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ABSTRACT

How malleable is alcohol consumption? Specifically, how much is alcohol consumption driven by the current environment versus individual characteristics? To answer this question, we analyze changes in alcohol purchases when consumers move from one state to another in the United States. We find that if a household moves to a state with a higher (lower) average alcohol purchases than the origin state, the household is likely to increase (decrease) its alcohol purchases right after the move. The current environment explains about two-thirds of the differences in alcohol purchases. The adjustment takes place both on the extensive and intensive margins.

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A data appendix is available at http://www.nber.org/data-appendix/w30245

1 Introduction

Alcohol is one of the leading killers among substances. In 2016, alcohol was responsible for 5.3% of all deaths and 7.2% of all premature deaths (among persons 69 years of age and younger) worldwide (World Health Organization, 2018). Beyond direct health consequences, excessive alcohol consumption generates a social and economic burden on other people (Cook and Moore, 2000; Cawley and Ruhm, 2011).

Various factors affect alcohol consumption, such as taxes and other regulations, peers, and social norms. To identify each factor's direct impact, studies have used changes in regulation (e.g. Carpenter, 2004; Marcus and Siedler, 2015) and random assignment of peers (e.g. Eisenberg et al., 2014). These studies give us a well-identified local average treatment effect in the short term. But over the long term, institutions and cultural norms are known to interact (Guiso et al., 2016), either magnifying or decreasing each factor's direct effect.[1] For example, peers who do not consume alcohol could vote for stricter alcohol regulation; strict alcohol regulation could lead to norms of consuming less alcohol and affect how children grow up viewing alcohol, which again changes the norm and leads to a different local environment. Quantifying the overall impact of the environment is important, as it tells us how malleable alcohol consumption is and informs us how much room there is for any alcohol policy. But measuring the combined effect is challenging because it typically requires looking at a long time horizon. However, over the long term, economic conditions change in other ways that also affect alcohol consumption. Therefore, less is known about the magnitude of the combined effect.

In this paper, we study how much the current environment drives alcohol purchases. To answer the question, we analyze changes in alcohol purchases when consumers move from one state to another in the United States. The magnitude of the change in movers' alcohol purchases allows us to measure the relative importance of the current environment. Understanding how alcohol consumption responds to the current environment is crucial for designing effective policies.

Our empirical strategy relies on the fact that the environment, including supply conditions, alcohol regulation, taxes, and movers' peers, changes discretely when consumers move. If the current environment mainly drives alcohol consumption, we would expect a jump in the mover's alcohol purchases in the same direction as the gap between the destination and origin state. On the other hand, if alcohol consumption is only driven by individual charac-

[1] For an overview of interactions of institutions and cultural norms, see Alesina and Giuliano (2015).

teristics, such as personal preferences and past experiences, we would not expect a change in the mover's alcohol purchases.

We study the question using a panel of movers in the NielsenIQ scanner data of alcohol purchases. We observe their alcohol purchases years before and after the move. Our primary outcome variable is the logarithm of quarterly off-premise alcohol purchases measured in pure ethanol. We also measure alcohol purchases separately in beer, wine, and liquor categories and analyze the extensive margin—whether consumers buy any alcohol at all.

We estimate event study and difference-in-differences regressions with consumer and time period fixed effects. A possible concern with our identification strategy is that moves occur due to a shock that changes alcohol purchases. To alleviate the concern, we provide two pieces of evidence. First, we restrict the sample to movers whose observable characteristics, like household size, employment, and marital status, don't change, and our results remain similar. Second, we compare trends in pre-move purchases of movers to higher versus lower alcohol-purchasing states. This shows that the movers who chose to go to different states before the move had similar trends in their purchases.

We find that if a household moves to a state with a higher (lower) average alcohol purchases than the origin state, the household is likely to increase (decrease) its alcohol purchases right after the move. About two-thirds of the gap in alcohol purchases between the origin and destination state closes immediately when a consumer moves. No sizable further change is seen after the immediate jump. This finding implies that the current environment explains a large share of the differences in alcohol purchases and that government policies and regulations targeting the drinking environment (e.g. alcohol availability) could have a significant impact on the amount of alcohol consumed.

There is some heterogeneity across product types. Consumers adjust their wine purchases more and their liquor purchases less. The adjustment takes place both on the extensive and intensive margins. On the extensive margin, movers are more (less) likely to buy alcohol when moving to a state with a larger (smaller) share of consumers buying alcohol. On the intensive margin, movers who bought alcohol before the move adjust the quantity in the direction of the average purchases in the destination state. There is evidence of asymmetries in adjustment, but mainly on the extensive margin. Consumers are more likely to adjust upward (start to purchase alcohol) than downward (stop buying alcohol). Our results are robust to a number of robustness checks using alternative samples, functional forms, controls, and geographic aggregation levels.

An important concern about the analysis is underreporting of alcohol purchases. In

particular, whether the magnitude of underreporting changes at the move. This could happen if reporting depends on retail conditions, such as alcohol availability in grocery stores, which vary by state. We provide evidence that our results are not driven by the changes in reporting related to the retail conditions. When we restrict the sample to moves between states with similar retail conditions, movers still adjust their purchases. Another concern is that we only measure off-premise alcohol purchases, and we provide suggestive evidence that this is predictive of overall heavy drinking and chronic liver disease and cirrhosis mortality.

Our results based on movers and alcohol purchases might not generalize to the general population and other products. If movers are more likely to adjust to the new environment, our results provide an upper bound of how malleable alcohol purchases are in the general population. However, movers are also interesting in their own right because they are a large share of the population—more than 30% of the U.S. population has moved across the state lines in their lifetime (Molloy et al., 2011). Furthermore, our findings of the extent of the adjustment are specific to alcohol and don't necessarily generalize to other products. For example, we show that movers adjust their cigarette purchases less than alcohol, and they don't significantly adjust their food purchases. We also show that our estimation method replicates the results from the literature of a large adjustment for brands as in Bronnenberg et al. (2012) and no adjustment for healthy eating choices as in Allcott et al. (2019).

Our work contributes to the ongoing debate about how malleable alcohol consumption is and how much it is driven by the environment.[2] We provide new causal evidence that the current environment explains about two-thirds of the variation in alcohol purchases. The current environment consists of many factors (including local regulation, norms, and peers), which in the long-term affect each other, making it difficult to measure the combined effect. Therefore, the literature mostly estimates the short-term direct effect of either taxes and regulations or peer effects. A notable exception is Yakovlev (2018), who uses a structural model and data on alcohol consumption and peers to estimate the impact of an increase in the price of vodka in Russia. He finds that peer effects play a large role in magnifying the impact of the price increase. We contribute to the literature by using an alternative method based on movers to overcome the difficulties in measuring the combined effect.

