The Rise of Niche Consumption

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Online Appendix
July 2019

Appendix A. Detailed Data Description

Our primary data set is the AC Nielsen Homescan data, which we use to measure household-level shopping behavior.\(^1\) As discussed in the text, our panel contains weekly household-level product spending for the period 2004-2016. The panel has large coverage, with roughly 170,000 households in over 22,000 zip codes recording prices for almost 700 million unique transactions covering a large fraction of non-service retail spending. Roughly half of expenditures are in grocery stores, a third of expenditures are in discount/warehouse club stores, and the remaining expenditures are split among smaller categories such as pet stores, liquor stores, and electronics stores.

While panelists are not paid, Nielsen provides incentives such as sweepstakes to elicit accurate reporting and reduce panel attrition. Projection weights are provided to make the sample representative of the overall U.S. population.\(^2\) A broad set of demographic information is collected, including age, education, employment, marital status, and type of residence. Nielsen maintains a purchasing threshold that must be met over a 12-month period in order to eliminate households that report only a small fraction of their expenditures. The annual attrition rate of panelists is roughly 20 percent, and new households are regularly added to the sample to replace exiting households.

Households report detailed information about their shopping trips using a barcode scanning device provided by Nielsen. After a shopping trip, households enter information including the date and store location and scan the barcodes of all purchased items. Products are allocated by Nielsen into three levels of category aggregation: roughly 1304 "product modules", 118 "product groups" and 11 "department codes". For example, "vegetables - peas - frozen" are a typical product module within the "vegetables - frozen" product group within the "frozen foods" department, and "fabric softeners-liquid" is a typical product module within the "laundry supplies" product group within the "non-food grocery" department.

In our baseline analysis, we define a product as a UPC. UPCs are directly assigned by the manufacturer and will typically change any time there is any change in product characteristics. However,\(^1\) These data are available for academic research through a partnership with the Kilts Center at the University of Chicago, Booth School of Business. See http://research.chicagobooth.edu/nielsen for more details on the data.

\(^2\) We use these projection weights in all reported results, but our results are similar when weighting households equally.
we also compute results instead defining a product as a "brand". Information on brands is constructed by Kilts/Nielsen and is more aggregated than UPCs but still very disaggregated: for example, "Pepsi" and "Caffeine Free - Pepsi" are two different brands, as are "Pepsi" and "Mountain Dew", despite the latter being produced by the same parent company. However, different flavors of Pepsi are typically all listed under the same Pepsi brand. We focus on UPCs as our baseline product definition for several reasons: 1) Most importantly, UPCs are directly assigned by the manufacturer, while the brand variable is constructed by Kilts/Nielsen. Which UPCs are grouped into more aggregate brands involves some subjective judgment, and this aggregation is not necessarily consistent across categories or time. 2) UPCs are the most fine-grained definition available and will capture relevant product changes like the introduction of new flavors which will typically not be captured with the brand-definition. 3) In order to preserve anonymity of the stores in the Nielsen sample, all generic UPCs are assigned the same brand code. This means that analysis of brand-level spending can only be done on the subset of name-brand products and must exclude the large and growing share of generic products from the sample. (see e.g. Dube, Hitsch, and Rossi (2018)).

However, there is legitimate concern that UPCs may be too fine a notion of product when considering the concentration of household purchases, since households may view certain UPCs (for example minor differences in size or packaging for otherwise equivalent UPCs) as identical products. For this reason, we show robustness to instead defining a product as a brand rather than a UPC.

Our baseline analysis focuses on annual spending and computes household market shares across products within product groups, but all results are robust to calculating household product market shares in more disaggregated product modules or more aggregated department codes. There is substantial heterogeneity across product modules in the degree of household concentration, so our analysis focuses on a set of balanced product modules. This eliminates spurious changes in concentration which might otherwise arise from changes in the set of goods sampled by Nielsen (which do not represent real changes in household’s actual consumption and instead merely changes in the categories of consumption reported in Nielsen). This focus on balanced product modules reduces our sample from 118 to 107 product groups. Our analysis excludes fresh produce and other "magnet" items without barcodes since products in these categories cannot be uniquely identified and products with identical product codes in these categories can potentially differ substantially in quality. Our baseline sample includes all households and weights each household using sampling weights provided by Nielsen which are designed to make the Nielsen demographically representative of the broader U.S. population. Appendix Figure A2 shows that aggregate spending growth in our sample tracks government data on aggregate spending growth in comparable categories. Our conclusions are even stronger when instead using a balanced panel of households to eliminate household composition changes.

