household in a cheap city, because those who like housing more should sort themselves into cities where they can afford to consume large quantities of it. We are not, however, analysing the behaviour of randomly selected households but of those who *chose* to move to a different city. It is possible that in a self-selected sample of movers such as ours, movers from expensive cities enjoy housing consumption more than those from cheaper ones. This type of self-selection is plausible; households with children, for example, will tend to leave expensive cities searching cheaper large homes, while households without them will be more likely to leave cheap cities, in search of the amenities of expensive ones.

Addressing this potential problem requires a proxy for taste for housing. We construct such a proxy by calculating for period $t-1$ (i.e., in the origin city) the difference between what we would *expect* a given household to spend in housing based on their observable characteristics, and what it *actually* spends.

In particular we construct the following two proxies:

(*i*) 'Relative expenditure' ($r_{i,t-1}/\bar{r}_{i,t-1}$): constructed by dividing the amount spent in rent in period $t-1$ by the median in that city in $t-1$.
(*ii*) 'Residual from $t-1$' (e_{t-1}): Constructed by running a housing demand regression for period $t-1$, and using the residuals of this regression as a proxy for taste.

Using these proxies we

(*i*) examine whether indeed people with a high taste for housing are more likely to move from more expensive cities and
(*ii*) add the taste proxies to the main regression.

In addition

(*iii*) we replicate the analysis excluding from the main regression people who gave a housing related reason for moving.

(*i*) *Do people with high taste for housing leave expensive cities?* We estimated logistic regressions on the decision to move to another city. If households with a higher taste for housing are leaving expensive cities and those with lower taste are leaving cheaper cities, the interaction term between $e(t-1)$ and $\bar{r}_{i,t-1}$ should have a positive impact in the probability of moving out of a city. The results of such a regression run on a sample that includes all households in the PSID are reported in Table 8, column 1. Column 2 reports the results from a sample of households that moved between cities, where the dependent variable is 1 if the household moved to a more expensive city, and 0 otherwise. The interaction term of $e(t-1)$ with $\bar{r}_{i,t-1}$ is not significant in both regressions. Thus, we find no evidence to support the direct mechanism of self-selection out of cities that could lead to a positive relationship between unobserved taste and $\bar{r}_{i,t-1}$.

(*ii*) *The impact of observable variation in taste.* As a second approach we examine the impact of adding and subtracting observable measures of heterogeneity in tastes. Note that in the regressions used in the discussion of unobserved income differences (Table 5), we not only excluded all observable controls for income, but also

Table 8

Logistic Regression for Decisions to Move to Other City (Renters in t–1)

Dependent Variable: 1 if moved 0 if did not

	(1) $Y = 1$ if moved to other city	(2) $Y = 1$ if moved to more expensive city
Intercept	−2.2149	7.6464
	(0.5512)	(1.3423)
Family income (in $1,000s)	0.0027	−0.0037
	(0.0002)	(0.0042)
Change in income (in $1,000s)	−0.0086	0.0028
	(0.0020)	(0.0050)
Number of adults	0.2248	0.2536
	(0.1350)	(0.2986)
Change in number of adults	−0.0144	−0.7305
	(0.1414)	(0.3209)
Number of children	−0.1708	0.0617
	(0.0505)	(0.1154)
Change in number of children	0.0158	−0.2768
	(0.0896)	(0.1908)
Head of household is female (1 or 0)	−0.1876	0.3105
	(0.1384)	(0.3070)
Attended college (1 or 0)	1.0968	0.2558
	(0.1023)	(0.2328)
Age of head of household	−0.0446	−0.0861
	(0.0225)	(0.0575)
Age squared	0.0003	0.0010
	(0.0002)	(0.0007)
(Median Price in $t-1$)/1000	0.1470	−10.40
	(0.4140)	(1.06)
$e(t-1)$	1.0483	2.6738
	(0.6059)	(1.4137)
$e(t-1) \times$ [Median rent in $(t-1)$]/1000	−0.4700	−3.2600
	(0.9270)	(2.1800)
Number of Observations	6258	537
Pseudo R-Squared (%)	7.45	25.27

Standard Errors below parameter estimates.

for taste (e.g. family size and age). These exclusions had virtually no impact on the coefficient of $\bar{r}_{i,t-1}$. The fact that excluding observable measure of taste does not affect our estimates suggests that excluding unobservable measures probably does not either.