Our work also adds a new finding to the literature on how changes in the environment

[2]The literature has studied the impact of environmental factors such as alcohol taxes and regulations (Carpenter, 2004; Marcus and Siedler, 2015; Aguirregabiria et al., 2016; Bernheim et al., 2016; Hinnosaar, 2016; Miller and Weinberg, 2017; Illanes and Moshary, 2018; Griffith et al., 2019; Miravete et al., 2019; Seo, 2019; Kueng and Yakovlev, 2021; Gehrsitz et al., 2020), peer effects (Lundborg, 2006; Clark and Lohéac, 2007; Eisenberg et al., 2014), and individual characteristics such as family background, cognitive ability, discount rate, and self-control (Cutler and Lleras-Muney, 2010; Schilbach, 2019).

affect consumer behavior. In the case of food and drinks, the question was studied using movers in the same dataset, the NielsenIQ scanner data, by Bronnenberg et al. (2012); Allcott et al. (2019); Hut (2020).[3] Bronnenberg et al. (2012) study the evolution of brand preferences and find that 60% of the gap between the destination and the origin average purchases of grocery products is bridged immediately after the move. Allcott et al. (2019) and Hut (2020) study how much the healthiness of food purchases changes with a move and find that the change is very small. Our results align with the evidence of large changes in brand choices while standing in contrast to little changes in the healthiness of food purchases. We hypothesize that large regional differences in alcohol regulation (availability and taxes) are the main reason for the large adjustment in alcohol purchases. So large regional differences in supply conditions are absent in food healthiness while existing for brands.

More generally, the literature on the convergence of behaviors of migrants has often found evidence of persistence, for example, for food preferences (Atkin, 2016), living arrangements (Giuliano, 2007), and fertility and female labor force participation (Fernández and Fogli, 2009). Our finding of sizable changes in alcohol purchases is not surprising, considering that alcohol consumption is a social activity. It is plausible that alcohol consumption is more influenced by social forces and the environment than other behaviors examined in the literature.

To quantify the importance of the environment in affecting population health, Finkelstein et al. (2016, 2018, 2021) develop and employ the same empirical strategy of examining migrants' behaviors. Our results on the role of the environment in alcohol purchases add an important dimension not previously examined in this literature. More generally, our paper relates to the recent work of Chetty et al. (2016) and Chetty and Hendren (2018b), which find that where one grows up is an important factor in affecting long-term outcomes such as intergenerational mobility and earning. Our paper provides an additional mechanism of why the environment matters. According to our findings, the current environment largely determines individuals' alcohol purchases. Using a simple back-of-envelope calculation would suggest that if a household of two adults and one child moves from Utah to New Hampshire,

[3]Other papers using data of movers have estimated the impact of urban sprawl on obesity (Eid et al., 2008), the impact of location on healthcare utilization (Finkelstein et al., 2016), food consumption in India (Atkin, 2016), intergenerational mobility (Chetty and Hendren, 2018b,a), opioid abuse (Finkelstein et al., 2018), relative obesity (Liu and Zuppann, 2018), physicians practice styles (Molitor, 2018), mortality (Finkelstein et al., 2021), and consumer financial distress (Keys et al., 2020). More generally, the same idea of using movers is used to measure worker and firm effects (Abowd et al., 1999; Card et al., 2013) and teacher effects (Jackson, 2013; Chetty et al., 2014). The impact of other large changes in the environment on food consumption has been studied, for example, by Dragone and Ziebarth (2017) in German reunification.

the family's alcohol purchases would increase by $27 per quarter. This shift in alcohol purchases could affect wealth, earnings, alcohol abuse, and the overall well-being.

Section 2 describes the data. Section 3 describes the empirical strategy. Section 4 presents our main analysis. Section 5 concludes.

2 Data

NielsenIQ Consumer Panel. We use NielsenIQ Consumer Panel from 2004–2017 to measure household-level alcohol purchases (quantity and expenditures). The panel is representative of the U.S. population. The households in the panel are asked to scan all their grocery purchases, including alcohol. The reliability of the data has been extensively analyzed (Einav et al., 2010; Zhen et al., 2019).

Each year, the households report demographic characteristics, including income, household composition, marital status, employment status, and geographic location. Within household variation in demographic characteristics in the NielsenIQ Consumer Panel has been used widely in the literature, including changes in location (Allcott et al., 2019; Hut, 2020), employment status (Dubé et al., 2018; Hinnosaar, 2018), household composition (Hinnosaar, 2019; Janssen and Parslow, 2021), and income (Dubé et al., 2018; Argente and Lee, 2021).

We think of alcohol purchases as a reasonable measure of alcohol consumption. We acknowledge that some purchases could be used as gifts or consumed by guests. After presenting the main estimates, we will examine the concern that the household does not necessarily consume the alcohol it purchased.

All the purchases data is at the household level, which makes the person-level analysis impossible. Therefore, with a slight abuse of terminology, when talking about individual characteristics, we mean individual household characteristics. Analyzing household-level data misses some person-level effects, such as one household member increasing and the other equally decreasing their consumption. To look at some of the person-level effects, in the robustness analysis, we separately analyze single-adult households.

Sample construction. Our main sample consists of movers. We define a household to be a mover if its state of residence changes once.[4] We exclude from the sample households

[4]We analyze robustness to alternative geographic levels, such as county and 3-digit zip code. We focus on across states moves because a lot of the variation in the alcohol regulation is at the state level, and at more disaggregate levels, data is noisier.

whose state of residence changes more than once. Robustness analysis shows that further restricting the sample to the movers with constant demographic characteristics (employment status, marital status, household size, the number of members aged 21 and above, income, and occupation) does not substantially change the main estimates.

The dataset has information of the year on the move but not the exact time of moving. The geographic location of the stores where movers shop confirms that indeed they change the shopping location during the year of the move (figure A.1 in online appendix). Unsurprisingly, timing is heterogeneous: some movers start shopping in their destination at the beginning of the move year, while others switch later. We drop the year of the move from our main sample to avoid mismeasurement associated with not knowing the exact timing of the move. Dropping the year of the move is not critical for our results. In the robustness analysis, we assume that households move in the quarter with the first shopping trip in the destination during the move year. However, this definition of the move's timing is imprecise because the store location is known for about half of the shopping trips.