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3It is not clear that we want to classify a switch from spending $10 on Brand-X 64 oz laundry detergent and $10 Brand-X 60 oz laundry detergent to instead spending $20 on Brand-X 64 oz laundry detergent as a large increase in concentration. If UPCs become more homogeneous across time, using UPCs as our notion of product may lead to spurious changes in concentration with no substantive change in household behavior.
While our baseline sample includes all UPCs, we also show that our results hold when excluding generic/private-label products. In order to preserve anonymity of the stores in the Nielsen sample, the exact identity of generic brands in the Nielsen data is masked. There has been an increase in the private label share of all purchases over the last decade (see e.g. Dube, Hitsch, and Rossi (2018)) so including generic spending which cannot be properly allocated to constituent UPCs might lead to spurious concentration trends. However, we show that excluding generics and calculating concentration trends for branded products produces nearly identical results.

Finally, it is also useful to discuss the potential role of online shopping for our measurement. Households in the Nielsen Homescan sample are supposed to scan barcoded purchases of purchases from online retailers in addition to the items they scan from brick-and-mortar retailers. Indeed the Nielsen panel shows a growing share of online spending across time (Figure A1). However, for the categories covered in Nielsen data, online spending is relatively unimportant, so even by the end of the sample these spending shares remain low.4. Breaking results out further for particular categories where online spending is likely to be more and less relevant delivers no obvious interaction with concentration trends. For these reasons, we conclude that online shopping is unlikely to be of direct importance for understanding the diverging trends that we document.

Figure A1: Online Spending Shares

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4Online vs. brick-and-mortar spending is classified at the level of the retail chain. This means that our measure captures spending at online only retailers such as Amazon but misses online spending associated with traditional retailers such as spending at Walmart.com.
Appendix B. Additional Empirical Results

Figure A2: Household Spending in Nielsen vs. Consumer Expenditure Survey
Figure A3: Concentration Trends: Excluding Generics

Figure A4: Concentration Trends: Including Category Composition Changes
Figure A5: Concentration Trends: Brand Instead of UPC

Figure A6: Concentration Trends: Product Module instead of Group
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(a): Household Shares on Top Products

(b): Aggregate Shares on Top Products

Figure A8: Concentration Trends for Different Samples

(a): Household Herfindahl

(b): Aggregate Herfindahl

Figure A9: 2004-2016 Concentration Growth by Household Size

(a): Household Concentration

(b): Aggregate Concentration
Figure A10: 2004-2016 Concentration Growth Within Location

(a): Household Concentration

(b): Aggregate Concentration

Figure A11: 2004-2016 Concentration Growth Within Retailer

(a): Household Concentration

(b): Aggregate Concentration
Figure A12: Intensive Margin P v. Q effects for UPCs

Figure A13: Effects of $\bar{N}$ ↑ and $F$ ↑ on Profit Distribution
Figure A14: 2004-2016 Concentration growth for continuing vs. all products (brands)

(a): Household Herfindahl

(b): Aggregate Herfindahl
Table A1: Effect of Demographics on Household Concentration Trends

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**Note:** Table shows results from a regression of household herfindahls on various demographic variables and a time trend. Omitted categorical variables are household size 1, age<=29, and income<$20000. In column (5) additional controls are: dummy variables for education, employment status, occupation, scantrack markets, marital status, type of residence, race, presence of children, presence of household internet, cable/non-cable tv, and various indicators for the presence of major kitchen appliances. The unit of observation is a household-year, and observations are weighted using Nielsen sampling weights. Standard errors shown in parantheses are clustered by household. Significance levels: * (p<0.10), ** (p<0.05), *** (p<0.01).
Appendix C. Relationship to External Data

C.1 External Spending Data

Figure A2 shows that aggregate Nielsen spending lines up well with spending growth measures from the Consumer Expenditure Survey and BEA national accounts for similar categories.\(^5\)

However within-household spending growth is substantially less strong than overall household spending. This is likely driven by two forces: 1) The panel dimension of Nielsen is not representative of all households. The continuing households in the sample are substantially older than the overall Nielsen sample and the overall population, and we know from other research that households around retirement have declining food spending. While Nielsen provides sampling weights to make the overall sample representative of the U.S., they do not provide weights to make the panel dimension representative of the overall US, and the requisite demographic variables in the data to construct them ourselves do not exist. 2) There is likely attrition bias and households probably report a declining share of spending across time. This attrition bias may be particularly strong in the final year in which a household is in the sample, which could potentially explain the difference between the fully balanced and within-household spending growth patterns. If reduced reporting tends to proceed exit, then one would expect attrition bias to be less severe for households who remain in the sample for the full 12 years. Consistent with this, the balanced sample exhibits stronger spending growth than the within household sample.