We added the two proxies for housing taste described above to the housing demand regression and examined what happened to the coefficient of $\bar{r}_{i,t-1}$. If the estimated coefficient diminishes significantly, our results may indeed be the consequence of improperly controlling for taste differences. Columns 5 and 6 of Table 3 show the results of these regressions. Notice that the coefficient of both taste proxies are positive and significant, consistent with the notion that they are picking up *stable* unobserved differences in housing preferences across households. However, far from eliminating the effect of $\bar{r}_{i,t-1}$ on $r_{i,t}$, adding the taste proxies actually increases it.

(*iii*) *Excluding housing motivated moves.* A final way to assess the potential impact of taste is to limit our sample to movers who were less likely to have chosen their destination city because of its housing market. If endogenous moves are driving our results (e.g. families with children seeking large homes in cheaper cities), then restricting the sample to those moving for exogenous reasons should diminish (or eliminate) the reported relationship between $\bar{r}_{i,t-1}$ and $r_{i,t}$. Column 7 of Table 3 reports the estimates of the main regression excluding households who moved for a housing related reason. The coefficient of $\bar{r}_{t-1}$ exhibits only a minor drop from 0.192 in column 4 to 0.182 in column 7.

In sum we find no evidence either of a connection between taste for housing, cost of city and moving that would lead movers from expensive cities to have higher taste for housing than movers from cheaper ones, or of the consequences that such self-selection would generate. In addition, as was the case with unobserved wealth, stable taste differences cannot account for the evidence consistent with Prediction 2, whereby households systematically revise housing expenditure as they stay in their new city.

3.3. *Imperfect Information*

A third possible alternative explanation for the impact of previous housing prices involves imperfect information. If people expect prices in their new city to be more similar to prices in their previous city than they really are, then movers from expensive cities might be more likely to rent expensive apartments, not realising that cheaper options are available.

An account based on imperfect information would suggest that those arriving from expensive cities will search little (thinking they have found a bargain after seeing just a couple of apartments), while those moving from cheaper cities would engage in a long (and frustrating) search, presumably until they have achieved a more realistic assessment of local prices. Movers from expensive cities, then, should rent based on a biased assessment of local prices while movers from cheap cities should eventually gain a much more accurate idea of prices.

We do not have data on search behaviour per se, but we do know whether people decided to rent an apartment before buying their home, or if they bought a home as soon as they arrived in the new city. Interpreting this rent-then-own behaviour as search, the imperfect information story would suggest that movers from expensive cities will be less likely to rent before buying. Although the proportion of owners that rented prior to owning was indeed slightly higher for those coming from more expensive cities (29% vs. 24%), this difference was not significant ($p = 0.44$).

In addition to this direct test of the mechanism of a differential search based explanation, we also tested additional implications that could be expected to follow from it. In particular:

(*i*) Because ultimately, only movers from expensive cities would have a biased assessment of local prices (those coming from cheaper cities would search longer and eventually learn about local prices), their choices should be more affected by prices in the previous city than choices made by movers from cheaper cities.

(*ii*) Because movers from expensive cities did not conduct an extensive search, the apartment they chose should be further from their optimal (on all dimensions) and, hence, they should be more likely to move out after their first year in the new city.

(*iii*) Because movers from expensive cities had a larger bias in their assessment of local prices at the time they made a final housing decision, conditional on moving, they should readjust their expenditure in housing more than movers from cheap cities.

To test (*i*) we add to the housing demand regression a dummy variable that takes the value of 1 when the household came from a city that was more expensive than the destination city and 0 otherwise, and we interact this dummy with current and previous prices (not reported). The point estimate of the elasticity suggests a slightly larger influence of previous housing prices on current expenditure for movers from expensive cities: 0.25 versus 0.19 for those from cheaper cities. But, neither the dummy nor its interaction with previous prices is significant.