Outcome measures. Our main outcome measures are household-level quarterly alcohol purchases per adult: quantity of beer, wine, liquor, the total quantity of pure alcohol, and the total expenditure on alcohol. We calculate the total quantity of pure alcohol from all types of alcohol using the following formula: Q(pure alcohol) = 0.4Q(liquor) + 0.12Q(wine) + 0.045Q(beer). We deflate alcohol expenditures to 2015 dollars using the consumer price index for urban consumers.[5] We calculate alcohol purchases per adult by dividing household purchases by the number of persons aged 21 and above.[6] To analyze alcohol purchases on the extensive margin, we calculate a rolling average measure of whether the household has bought any alcohol in the past year. We construct the extensive margin measure based on a full year instead of a single quarter because consumers might store alcohol and consume it later. Thus a quarter without any alcohol purchases is not sufficient evidence that they have stopped consuming alcohol, they might simply consume what they have stored. While the year of the move is otherwise excluded from the main regressions, these time periods are still used to construct the extensive margin measure. For example, the extensive margin measure in the first quarter after the move year is based on the first quarter after the move year and the last three quarters during the move year.

In our main specification, the outcome variable is the logarithm of quarterly alcohol quantity or expenditures plus one. The results do not depend on the functional form. Robustness

[5]In the same way, we also deflate all measures of income, prices, and taxes.

[6]We drop households from the sample that do not have any members aged 21 or above.

analysis shows that three alternative functional forms (inverse hyperbolic sine transformation, percentile ranks, and actual values) give similar results.

We compute state-level average outcomes using data on non-movers, that is, households whose state of residence does not change. When calculating state-level averages, we first average across households in each time period (calendar quarter) and in each state using sample weights (and information on the number of adults) and then take averages across time periods. To calculate the state-level averages, we use the entire period 2004–2017. State-level alcohol purchases have remained rather stable over time (figure A.3 in the online appendix). As a robustness check, we also re-run the analysis using state-level purchases at the origin state two years before the move year and at the destination state two years after the move year.

Summary statistics. Online appendix A presents summary statistics. Movers compared to non-movers are more likely to have higher income, be college-educated, and women are less likely to be employed (table A.1). Movers are also likely to consume more alcohol. Moves are rather symmetric—moves to states with larger average alcohol purchases are about as likely as moves to states with smaller average alcohol purchases (figure A.2). Moves take place between all regions (table A.2). Movers who move to states with larger average alcohol purchases have relatively larger pre-move alcohol purchases than those who move to states with smaller average purchases (table A.3). In our analysis, these differences among movers will be absorbed by household fixed effects.

3 Empirical strategy

Our empirical strategy decomposes the variation in alcohol purchases to the current environment versus individual factors. In the main specification, we restrict the sample to movers and regress mover's alcohol purchases on the *size of the move* defined as the difference between the average outcome variable (measuring alcohol purchases) in the mover's destination and origin states, and on household and time fixed effects.[7] Specifically, we estimate the following event study regression for mover i in period t:

$$y_{it} = \alpha_i + \tau_t + \sum_{r(i,t)} \theta_{r(i,t)} \cdot \Delta_i + \varepsilon_{it} \tag{1}$$

[7] An alternative strategy (in online appendix B) that instead of the size of the move uses household and state fixed effects and data on both movers and non-movers gives similar estimates.

where the outcome variable y_{it} is a measure of alcohol purchases. All regressions include household fixed effects α_i and time period fixed effects τ_t. Index $r(i,t)$ indicates quarters relative to the move for household i in period t. The first quarter after the year of move in the new state is indexed by 0. The coefficient θ_{-1} on the last quarter in the state of origin is normalized to zero.[8]

The size of the move $\Delta_i = \bar{y}_{D,i} - \bar{y}_{O,i}$ measures the difference between the average alcohol purchases in the mover's destination and origin state. We calculate the difference $\bar{y}_{D,i} - \bar{y}_{O,i}$ based on non-movers in the destination and origin as described in section 2.

The coefficients of interest $\theta_{r(i,t)}$ measure how much individual alcohol purchases change relative to the gap in average purchases between two areas, Δ_i. If, after the move, the mover's alcohol purchases change by the same amount as the gap Δ_i, the coefficient $\theta_{r(i,t)}$ equals one. However, if there is no change in alcohol purchases after the move, the coefficient $\theta_{r(i,t)}$ equals zero. The value of the coefficient measures the fraction of the difference between destination and origin that has been covered. In this way, the size of the jump at the time of move measures the share of the average difference between areas attributable to the current environment (as opposed to individual characteristics).[9] We calculate standard errors clustered at the household level.[10]

In addition to the event-study, we also estimate difference-in-differences regressions. The specification is the same as equation (1), except that all the coefficients post-move $\{\theta_{r(i,t)} : r \geq 0\}$ are collapsed into one and the coefficients pre-move are normalized to zero.

Identification. The identifying assumption is that the trends in movers' purchases are not correlated with the size of the move. The inclusion of household fixed effects would capture any time-invariant differences in alcohol purchases at the household level.[11] A possible concern is that movers to higher (lower) alcohol purchasing states would have increased (decreased) their purchases anyway. For example, the moves and changes in alcohol purchases happen due to getting a new job or unemployment. While in NielsenIQ, we do not know

[8] We exclude the calendar year of the move to avoid mismeasurement due to not observing the exact quarter of the move. By the first quarter in the new state, we mean the first quarter after the year of the move; and by the last quarter in the origin state, we mean the last quarter before the year of the move.

[9] Finkelstein et al. (2016) show formally how the coefficient measures the share of variation explained by the location as opposed to individual characteristics.

[10] The estimates retain their level of statistical significance when clustering at the level of origin-destination states pair.

[11] In the robustness check section, we also include non-movers in the event study analysis. The household fixed effects would also absorb any time-invariant differences between the movers' and non-movers' alcohol purchases.

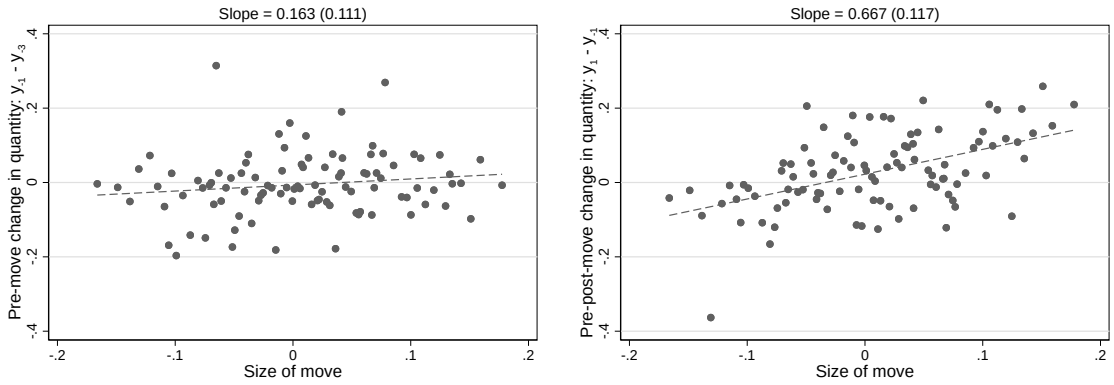
why someone moves. We utilize Annual Social and Economic Supplement (ASEC) of the March Current Population Survey (CPS) from 2013 to 2017. Table 1 summarizes the reasons why people move across the state lines in CPS data, by the type of states they move to. One can see that in general the reasons of moving are similar for those who move to a higher and a lower-consumption state. Therefore, our findings are unlikely to be driven by differential moving reasons between high and lower-consumption state. In addition, we provide evidence that we get similar results when we exclude households whose income, marital status, employment, occupation, household size, or the number of members aged 21 and above changes.