For these reasons, our baseline results use the entire Nielsen homescan panel rather than focusing on a balanced panel of households. However, it is useful to compare our basic trends in the full sample to those computed using within-household variation. Figure A8 thus redoes Figure 1 using a fully balanced panel and with a specification using only the within-household changes specification.

Clearly trends are even stronger than our baseline results when using the fully balanced panel or when identifying off of within-household variation, so in this sense our baseline is conservative. We now describe several forces that might spuriously increase the within-household trend as well as some alternative forces which might spuriously flatten the full sample trend. This makes it difficult to know whether our baseline sample is likely to be understated or whether it is instead the balanced panel specification that is overstated. However, in either case, the trend is robustly positive, and our baseline sample is the one which generates more conservative results.

More specifically, the full sample trend could potentially be biased downwards because the Nielsen sampling technology changes across time, and these changes are implemented when households enter the sample. These changes in technology could obscure underlying trends in the data, but would be

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\(^5\)It is well-known that the consumer expenditure captures a lower level of spending than the BEA and this “missing spending” has a positive trend. However, this growth in missing spending mostly occurs prior to our sample period. Throughout our sample period, the CEX captures a relatively constant share of aggregate spending. This means CEX spending growth is slightly lower but broadly similar to aggregate spending growth from the BEA.
stripped out when using within-household variation. More generally, households have very different concentration levels, as shown above, so that random household entry and exit in the sample could make it more difficult to pick up underlying trends. These are both forces that might lead our baseline full sample to understate the true increase in concentration across time.

Conversely, we have shown above both that increases in spending are strongly negatively correlated with increases in concentration and that the within-household sample has spending growth much lower than in the consume expenditure survey. To the extent that the within-household sample has spurious declining spending due to sample attrition, there is then a concern that using within household variation might lead to an upward biased trend. However, if we redo all our regression results using within household variation controlling for within household changes in spending, we continue to find upward trends which are stronger than in the full sample. This suggests that the stronger upward trend in the within-household results is not driven solely by the lower reported spending growth in this sample. In addition, we can also recompute results using only households in the first year in the sample. By construction, attrition bias in spending across time cannot drive any trend, since this sample has no within-household time-series variation but it still delivers an upward trend. Finally, attrition bias is less likely to be a concern for the fully balanced sample: The upward trend in the fully balanced panel is roughly linear across time, so if this upward trend was explained by attrition bias and progressive under reporting, this under reporting would need to grow at a constant rate, which seems unlikely, especially because Nielsen tries to drop households from the sample who are not reporting accurately. It seems much more likely that the biggest under reporting would occur in the first year or two in the panel as households are likely to be most enthusiastic about scanning purchases initially and then reduce scanning as it becomes more tedious across time. It would be quite surprising if enthusiasm waned at a constant linear rate across time but that households continued to participate in the homescan panel.

Together, we think that these results suggest the stronger upward trends using the balanced samples and the within-sample variation are not driven by spurious attrition bias. Nevertheless, we cannot fully rule out this concern. Furthermore, as discussed above the panel element of the sample is not representative since households who remain in the sample for progressive years are demographically different and not representative of the population leading total spending for this population to line up less well with aggregate spending inferred from the consumer expenditure survey. For these reasons and to be conservative, we focus on the full sample in all our baseline results but only note here that using other samples only strengthens our conclusions.

C.2 Census Concentration of Production

A large and growing literature uses production data from the Census to show that the concentration of production has been broadly increasing from 1982-2012. For example, Autor, Dorn, Katz, Patterson,
and Reenen (2017) calculates industry concentration within 4-digit industries, and averages this up to 6 major sectors and shows that various concentration measures have all increased when comparing 1982 to 2012. In this section we explore the relationship between the concentration measures in our paper and this large literature and argue that relevant comparisons from Nielsen data are broadly consistent with this Census based literature.