To test (*ii*) we compare the probability that households coming from expensive and cheap cities will move in the destination city within a year of arriving. We find that movers from *cheaper* cities are slightly *more* likely to move than those from expensive cities (58% vs. 55%), although the difference is not statistically significant. A logistic regression that controls for observables replicates this result, now marginally significant at the 10% level (not reported).

To test (*iii*) we add the same dummy variable and its interaction with the key independent variable as in (*i*) to the regression that tested Prediction 2: the adjustment of rent among those moving again in the destination city. Movers from expensive cities show *less* adjustment than movers from cheap cities, although the difference is not significant, perhaps due to small number of observations (140) used in the analysis. In sum, out of the four tests of the imperfect information based explanation, two have weak and non-significant confirmatory evidence, and the other two have weak contradictory evidence. Imperfect information, it seems, is unlikely to be the cause of the relationship between past housing prices and subsequent spending on housing.

In addition, given the importance of the decision involved, and the low cost of acquiring information about housing prices, an imperfect information based explanation would be inconsistent with optimal information search. Note that if movers knew only the median rent in the city they moved to, our analyses would not find an effect of city of origin, since median price is all we use to predict their behaviour.

4. Possible Psychological Mechanisms

Having documented the impact of previous prices on current consumption and ruled out, to the maximum extent that we could, alternative explanations, we now discuss the specific psychological mechanisms that could potentially underlie the effect. The predictions we postulated and found empirical support for originated from previous experimental work on contrast effects. It remains unclear, however, exactly how previous prices influence subsequent consumption decisions.

Possible mechanism for the effect include Thaler's (1985, 1999) notion of *mental accounting*. If people establish mental budgets to facilitate their financial decisions and there is some stickiness to these budgets, then the more expensive a city an individual came from, the bigger his or her mental budget and the larger his or her propensity to consume housing, would be. A similar prediction would arise from Thaler's notion of *transaction utility*, which proposes that people derive utility not only from consumption but from getting good deals (and disutility from bad ones). If people judge how good a deal they are getting based on previous prices they have observed, then people from more expensive cities will derive more utility from consuming larger amounts of housing than movers from cheaper locations.

Another possible mechanism relies on the notion of thick indifference curves, mentioned in the background Section (Loomes, 1988). If indifference curves are thick – if people have some uncertainty about trade-offs that leave them equally happy – then factors that influence which bundles are considered first may in turn affect choice (a notion closely related to Herbert Simon's *satisficing*). In the case of housing, if movers from expensive markets look at expensive/high-quality units first, while those from cheaper ones may start their search with the less-expensive/lower-quality units, they will hit their region of indifference from different directions and choose different housing bundles.

A final mechanism – or actually set of mechanisms – that could produce the effect we observed involves habit formation. The implications of habit formation, however, depend on what attributes people develop habits for.

One possibility draws on the documented positive correlation between price of housing and quality of non-housing amenities such as restaurants and nightlife across cities; see Black *et al* (2002), Roback (1982), Rosen (1974), Blomquist *et al.* (1988) and Kahn (1995) among others. It is possible that someone coming from an expensive city with high quality amenities might choose to compensate themselves for the loss of these amenities by spending more on housing. Such an account seems possible but leaves unexplained why the culturally deprived household chooses to compensate in this fashion rather than, for example, taking more expensive vacations or buying more expensive automobiles. To the extent that people compensate for the loss of amenities specifically by spending more on housing, we would argue, they do so precisely because, to a mover from an expensive city, housing appears cheaper and hence more appealing as a substitute to amenities, than to a mover from a less expensive city.

It is also possible that, after enjoying life in roomier housing units for some time (i.e. living in a cheaper city) people may develop a habit for it (Becker and Murphy, 1988) or use it as a reference point with loss-averse valuation of deviations from it (Tversky and Kahneman, 1991).[14] This would, however, predict the opposite of the pattern that we observe, movers from expensive cities, who have

[14] Intuition and economic theory suggest that more expensive cities have smaller apartments. We corroborated this intuition by comparing the average number of bedrooms and squared footage in apartments across cities with different housing costs in the American Housing Survey; more expensive cities indeed have smaller housing units.

grown accustomed to smaller housing units, should spend *less* on housing in their new city. To the extent that we are not appropriately controlling for taste differences that work in the opposite direction, the estimated contrast effect would be biased towards zero.