Table 1: Reasons of moving across states

	Move to a higher-alcohol consumption state	Move to a lower-alcohol consumption state	P-values for Col(1)=Col(2)
Panel A: Breakdown of Reasons of Move			
job-related reasons	.49	.47	0.17
family-related reasons	.24	.24	0.87
housing-related reasons	.2	.21	0.33
other reasons	.08	.09	0.23
Panel B: Breakdown of Job-Related Reasons			
new jobs or transfer	.35	.35	0.91
lost jobs or to look for job	.03	.03	0.66
retired	.02	.02	0.28
easier commute	.05	.03	0.00
other job related reasons	.04	.04	0.49
Observations	2596	2435	

Notes: This is imputed using the March Current Population Survey (CPS), 2013-2017. We restrict the sample to those households who move between states between 2013 and 2017, which is roughly 1.5% of the entire CPS sample. Family-related reasons include change of marital status, starting new households, starting new relationship and other family reasons. Job-related reasons include new job or job transfer, looking for a job or lost a job, easier commute, retired and other job-related reasons. Housing-related reasons include want to own home, want a new house/apartment, want to move to better neighborhood, etc. We merge March CPS with the state-average alcohol consumption level from NielsenIQ to impute whether someone moves to a higher-alcohol consumption or a lower-alcohol consumption state.

To provide additional support for the identifying assumption, we analyze whether the pre-move trends in alcohol purchases are correlated with the size of the move. Figure 1a presents binned scatter plots of changes in alcohol purchases over three years before the move by the size of the move. While on the figures by eye-balling, one could detect a slight



(a) Pre-move change: $y_{-1} - y_{-3}$ (b) Pre-post-move change: $y_1 - y_{-1}$

Figure 1: Changes in alcohol purchases by the size of move: pre-move (1a) and from pre- to post-move (1b)

Notes: Each figure presents a binned scatter plot of the change in the logarithm of alcohol purchases (y-axes) on the size of the move $\hat{\Delta}_i$ (x-axes). For each mover we calculate the size of the move Δ_i and group these into percentiles. The x-axes displays the mean Δ_i for movers in each percentile. On figure 1a, the y-axes shows for movers in each percentile the average log purchases in the last calendar year pre-move minus average log purchases in the third calendar year pre-move. On figure 1b, the y-axes shows for movers in each percentile the average log purchases in the first calendar year post-move minus the average log purchases in the last calendar year pre-move. The line of best fit is obtained from OLS regression using the 100 data points (percentiles). Its slope coefficient and standard error (in parentheses) are reported on the graph. Figures 1a–1b use the same sample, which includes all movers that move across state lines, limiting the sample to those that move only once and who are observed continuously from three calendar years before the move to one calendar year after the move (1339 households).

pre-trend, this is not statistically significant at the 10 percent level. In any case, the small magnitude of the statistically insignificant pre-trend is in contrast with the large positive correlation of the size of the move and changes in the three years over the move (figure 1b).

4 Results: Measuring the extent of movers' adjustment

Event study results. Figure 2 presents event study estimates. It shows a sizable jump at the time of the move in the quantity of ethanol purchased. This indicates that the current environment plays a sizable role in affecting alcohol purchases. Figure A.4 in online appendix presents similar results for alcohol expenditures and separately for the quantity of beer, liquor, and wine.

It would be interesting to study even longer-term effects than two years after the move year, but then the sample will get smaller. The event study estimates are based on the

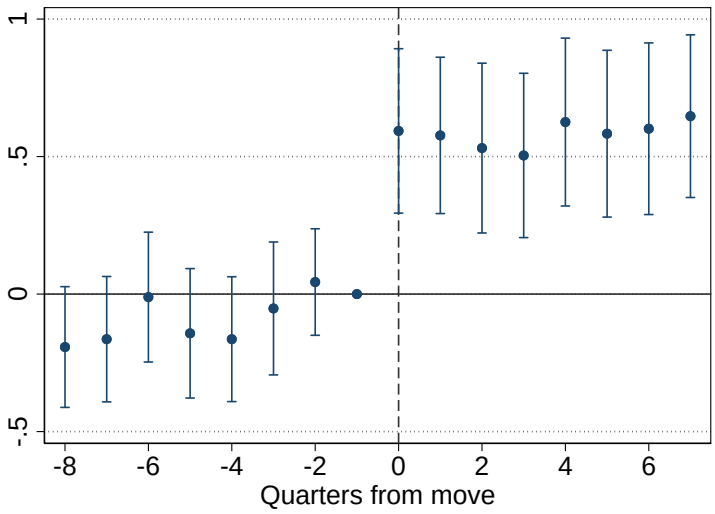


Figure 2: Event study of alcohol purchases (quantity)

Notes: Each figure presents the coefficients $\theta_{r(i,t)}$ (point estimates and 95% confidence intervals) estimated from equation (1). The coefficient for the last time period before the move is normalized to 0. The dependent variable is the logarithm of the quantity of total pure alcohol purchased. The regression includes quarter-year dummies and household fixed effects. Sample includes all movers that move across state lines, limiting the sample to those that move only once and who are observed continuously 2 years before and after the move; the year of the move itself is excluded from the sample; quarters more than 2 years before or after the move are included in the estimation but not shown on the figure (1,379 households and 50,964 observations). Standard errors are clustered at the household level.

sample of households that are observed continuously for five years: two years before and two years after the move year. Few households are in the sample long enough to observe longer-term effects. Figure A.5 in the online appendix presents results for three and four years after the move year. It is reassuring that the estimates in the third and fourth year after the move year also show sizable convergence.

Difference-in-differences results. Panel A of table 2 summarizes the above event-study estimates, re-estimating regression (1) pooling all time periods before the move and all time periods after the move.[12] The change in alcohol purchases after the move equals about 70% of the destination minus origin difference. There is some heterogeneity across the types of alcohol—the adjustment (importance of the current environment) is slightly larger for wine and smaller for liquor.