First, it is important to note that the concentration notions we emphasize in our paper are conceptually distinct along a number of important dimensions from the concentration of firms or establishments studied using census data. Most importantly, we are measuring the concentration of spending over very detailed UPCs (or slightly coarser but still highly disaggregated brands). This is a fundamentally much more disaggregated notion of concentration than that studied with production data, since firms can potentially produce tens, hundreds or even thousands of different products. For example, in our data Procter and Gamble produces over 40,000 unique UPCs, L’Oreal produces over 28,000 UPCs and General Mills, Unilever, and Kraft Heinz all produce 10,000-20,000 UPCs.6

Furthermore, the categories within which we calculate concentration are also more disaggregated than those in typical Census-based calculations and also cover a more narrow subset of production. For example, the broad manufacturing sector in Autor, Dorn, Katz, Patterson, and Reenen (2017) covers 86 4-digit industries within which concentration is computed. However, of these 86 industries only a small subset produce in categories which are covered by Nielsen (for example NAICS Code 3111 "Animal Food Manufacturing") while most are in production industries which have no overlap with Nielsen categories (for example NAICS Code 3336 "Engine, turbine, and power transmission equipment manufacturing" or NAICS Code 3365 "Railroad rolling stock manufacturing").

Finally, it is important to note that our sample covers the period 2004-2016 while census data starts in 1982 and is last available in 2012. The exact timing of concentration trends in Census data varies substantially, with many sectors exhibiting increases primarily in the period prior to our sample period.

Since they are conceptually different notions, this means the aggregate product concentration trends which we emphasize in the body of the paper should not be directly compared to production concentration trends in Census. However, we can construct concentration measures using the Nielsen data which are more comparable with Census calculations and that can be used to explore the external validity of our data. We now explore these comparisons.

Since households in the Nielsen sample report the retail chain in which they shop, we can aggregate up total spending to compute a Nielsen based measure of spending at each retail chain and resulting retailer concentration. This can then be compared to the concentration of retail trade in Census data. Specifically, since the Nielsen sample is focused on grocery and drug store spending, in the Census we use firm concentration numbers only from NAICS Code 445 "Food and beverage stores" and 446

6It is also worth noting that our “household” concentration measures have no analogue in the Census literature even if we were measuring producer rather than product concentration.
"Health and personal care stores" and weight the publicly available Census concentration numbers for these two sectors using their relative share of sales. This clearly does not provide a precise match between the retail establishments covered in Nielsen and Census so we should not expect numbers to line up exactly, but Figure A15 shows that that Nielsen data broadly matches the level of retail spending accounted for by the Top 4, Top 20 and Top 50 firms as well as the upward trend in retail concentration.

Figure A15: Retail Trade Concentration

We can also perform a similar exercise by allocating UPC-level spending up to the manufacturer. When manufacturers produce a new product, it is assigned a barcode by the company GS1, which then maintains a database which can be used to link UPCs to manufacturers. This lets us aggregate product spending up to a measure of manufacturer spending, with two important caveats:

First, the link from UPCs to parent companies is sometimes inconsistent. For example, Gillette and Old Spice were both acquired in the past by Proctor and Gamble, and the UPCs for Gillette and Old Spice products both map to Proctor and Gamble. However, Ben and Jerry’s was acquired by Unilever in 2000, yet UPCs for these products are assigned to the "Ben and Jerry’s Homemade Inc" firm name rather than to the Unilever parent company. Similarly, Goose Island Beer UPCs are assigned to "Goose Island Beer Company" even though this firm was acquired by InBev in 2011. To the extent that some UPCs are assigned to subsidiaries rather than parent companies, our Nielsen based measure of manufacturer concentration will be biased downwards.

Second, UPCs for store-brand products map to the retailer rather than the actual manufacturer of
the product. For example, Costco’s "Kirkland" store-brand barcodes all map to "Costco", even though Costco does not actually produce most of these products. Although sometimes the actual producer can be identified (for example Kirkland Coffees are advertised as being roasted by Starbucks), this information is typically a trade-secret. This means that we cannot measure the producer for most generic products, and as a result we must drop these products when aggregating up UPCs to manufacturers and focus only on branded products. To the extent that the production of generic products is proportional to the production of branded products, this will have no effect on concentration. However, it is likely that generic products are disproportionately produced by larger manufacturers, so dropping generic products is likely a second force that will bias our Nielsen based measures of manufacturer concentration downwards.

To again focus the comparisons on the most relevant producers, we keep NAICS codes 311 and 312 "Food Manufacturing" and "Beverage and Tobacco Product Manufacturing" from the Census data and weight these concentration measures by their relative sales shares. Figure A16 shows that despite the above concerns, Nielsen data again broadly matches Census data, producing similar levels of manufacturer concentration and a flat to mild downward trend.