If people's decisions are influenced both by the prices they observed in the previous city and the quantities they consumed (be it because of habit formation or loss aversion), one might wonder why the impact of previous prices on current spending dominates the opposing impact of previous quantities consumed. Perhaps the simplest explanation is that prices form a stronger reference point than quantities. Price is an attribute that is easy to compare across cities ($1,000 in Pittsburgh is the same as $1,000 in San Francisco) while housing attributes are so diverse that tradeoffs are more difficult (e.g. ocean view in Florida vs. good insulation in Pittsburgh). Hsee and colleagues (Hsee, 1996; Hsee *et al.*, 1999) have shown than when people evaluate options jointly (as opposed to assigning a value to each of them in isolation), they put more weight on attributes that are easy to compare.

Because housing characteristics differ across cities, it is also possible that the attributes people in one city grew accustomed to are simply not available (or available at very different marginal costs) in other cities, limiting the impact of previous consumption on current expenditures. For example, New Yorkers may really want an apartment close to the subway when they move to other cities, but living close to the subway in Pittsburgh does not come at a premium, and there is simply no subway to live close by in Tucson.

5. Conclusions and Discussion

This study is, to our knowledge, the first to test for context effects in an important domain of economic behaviour. More studies are needed to analyse how widespread this phenomenon is. It is possible, for example, that housing is especially prone to effects such as those documented here due to the dearth of feedback that people get on their decisions. Most people purchase or rent only a small number of houses or apartments over the course of their lives. It would be interesting to test for similar effects in markets in which consumers engage in more frequent transactions.

Housing is also extremely lumpy and relatively unique. For both of these reasons, the satisfactions derived from housing are difficult to compare with those that could be obtained from alternative uses of the money.

On the other hand, housing has many properties that weigh against the influence of non-normative external cues. Housing is a good that is traded in competitive markets, experienced daily throughout one's life, and is of sufficient magnitude to motivate people to make the decision carefully. It is natural to expect that preferences over less tangible goods, such as clean air, human capital, diseases, the value of time and even time discounting, would be at least as unstable and susceptible to arbitrary cues.

Our findings also have potentially important implications for empirical investigations of preferences. Applied economists from various fields use observed consumer behaviour to infer preferences in an array of different domains, such as wage differentials in labour economics, hedonic prices in housing economics and travel costs in environmental economics among many others. The validity of such studies relies on the assumption that the preferences that are revealed for the goods and services the researchers are studying exist and are stable. If, as the results from this article suggest, preferences are affected by arbitrary cues, future empirical work should strive to identify such cues and include them in the analysis, particularly if they are likely to be affected by the very policy under review.

Our findings also have implications for short vs. long-term elasticities. Standard assumptions from economics indicate that demand should be more price sensitive in the long run than in the short run, because in the long run people have the opportunity to adjust fixed inputs. Our results suggest that contrast effects work in the opposite direction: in the short run price changes will appear as more dramatic and influence behaviour both directly through a 'price-effect', and indirectly through a 'contrast effect' with previous prices. In the longer term people grow accustomed to new prices, eliminating the initial contrast. It seems possible that in certain markets where fixed inputs are secondary and contrast effects are important, long-term term demand may actually be less price-sensitive than short term. This promises to be an interesting topic for future research.

If further studies replicate our findings and generalise them to other domains, the consequences could be important for economics. If consumer behaviour can be affected by arbitrary cues, then the interpretation we give to consumer sovereignty, welfare and even the very concept of utility seems to be in need of re-examination.

The Wharton School of the University of Pennsylvania
Carnegie Mellon University

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Table A1

Logistic Regression on Decision to Purchase ($Y = 1$) or Rent ($Y = 0$)

Intercept	−11.085
	(2.949)
Log (permanent income)	0.950
	(0.182)
Dummy for owner in period $t - 1$ (1 if owner, 0 if renter)	1.509
	(0.184)
Age of head of household	0.131
	(0.037)
(Age squared)/100	−0.089
	(0.039)
Attended College (1 or 0)	0.150
	(0.206)
Number of adults in household	0.637
	(0.250)
Number of children in household	0.138
	(0.090)
Head of household is female (1 or 0)	0.181
	(0.312)
Log (own/rent cost ratio in destination city)	−1.135
	(0.419)
Log (own/rent cost ratio in origin city)	0.385
	(0.346)
−2log Likelihood	823.0
Number of Observations	928
Pseudo R-squared (%)	27.37

Standard error in parenthesis below parameter estimates.