Is the change in alcohol purchases coming from the intensive or extensive margin? According to the 2017 National Survey on Drug Use and Health, 34% of U.S. adults did not

[12]When restricting time periods to 2 years before and after the move, results remain similar.

Table 2: Change in alcohol purchases after move. Difference-in-differences estimates.

	(1)	(2)	(3)	(4)	(5)
		Quantity			Expend.
	Total	Beer	Liquor	Wine	Total
Panel A: Average effects					
$\Delta\cdot$ After move	0.708***	0.689***	0.608***	0.821***	0.695***
	(0.082)	(0.087)	(0.097)	(0.093)	(0.058)
Households	3267	3267	3267	3267	3267
Observations	97860	97860	97860	97860	97860
		Intensive margin Quantity conditional on purchasing alcohol before move			Extensive margin Purchasing alcohol
	Total	Beer	Liquor	Wine	
Panel B: Intensive and extensive margins, average effects					
$\Delta\cdot$ After move	0.775***	0.771***	0.670***	0.889***	0.500***
	(0.095)	(0.101)	(0.112)	(0.107)	(0.055)
Panel C: Intensive and extensive margins, asymmetric effects					
$\Delta\cdot 1[\Delta>0]\cdot$ After move	0.868***	0.919***	0.855***	1.044***	0.705***
	(0.145)	(0.146)	(0.191)	(0.162)	(0.087)
$\Delta\cdot 1[\Delta<0]\cdot$ After move	0.664***	0.571***	0.486***	0.719***	0.291***
	(0.152)	(0.161)	(0.157)	(0.176)	(0.085)
Wald test, coef. equality, p-value	0.373	0.133	0.170	0.215	0.002
Households	2722	2722	2722	2722	3267
Observations	83596	83596	83596	83596	86112

Notes: Each column-panel combination presents estimates from a separate regression. Dependent variable is logarithm of alcohol purchases or indicator for purchasing alcohol (column 5 in panels B–C). $\Delta_i = \bar{y}_{D,i} - \bar{y}_{O,i}$ is the difference in average logarithm of alcohol purchases between the destination and origin state of the mover; or in column 5 in panels B–C, the difference in the share of households purchasing any alcohol between the destination and origin state. $1[\Delta_i > 0]$ is an indicator for Δ_i being strictly positive, that is, a move to a state with larger average alcohol purchases, and $1[\Delta_i < 0]$ indicates a move to a state with smaller average alcohol purchases. Each regression includes quarter-year fixed effects and household fixed effects. Sample includes all movers that move across state lines, limiting the sample to those that move only once, the year of move is excluded from the sample (3267 households and 97860 observations). In panels B–C columns 1–4, to analyze changes on the intensive margin, the sample is further restricted to households who bought alcohol before the move (2722 households and 83596 observations). In panels B–C column 5, to analyze changes on the extensive margin, the outcome variable indicator for purchasing any alcohol is calculated as a rolling average over four quarters, therefore the number of household-quarters is smaller than in panel A (3267 households and 86112 observations). In panel C, to test whether the effect is asymmetric, p-value of the Wald test for the equality of the two coefficients is included. Standard errors (in parentheses) are clustered at the household level. *** Indicates significance at 1 percent level.

drink alcohol in the past year.[13] Does the move change whether consumers purchase any alcohol (extensive margin)? Or is all the adjustment coming from those who purchased alcohol before the move and now change the quantity? To examine the intensive margin (table

[13]Source: Substance Abuse and Mental Health Services Administration, National Survey on Drug Use and Health, 2017, `https://datafiles.samhsa.gov/`.

2, panel B, columns 1–4), we restrict our sample to those who have purchased alcohol before the move. To analyze the extensive margin (panel B, column 5), the dependent variable indicates whether a household purchased alcohol in the previous 12 months and Δ the difference between the destination and origin in the share of households purchasing alcohol.[14] The results show that movers make large changes in their alcohol purchases both on the intensive and extensive margins.

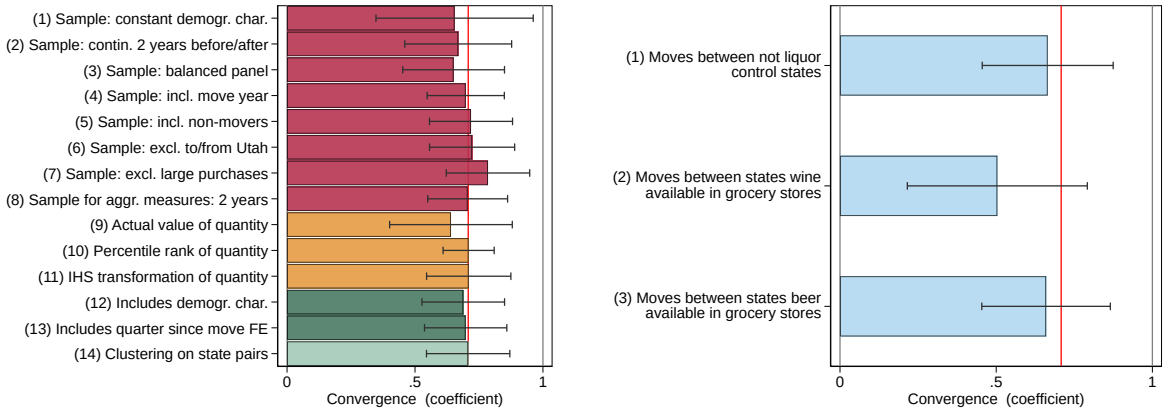
Is the effect asymmetric? It might be easier to adjust alcohol consumption upwards than cut it down. In the extreme, is all the adjustment only upwards? Panel C of table 2 shows that adjustment takes place in both directions. On the intensive margin (columns 1-4), the magnitude of adjusting alcohol quantity upwards versus downwards is not different from each other at 10 percent significance level. On the extensive margin (column 5), the effect is indeed asymmetric. When moving to a state with a larger share of consumers purchasing alcohol ($\Delta > 0$), movers are more likely to adjust upward (start to purchase alcohol), compared to adjusting downward (stopping purchasing alcohol), when moving to a state with a smaller share of consumers purchasing alcohol ($\Delta < 0$). The average Δ in column 5 is about 10%, implying that on average a non-drinker moving from a low alcohol consumption state to a high alcohol consumption state is seven percentage points more likely to purchase alcohol after the move.

Robustness. Below, we summarize the analysis meant to explore the sensitivity of our results to alternative samples, functional forms, controls, clustering, and geographic levels.