Figure A16: Manufacturer Concentration

Overall the results in these two subsections give us confidence that the Nielsen data is largely in line with external evidence on aggregate spending and with Census data on producer concentration.
Appendix D. Robustness of Model Inference to Alternative Trends

In this appendix, we show that the need for increasing $\tilde{\alpha}$ to fit the rise in niche product consumption is very robust and does not depend importantly on the exact strength of this phenomenon in the data.

Given data on $\Omega$, $H^{HH}$, and $HH^{Agg}$, one can immediately solve for $\tilde{\alpha}$ at each date. Doing so implies that in order to rationalize the data, $\tilde{\alpha}$ must rise by 67.4%. Thus, when viewed through the lens of our model, the data can only be rationalized with a large increase in $\tilde{\alpha}$.

But how strong/robust is this conclusion? For example, if we had slightly different trends (or potentially some measurement error in trends), could we have reached a different conclusion? To show that the need for increasing $\tilde{\alpha}$ is a very robust qualitative conclusion that is not particularly sensitive to the exact empirical trends, suppose that we knew only that $\Delta H^{HH} \geq 0$ and $\Delta \Omega \leq 0$, as in our data, but we knew nothing about the strength of the trend. What decline in $HH^{Agg}$, if any, could be rationalized in a constant $\tilde{\alpha}$ environment if we only knew this weaker condition?

Theorem 1 Assume that $\Delta H^{HH} \geq 0$ and $\Delta \Omega \leq 0$. Then given initial $0 \leq \eta_0 \leq 1$, the maximum possible percentage decline in the aggregate Herfindahl without an increase in $\tilde{\alpha}$ is $\max \left( 0, \frac{\eta_0}{\eta_0 + 1} \right)$. This is equal to 0 for $\eta_0 < 1/4$ and the minimum over all $\eta_0$ is $2/3 \sqrt{2} - 1 \approx -5.72\%$.

Proof. We now compute the maximum possible percentage decline in $HH^{Agg}$ that can be achieved without increasing $\tilde{\alpha}$.

First, note that the maximum decline will be obtained when $\Delta H^{HH} = 0$, since if $\Delta H^{HH}$ were strictly positive, one could always find a different pair of $\eta$ and $\Omega$ that satisfy $\Delta H^{HH} \geq 0$ and $\Delta \Omega \leq 0$ but with lower $HH^{Agg}$. This means that we can solve for $\Omega$ as a function of $\eta$ and the initial household herfindahl: $H_0^{HH}$:

$$\Omega = \frac{(\eta + 1)^2}{4\eta} \frac{1}{H_0^{HH}}. \quad (A1)$$

This relationship must hold for any possible pair of $\Omega$ and $\eta$. We label the initial values as $\Omega_0$ and $\eta_0$, and the new values as $\Omega_1$ and $\eta_1$

Since this is a declining function of $\eta$, $\Omega_1 \leq \Omega_0 \implies \eta_1 \geq \eta_0$.

We thus simply want to compute the minimum value of $HH_1^{Agg} / HH_0^{Agg}$ subject to $\eta_1 \geq \eta_0$ and A1.

$$\frac{HH_1^{Agg}}{HH_0^{Agg}} = \frac{\frac{(\eta_1 + 1)}{2\eta_1 + 1}}{\frac{(\eta_0 + 1)}{2\eta_0 + 1}} \left( \frac{\Omega_0}{\Omega_1} \right)^{\frac{1}{2}} \quad (A2)$$

$$= \frac{\frac{(\eta_1 + 1)}{2\eta_1 + 1}}{\frac{(\eta_0 + 1)}{2\eta_0 + 1}} \left( \frac{\eta_0^2}{4\eta_0} \right)^{\frac{1}{2}} \quad (A3)$$
\[ \frac{(\eta_1)^{\frac{1}{2}}}{2\eta_1 + 1} \text{ subject to } \eta_1 \geq \eta_0 \] 

(A5)

\[ \frac{(\eta_1)^{\frac{1}{2}}}{2\eta_1 + 1} \] is increasing with \( \eta_1 \) for \( \eta_1 < \frac{1}{2} \) and decreasing for \( \eta_1 > \frac{1}{2} \). This implies that \( \eta_1 = 1 \) will be a local minimum. There will be a second local minimum \( \eta_1 = \eta_0 \) if \( \eta_0 < 1/2 \). This means that for \( \eta_0 > 1/2 \) we know that the minimum is achieved by setting \( \eta_1 = 1 \), and

\[ \frac{HH_{1^{Agg}}}{HH_{0^{Agg}}} = \frac{2\eta_0 + 11}{(\eta_0)^{\frac{1}{2}}} \] 