Table A2

Housing Demand Estimations for Home Buyers

Dependent Variable: log (dollar amount of price of home)

	(1) Baseline	(2) Adds costs in previous city	(3) Adds selection adjustment	(4) Adds fixed effects	(5) Adds relative expenditure $(t-1)$	(6) Adds $e(t-1)$	(7) Excludes housing motivated moves
Constant	0.147	−1.376	−0.641	−1.018	0.146	0.931	−0.655
	(1.445)	(1.418)	(1.465)	(1.393)	(1.761)	(1.895)	(1.486)
Log (income)	0.441	0.424	0.395	0.405	0.154	0.372	0.392
	(0.058)	(0.060)	(0.060)	(0.060)	(0.067)	(0.066)	(0.079)
Number of children in household	0.061	0.059	0.032	0.037	0.007	−0.037	0.054
	(0.029)	(0.028)	(0.029)	(0.029)	(0.038)	(0.037)	(0.029)
Number of adults in household	0.093	0.104	0.030	0.023	0.046	−0.126	−0.055
	(0.118)	(0.116)	(0.122)	(0.119)	(0.176)	(0.174)	(0.126)
Age of head of household	0.042	0.042	0.020	0.021	−0.009	−0.053	0.019
	(0.018)	(0.018)	(0.018)	(0.018)	(0.029)	(0.029)	(0.029)
Age squared) × 100	−0.037	−0.038	−0.022	−0.022	0.003	0.042	−0.015
	(0.019)	(0.019)	(0.019)	(0.019)	(0.025)	(0.025)	(0.020)
Attended college (1 or 0)	0.093	0.098	0.065	0.049	0.034	0.093	0.348
	(0.077)	(0.076)	(0.078)	(0.076)	(0.085)	(0.093)	(0.079)
Head of household is female (1 or 0)	0.232	0.225	0.275	0.279	0.196	0.280	0.131
	(0.134)	(0.134)	(0.136)	(0.131)	(0.179)	(0.181)	(0.137)
Log (median house price in t)	0.466	0.365	0.398	0.411	0.328	0.531	0.388
	(0.111)	(0.117)	(0.113)	(0.108)	(0.132)	(0.131)	(0.111)
Log (median house price in $t-1$)	–	0.245	0.261	0.266	0.521	0.250	0.272
	–	(0.073)	(0.072)	(0.074)	(0.098)	(0.088)	(0.081)
Inverse of Mill's Ratio	–	–	−0.261	−0.265	−0.527	−1.169	−0.0183
	–	–	(0.096)	(0.094)	(0.470)	(0.485)	(0.098)
Own price $(t-1)/$ median price $(t-1)$	–	–	–	–	0.444	–	–
	–	–	–	–	(0.070)	–	–
Residual regression in $(t-1) - e(t-1)$	–	–	–	–	–	0.623	–
	–	–	–	–	–	(0.090)	–
Fixed effects for year (8 dummies)	no	no	no	yes	yes	yes	yes
Number of observations	271	271	271	271	185	185	231
R-squared (%)	38.02	40.35	41.91	49.24	62.69	61.20	42.32

Robust standard errors are presented below parameter estimates in parenthesis.

Table A3

Logistic Regression on Decision to Move in New City

Intercept	−0.111
	(0.093)
Absolute change in Income between t and $t+1$	0.001
	(0.002)
Absolute change in adults between t and $t+1$	0.138
	(0.206)
Absolute change in children between t and $t+1$	0.114
	(0.113)
Absolute difference in housing costs between cities	0.076
	(0.344)
Number of Observations	550
Pseudo R-squared (%)	0.36

Standard errors below parameter estimates.

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