Our results are robust to using alternative samples (figure 3a, regressions 1–8). Importantly, the point estimate of the main result remains similar when restricting the sample to movers whose demographic characteristics (employment status, marital status, household size, the number of members aged 21 and above, income, and occupation) remain constant.[15] This helps to alleviate the concern that a move and a change in alcohol purchases are due to the same shock in personal circumstances. Results remain the same when aggregate measures are calculated based on only two years before the move year in the state of origin and two years after the move year in the state of destination. Results are also robust to whether the extensive margin measure is calculated based on a year or half a year (table A.12). The results are not driven by the specific functional form of the outcome variable (figure 3a,

[14] Nearly one-third of the sample do not purchase any alcohol, which matches the national average from the National Survey on Drug Use and Health.

[15] As in the dataset, income is reported in 16 intervals, we consider income to be constant if it remains in the same income interval.



(a) Changing samples, dependent variables and regression specifications

(b) Moves between states with similar alcohol retail conditions

Figure 3: Robustness checks of difference-in-differences estimations

Notes: Each bar presents a point estimate from a separate regression analogous to that in column 1 panel A in table 2. The x-axes measures the estimated extent of changes in alcohol purchases after move ($\Delta\cdot$ After move in table 2). The red vertical line at 0.708 describes the comparison which is the value of the point estimate in the regression in column 1 in table 2. The dependent variable is the logarithm of the quantity of total ethanol purchases, except regressions 9-11 on figure 3a where it is either the actual value of the quantity, percentile rank, or IHS transformation. For percentile ranks, in each time period, all households are ranked by alcohol purchases; each period, household's percentile rank is its position in the national distribution; the area-level change is measured as a change in the area-level average percentile rank. Each regression includes quarter-year fixed effects and household fixed effects. Demographic characteristics (logarithm of income, household size, number of adults aged 21+, marital status, employment status, an indicator for children aged 0-5, and dummies for time period relative to move) are included in regression 12 on figure 3a, and quarter since move fixed effects are also included in 13 on figure 3a. Sample includes all movers that move across the state lines, limiting the sample to those that move only once. The sample excludes the move year, except in regression 4 on figure 3a. On figure 3a, the sample is further restricted to households whose demographic characteristics (employment status, marital status, household size, the number of members aged 21 and above, income, and occupation) remain constant (regression 1); or who are observed continuously two years before and after the move (regression 3); or to the balanced panel two years around the move (regression 4). In regressions 5 and 13, the sample includes non-movers; in regression 6, it excludes movers to and from Utah. In regression 7, the sample excludes days when the household made a large alcohol purchase, that is, purchased either more than a six-pack of beer, more than a typical large bottle of liquor (1.75 liters), or more than four bottles of wine. The state-level average alcohol purchases are calculated using the entire period of 2004–2017, except in regression 8, where state-level purchases at the origin state are calculated two years prior to the move year, and at the destination state, two years after the move year. On figure 3b, the sample is further restricted to moves between states that are not liquor control states (regression 1), moves between states where in the majority of grocery stores wine (regression 2) or beer (regression 3) is available. Capped spikes present the 95% confidence intervals with standard errors clustered at the household level, except for regression 12 where the standard errors are clustered on the origin-destination state pair. Details about the estimates are in tables A.4–A.11 and A.16–A.18 in online appendix A.

regressions 9–11). Instead, the results are similar with three alternatives: when we measure alcohol purchases using the actual value of quantity (instead of the logarithm), inverse hy-

perbolic sine transformation, or percentile rank (as used by Chetty and Hendren (2018a)). Including event time fixed effects or demographic characteristics as controls also does not change the results (figure 3a, regressions 12–13). When including event time fixed effects we use the sample of both movers and non-movers, because if we use only movers, time period fixed effects and event time fixed effects would be perfectly collinear. While in the main analysis, we cluster standard errors at the household level, the calculated standard errors remain similar if clustered at the level of origin-destination states pair (figure 3a, regression 14).

We explore robustness to the geographic area, comparing zip codes, counties, states, and census regions (tables A.13–A.15 in online appendix). Our results remain similar as long as movers cross state lines. For moves inside the state, there is much less convergence towards the destination zip code or county level. Although county- and town-level differences in alcohol regulation exist, we hypothesize that most variation in alcohol purchases are driven by state-level regulation. For within-state moves, there is little, possibly noisily measured, variation between destination and origin.

Heterogeneity by alcohol retail conditions and initial alcohol purchases. A potential concern is whether our estimates are driven by changes in scanning behavior due to the move. If households are more likely to report purchases made in grocery stores than liquor stores, moving to or from a state which sells alcohol in grocery stores could lead to a large change in reported purchases. To check the validity of this concern, we restrict samples to moves between states with similar retail conditions (figure 3b). The estimates are less precise but not considerably different from those obtained from the full sample (table 2 column 1).

We also explore heterogeneity by before-move alcohol purchases (table A.19 in online appendix). The point estimates for moderate and heavy drinkers are similar (about 0.7), but the estimates for heavy drinkers are imprecise (possibly because of the smaller sample size).

Heterogeneity by demographic characteristics and changes in the characteristics. Does the effect of the move differ by demographic characteristics and changes in the characteristics? Figure 4 presents the estimates splitting the sample by education, income, age, the number of adults in the household, and changes in income, job (current occupation and employment), and the number of adults. It shows the convergence in all the groups. Tables A.20–A.22 present asymmetric effects, showing that in all groups, alcohol purchases increase

when moving to states with average higher purchases and decrease when moving to states with lower purchases.

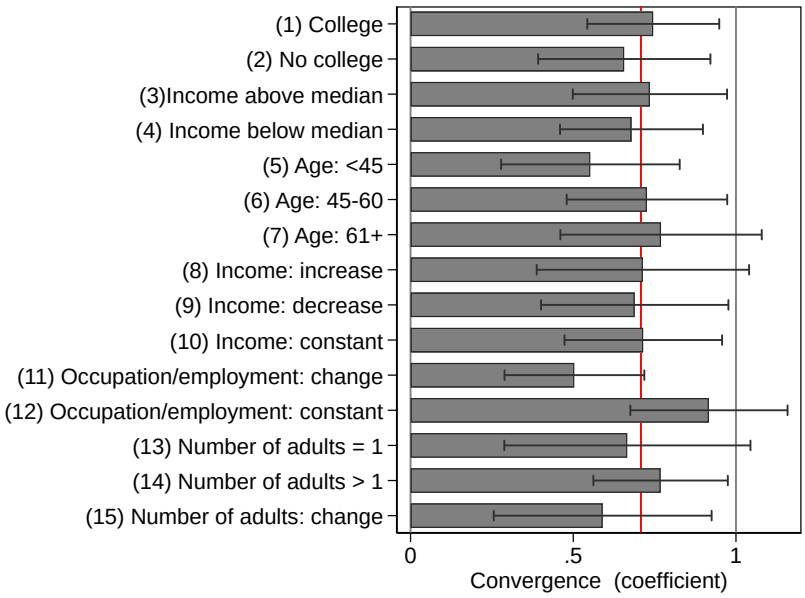


Figure 4: Heterogeneity by demographic characteristics and changes in the characteristics