(A6)

For \( \eta_0 < 1/2 \) the other local minimum implies that

\[ \frac{HH_{1^{Agg}}}{HH_{0^{Agg}}} = 1, \] 

(A7)

so we just need to solve for where

\[ 1 \leq \frac{2\eta_0 + 11}{(\eta_0)^{\frac{1}{2}}} \] 

(A8)

Solving this equation implies that for it is satisfied if \( \eta_0 < 1/4 \). What does this imply? It means for \( \eta_0 < 1/4 \): \( \Delta HH^{HH} \geq 0 \) and \( \Delta \Omega \leq 0 \) are inconsistent with any decline in \( HH^{Agg} \) when holding \( \tilde{\alpha} \) fixed.

Note that we can also write \( \eta_0 \) directly in terms of initial period observables, which makes it easier to interpret. From the equation for the household herfindahl, \( \eta_0 < 1/4 \) implies that

\[ H_0^{HH} \Omega_0 \geq \frac{25}{16}. \] 

(A9)

So if \( H_0^{HH} \Omega_0 \) satisfies this condition, it is not possible to increase household herfindahl, decrease aggregate herfindahl and household varieties without raising \( \tilde{\alpha} \). For our data, \( H_0^{HH} \Omega_0 = 4.24 \), so we easily satisfy this condition and it would take very different empirical values (a roughly 300% lower \( H_0^{HH} \) or \( \Omega_0 \)) to violate this constraint and even be in a region of the parameter space where it’s possible to get a decline in aggregate herfindahls. But even if we are in the region \( \eta_0 > 1/4 \), it is possible for
$HH^{Agg}$ to decline, but we have a strict bound on the maximum possible decline. In particular, the max possible percentage decline for a given $\eta_0$ is simply given by

$$\max \left( 0, \frac{2\eta_0 + 1}{3} - 1 \right).$$

(A10)

This is minimized at $\eta_0 = 1/2$ and implies a max possible decline across all possible parameters of $\frac{2}{3}\sqrt{2} - 1 \approx -5.72\%$, which is much smaller than the observed percentage decline of -19.56\% (and $\eta_0$ is totally inconsistent with the micro data; but even if willing to go to that unrealistic parameter, then still can’t make things work without $\tilde{a}$).

Finally, note that we get these bounds when imposing non-negative growth in the household herfindahl. If we used the actual value imposed in the data, we would get even stronger bounds on the feasible decline in aggregate herfindahl without a change in $\tilde{a}$. So it is essentially impossible to get declines in the aggregate herfindahl when household concentration is rising and household varieties are falling, without an increase in $\tilde{a}$. 

\[\blacksquare\]
Appendix E. Model Simulation Results

In this section, we explore numerical simulations of our model to test the validity of our elasticity approximation as well as to explore how restrictive the assumption of a stable distribution of Pareto taste-adjusted prices is for our conclusion.

We simulate a discrete approximation to the main model in the paper by drawing a large random vector $\tilde{\gamma}_{\text{rand}}$ of price-adjusted tastes from a Pareto distribution for a large sample of households, using the same parameters as our baseline model. While $N = 15000$, our baseline random sample uses 2.25 million draws for each of 20,000 households since we are trying to approximate a continuum of products from $[0,N]$. However, rather than using analytical formulas to calculate household market shares, for each household we instead keep the random set $\Omega = \tilde{\gamma}_{\text{rand}} > \tilde{\gamma}^*$ and then calculate a numerical price index directly from $P = \left(\int_{k \in \Omega} (\tilde{\gamma}_{i,k} )^{\sigma-1} dk \right)^{\frac{1}{1-\sigma}}$ and then compute market shares from Equation 11. These formulas hold for arbitrary distributions of taste, so even though we still simulate the taste draws from a Pareto distribution, in this simulation we are using no analytical results that rely on this assumption, which also means that we can also perform a similar procedure even if tastes do not follow a Pareto distribution.