Notes: Each bar presents a point estimate from a separate regression analogous to that in column 1 panel A in table 2. The x-axes measures the estimated extent of changes in alcohol purchases after move ($\Delta\cdot$ After move in table 2). The red vertical line at 0.708 describes the comparison which is the value of the point estimate in the regression in column 1 in table 2. The dependent variable is the logarithm of the quantity of total ethanol purchases. Each regression includes quarter-year fixed effects and household fixed effects. Sample includes all movers that move across the state lines, limiting the sample to those that move only once. The sample excludes the move year. In each regression the sample is further restricted by the described characteristic. In regressions 1–7, all characteristics are measured in the last year before the move year. College indicates whether either the male or female household head has college education. Age indicates the age of the youngest household head. In regressions 8–15, the comparison is between the first year after the move year and the latest year before the move year. As in the dataset, income is reported in 16 intervals, in regressions 8–10, we consider income to be constant if it is the same income interval, and increasing or decreasing correspondingly otherwise. In regressions 11–12, the sample is split by whether at least one of the household head changed occupation or employment status. In regressions 13–15, the sample is split by the number of adults, and whether the number of adults changed. Capped spikes present the 95% confidence intervals with standard errors clustered at the household level. Details about the estimates are in tables A.20–A.22 in online appendix A.

Off-premise versus on-premise alcohol purchases. We might be worried that we measure only changes in off-premise purchases, and households are substituting to on-premise (bar) purchases. First, the adjustment in off-premise alcohol purchases is interesting in its

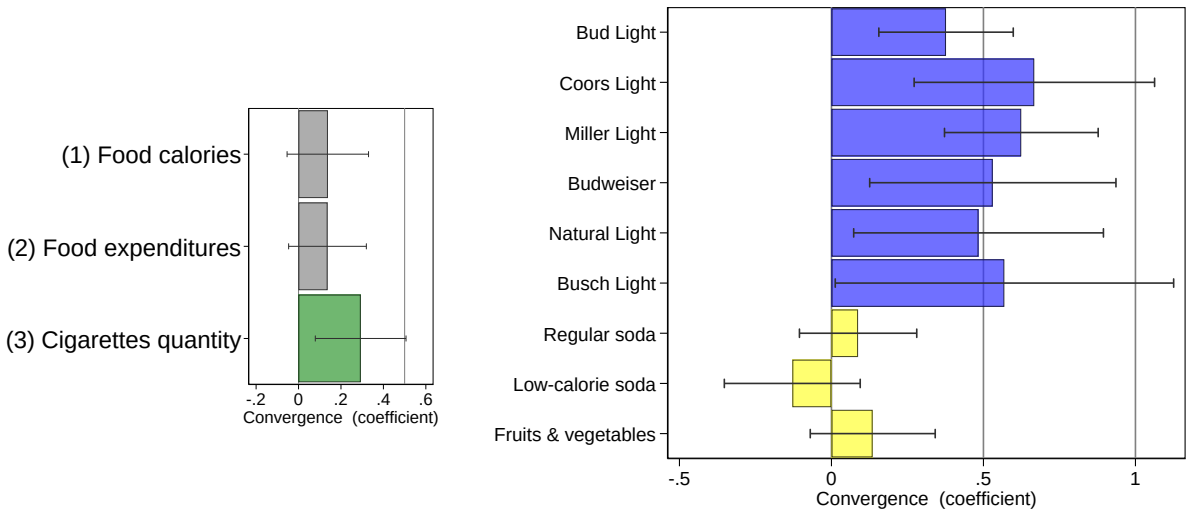
own right. Alcohol consumption in bars is a social activity that is likely to adjust to the level of new peers when moving. An adjustment in off-premise purchases is more surprising.

Second, we explore changes in at-home food purchases. Assuming that on-premise alcohol purchases are related to food consumption outside the home, we expect changes in on-premise alcohol consumption to be reflected in grocery purchases. We estimate the same regression as for alcohol also for grocery purchases (figure 5a). We exclude from the food purchases typical breakfast food products because alcohol is mostly consumed at lunch or dinner times. The adjustment in food purchases is considerably lower than in alcohol. While the estimates are imprecise, they rule out adjustment about half the size of that for alcohol.

Lastly, we compare alcohol purchases from the NielsenIQ dataset to heavy drinking measures from the Behavioral Risk Factor Surveillance System (BRFSS) and chronic liver disease and cirrhosis mortality from the Centers for Disease Control and Prevention (CDC). We find that localities with higher off-premise alcohol purchase in NielsenIQ dataset have higher shares of heavy drinkers in BRFSS (figure A.6) and higher mortality rates related to chronic liver disease (figure A.7).

Cigarette purchases. For robustness, we also analyze convergence in cigarette purchases. Some characteristics tobacco shares with alcohol, while others are different. As in the case of alcohol, tobacco regulation also considerably differs by state. This might lead to large adjustments after the move, but tobacco consumption is likely to be somewhat less social and more addictive than alcohol. That is, if we exclude alcohol abuse cases, then for a typical consumer, addiction is not the main reason for consuming alcohol. Because tobacco consumption is less social and more addictive, the adjustments after the move might be smaller than in the case of alcohol. We estimate the same regression for cigarettes as for alcohol (figure 5a). The estimated convergence is considerably lower than for alcohol (it is significant at the one percent level): 0.29 for cigarettes versus 0.71 for alcohol. It is possible that the convergence is lower because cigarette consumption is more addictive and less social.