In order to get aggregate market shares, we must identify the particular products that each household consumes. In order to do so, we use our rank function Equation 13 with a random uniform draw to compute for each household, the aggregate ranking of each of the 2.25 million possible products in $[0,N]$ and then compute household $i$'s particular idiosyncratic rank for each of the 2.25 million $j$ products. We then sort $\tilde{\gamma}_{\text{rand}}$ and map the highest value to $r_{ij} = 0$, the second highest value to $r_{ij} = 1$ and so on to $r_{ij} = 2.25$ million. Finally, since for each $r_{ij}$, we know the value of $j$, this means that we then know household $i$'s taste draw and resulting individual spending for each aggregate product $j$. For example, the households highest $\tilde{\gamma}_{\text{rand}}$ draw will always map to their $r_{ij} = 0$, but the corresponding aggregate $j$ which household 1 ranks highest might be $j = 0$, the $j$ which household 2 ranks highest might be $j = 2043$, and the $j$ which household 3 ranks highest might be $j = 17$. Once we have these household specific spending shares for each product $j$, we can then numerically add up total spending on each product $j$ to calculate aggregate market shares.

Since these are computed entirely numerically, they do not rely on any of our closed form solutions for aggregate market shares and are thus again valid even under departures from the Pareto distribution. As we note in 4.7, our analytical market shares are only valid under the Pareto distribution so we must approximate the elasticity of demand by modeling a price change as a switch with another product in the aggregate ranking. Since these numerical results do not rely on the Pareto distribution, we can use this numerical model to simulate the aggregate elasticity of demand and resulting markup for a product $j$ by just raising all households’ random taste draw for that product by a small amount. Note that calculating elasticities for each $j$ requires re-simulating a new set of aggregate market shares.
For these sample sizes, computing an elasticity for a single \( j \) requires roughly 2 hours of computational time, so it is infeasible to simulate the elasticity of demand for all 2.5 million products. Instead, we compute the elasticity of demand and implied markups for 50 different values of \( j \) distributed throughout the product space. Figure A17 compares this simulated markup to our analytical approximation and shows that the analytical approach produces essentially identical results (noting that there is still obvious numerical simulation error even with these large sample sizes).

**Figure A17: Simulated vs. Analytical Approximation for Markup**

As stressed throughout the paper, our analytical derivations and implications of changes in \( N \) are only valid under the assumption that the distribution of price-adjusted tastes continues to follow a Pareto distribution as we vary \( N \). If markups were fixed for all products, then assuming that the distribution of price-adjusted tastes is held fixed as \( N \) varies would be a natural benchmark. However, our model instead implies that optimal markups do vary across products, and that the markups for individual products change as we vary \( N \). This implies that if household tastes for products and their marginal costs held fixed, but we allow prices to change along with optimal markups when \( N \) changes, then there will necessarily be a violation of the assumed Pareto distribution. Since all of our analytical results assume the Pareto distribution of price-adjusted tastes, this means that our analytical comparative statics to changes in \( N \) and \( F \) which induce changes in product markups are technically comparative statics in response to these parameter changes plus whatever implicit changes in household tastes (or marginal costs) are necessary to preserve a Pareto distribution of price-adjusted tastes after markups adjust. In practice, high turnover means that the set of products purchased in 2004 and
in 2016 is mostly disjoint, so one can primarily interpret these as taste shifts for new products rather than taste changes for existing products. However, if the required taste shifts necessary to maintain the Pareto distribution under our counterfactuals were substantial, then this would potentially substantively change the interpretation of the welfare effects of changes in $N$.

However, we now use our numerical model to show that even though there are indeed implicit taste changes necessary to maintain the Pareto distribution as $N$ changes, in practice these required taste changes are quantitatively small and actually work against our conclusion that $N$ is welfare improving. We thus conclude that even though this is a large potential issue for the interpretation of our comparative statics, it is of little quantitative importance in practice. Specifically, we perform the following exercise: For the initial value of $N$ in 2004, we simulate our numerical model exactly as described above. Given household $i$’s resulting distribution of tastes for all $j$ products $\gamma_{i,j}^{\text{rand}}$, we can then compute a household’s actual (non-price adjusted) taste for product $j$ $\gamma_{i,j} = \gamma_{i,j}^{\text{rand}} \mu_j$ using the analytical formula for $\mu_j$ from Section 4.7. Note that as we explore above, even though our numerical model does not otherwise rely on analytical results, this analytical formula for the markup is valid since we are drawing the numerical distribution of price-adjusted tastes in the model from a Pareto distribution.

We then increase $N$ in the model but hold the particular random realizations of $\gamma_{i,j}^{\text{rand}}$ exactly fixed in the new simulation. Thus, by assumption, the values of price-adjusted tastes will be identical in the two simulations. However, as $N$ increases, the function $\mu_j$ and resulting prices will change. If price-adjusted tastes are fixed by assumption, but prices change then household tastes must change.

How large are the required taste changes necessary to maintain an identical realization from a Pareto distribution of price-adjusted tastes as $N$ increases? Figure A18 shows that these changes are small. The left panel plots the implied taste draws as a function of initial aggregate product rank $j$ for a fixed household before and after a 70% increase in $N$. Clearly the increase in $N$ induces some implied changes in tastes in order to maintain the Pareto distribution for price-adjusted tastes, but it is also clear that the requisite taste changes are small. The right panel of the plot shows a scatter plot of the realizations of taste before and after the increase in $N$. Overall the $R^2$ is above 0.999, so there is an almost perfect correlation of tastes under the two scenarios. In order to maintain an identical distribution of price-adjusted tastes, there is a modest decline in the implied average taste when $N$ increases, which lowers implied welfare by roughly 1.3%. This occurs because as $N$ increases, markups for incumbent products decline, which makes price fall and thus taste/price rise. In order to maintain a constant taste/price for that product, this means taste for those products must decline.

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7Only 13.2% of UPCs purchased in 2004 are still purchased in 2016.
8For notational simplicity, we assume that marginal cost is 1 for all products. More generally this approach actually recovers the distribution of marginal cost adjusted tastes. As long as we assume marginal cost is constant as we vary $N$, one can interpret changes in taste and changes in marginal cost adjusted taste equivalently so these are equivalent exercises.
9Further, since markup changes are a monotonic function of $j$ but individual rankings of the $j$ products are non-monotonic when $\alpha > 1$, these price changes will be non-monotonic over individual households’ consumption baskets.
10Here we focus on products which are consumed in both scenarios so that such taste comparisons are relevant.
However, the welfare conclusion in the body of the paper under the assumed constant Pareto distribution of price-adjusted tastes is that an increase in $N$ of 70% raises welfare by roughly 9.5%. The numerical results above show that those welfare results are only valid if there is also a simultaneous modest decline in non-price adjusted tastes when $N$ rises, suggesting that if one instead held tastes fixed when increasing $N$ and departed from Pareto, the welfare increase would be slightly stronger. While such an exercise could potentially be performed numerically, it would require solving for the entire equilibrium distribution of the elasticity of demand and resulting markups numerically. As discussed above, the numerical calculation of the elasticity of demand (even for a single product in partial equilibrium) is very computationally costly.

Finally, we use this simulated model to also explore the role of potential measurement error in driving concentration trends. Although C shows that the Nielsen data tracks aggregate spending measures fairly closely, the declining within-household spending patterns suggest there may be some role for attrition related measurement error across time. Furthermore, even though households are supposed to report online purchases and that Figure A1 shows that online spending is relatively unimportant for these sectors, it is possible that under-reported online spending might also drive increasing measurement error across time.

While it is difficult to analytically characterize the role of various forms of measurement error for concentration trends, we follow the indirect inference approach in Berger and Vavra (2015) and Berger and Vavra (2019) and simulate various flexible forms of measurement error in the numerical version of our model under the assumption that all other model parameters are held fixed. Specifically, we simulate the discrete version of our model and separately consider the effects of measurement error...
on household and aggregate concentration. We focus primarily on measurement error arising from failing to report transactions entirely rather than from misreporting the size of a transaction, since the former is much more likely given the structure of the Homescan data collection. We consider three types of potential under-reporting encompassing various different extremes: 1) households failing to report some randomly chosen purchases, 2) households failing to report their smallest purchases and 3) households failing to report their largest transactions. Overall, we find that while measurement error can change both household and aggregate concentration, it pushes both household and aggregate concentration in the same direction and so is unlikely to be an important explanation for the observed rise in niche consumption. Unsurprisingly, the first and second form of measurement error raise both household and aggregate concentration while the third form of measurement error instead lowers both concentration measures. Furthermore, the second form of measurement error seems most plausible given the nature of the Nielsen data, since a household might fail to report a small one-off purchase which is likely to be a small share of that household’s annual spending but is unlikely to consistently fail to report large, regular purchases that are likely to be a large share of annual spending. Since the third form of measurement error is especially unlikely, this means that measurement error is then also quite unlikely to explain a decline in aggregate concentration. As emphasized in Section D, a decline in aggregate concentration with flat household concentration would generally be sufficient to infer an increase in \( N \). Overall, these simulation results strongly suggest that measurement error does not drive the rise of niche consumption.