Comparison with previous literature. To compare with the literature, we use our estimation method to estimate the importance of the current environment in brand choice as in Bronnenberg et al. (2012) and healthy eating choices as in Allcott et al. (2019). Figure 5b presents the estimates. We focus on the most popular beer brands to compare the results with Bronnenberg et al. (2012). We estimate the average change in alcohol purchases to equal 0.54 for the most popular beer brands, similar to 0.6 across all grocery brands (not only beer) in Bronnenberg et al. (2012). To compare with Allcott et al. (2019), we focus on soda and



(a) Food and cigarettes (b) Beer brands, soda, fruits and vegetables

Figure 5: Change in purchases after the move. Difference-in-differences estimates

Notes: Each bar presents a point estimate from a separate regression, where dependent variable is the logarithm of quantity of purchases. The x-axes measures the estimated extent of changes in alcohol purchases after move ($\Delta\cdot$ After move in table 2). Each regression includes quarter-year dummies and household fixed effects. The sample of 3267 households, includes all movers that move across state lines, limiting the sample to those that move only once, the year of the move is excluded from the sample. In figure 5a, the dependent variable is the logarithm of either calories or expenditures of food, excluding typical breakfast foods, or the logarithm of the quantity of cigarettes. In figure 5b, the dependent variable is either the logarithm of quantity of one of the six most sold brands of beer; or the logarithm of quantity of regular soda, low-calorie soda, or fruits and vegetables. Capped spikes present the 95% confidence intervals with standard errors clustered at the household level. Details about the estimates are in tables A.23–A.25 in online appendix A.

fruits and vegetables, which over- or under-consumption plays a role in the healthiness of diet. Similar to the findings by Allcott et al. (2019), for none of these categories is the estimated convergence statistically significantly different from zero, the average point estimate is 0.03.

Which characteristics are common to high-alcohol-consumption environments? We explore the question in online appendix C. First, using data on both movers and non-movers, we regress alcohol purchases on household, state, and time fixed effects in order to quantify the role of environment in explaining the variation in alcohol purchases. Then, we measure which state-level characteristics are correlated with the estimated state-level location effects. We find that high-alcohol-consumption environments are more likely to have alcohol available in grocery stores and have lower alcohol prices. The correlation does not necessarily describe causal effects and instead could capture endogeneous responses to

voters' and consumers' preferences. This could happen both across states and time. For example, during economic downturns, governments tend to increase alcohol taxes (because other revenue sources have decreased), and alcohol consumption also tends to increase during economic downturns. This positive correlation between taxes and consumption does not mean that higher taxes lead to more drinking. In cross-section, in states with high demand, voters may support lower taxes and fewer restrictions on alcohol availability.

Limitations. While the length of our panel and the identification strategy provide advantages in answering the question in the paper, the analysis also has limitations. Below we address the concerns about underreporting, on-premise purchases, purchases versus consumption, and external validity.

As with any consumption survey data, underreporting is a concern with our dataset. Cook (2007) describes that other survey-based measures of alcohol consumption capture about half the consumption in the alcohol tax data. In our analysis, underreporting is not necessarily a problem, as long as the magnitude of underreporting is not correlated with the direction of the move. We might be worried that underreporting depends on retail conditions, which vary by state. But when we restrict the sample to moves between states with similar retail conditions, movers still adjust their purchases. Furthermore, in the main analysis, we drop the year of the move, which should alleviate the concern that consumers are too busy to report purchases during the move.

Our dataset includes only off-premise alcohol purchases, excluding alcohol consumed in bars and restaurants. First, we argue that any adjustment in off-premise alcohol purchases is surprising and interesting in its own right because compared to on-premise purchases, it is less social. Nevertheless, one might worry that we capture only substitution between off- and on-premise purchases. To alleviate the concern, we provide three pieces of evidence. First, off-premise alcohol purchases are likely to change when there are other changes in personal circumstances. But our main results remain similar when we limit the sample to movers whose demographic characteristics (employment status, marital status, household size, the number of members aged 21 and above, income, and occupation) stay constant. Second, suppose alcohol purchases in bars tends to increase after moving because movers want to get to know new colleagues, or decrease because they don't yet know anyone. In both cases, if that is reflected in off-premise purchases, we would observe a temporary short-term increase/decrease immediately after the move. Instead, we observe a lasting impact over two years (in the event study framework), suggesting these temporary effects are not

driving our results. Third, changes in off-premise versus on-premise alcohol consumption are likely reflected in food consumption at home. But we find no evidence of large adjustments of food purchases after the move.

Could the alcohol purchases be for someone else (as gifts or for a party) instead of own consumption? Since we only observe purchases instead of consumption, we cannot know who consumed it. Suppose households moving to regions where alcohol consumption is high after the move more often bring alcohol as a gift to a dinner party. While those moving to regions where alcohol consumption is low less often choose alcohol as a gift to bring to a party. If alcohol is brought to a party, most likely, the household itself will consume alcohol at the party. In this way, gifts may reflect the household's own consumption. One might also be concerned that households host more parties in regions where average alcohol consumption is high and fewer parties in regions with low average alcohol consumption. Since parties tend to involve some food, we would expect corresponding changes in food consumption, but we show that food purchases do not change in this way (table A.26). One might still be concerned that households moving to regions where alcohol consumption is high after the move could start to host more parties with alcohol, while those moving to regions where alcohol consumption is low could start to host fewer parties with alcohol. To alleviate the concern, we re-estimated the regressions while excluding from the analysis days when the household made a large alcohol purchase. The estimated convergence (figure 3a and table A.7) is only slightly higher compared to our main estimates (0.79 versus 0.71).

Finally, do our findings based on movers extend to the general population? First, we argue that movers are interesting in their own right because their share in the population is large. Each year about 2% of the U.S. population moves across the state lines,[16] and more than 30% of the U.S. population has moved across the state lines in their lifetime (Molloy et al., 2011). Second, if movers are more likely to adjust to a new environment, then our estimates give an upper bound of how malleable alcohol consumption is. Furthermore, our results show that movers are not particularly open-minded regarding all products. Using the same identification strategy, we find no evidence of large adjustment in purchases of fruits and vegetables, regular soda and diet soda, and total food purchases. Similarly, Allcott et al. (2019) and Hut (2020) find little change in movers' healthiness of food purchases. This suggests that alcohol is a particular type of product (like brands studied by Bronnenberg et al. (2012)) for which the current environment makes a large difference.

[16]In 2010-2019, the average annual interstate migration rate equals 2.3 according to the American Community Survey 1-year estimates: `https://www.census.gov/data/tables/time-series/demo/geographic-mobility/state-to-state-migration.html`.

5 Conclusion

Analyzing the purchases of households that move across states, we find robust evidence that alcohol purchases are strongly affected by the current environment. About two-thirds of geographic variation in alcohol purchases is due to the current environment instead of individual characteristics. This finding suggests that government policies and regulations that target the drinking environment (e.g. alcohol availability) could considerably impact the amount of alcohol consumed.

While we quantify the overall importance of the environment, our analysis does not determine the main environmental factors that increase alcohol consumption. We provide reduced form suggestive evidence that high-alcohol-consumption environments have higher alcohol availability in grocery stores and lower prices. To identify the separate impact of various factors, a more fruitful way is to examine the effect of exogeneous changes in regulations and taxes, as is done in recent research (Marcus and Siedler, 2015; Bernheim et al., 2016; Illanes and Moshary, 2018; Miravete et al., 2019; Seo, 2019; Kueng and Yakovlev, 2021).

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