Journal of Choice Modelling 42 (2022) 100343

Contents lists available at ScienceDirect

Journal of Choice Modelling

journal homepage: www.elsevier.com/locate/jocm

Volumetric choice experiments (VCEs)

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ARTICLE INFO

Keywords:
Count data
Discrete choice experiment (DCE)
Experimental design
Negative binomial models
Volumetric choice experiments (VCE)

ABSTRACT

Volumetric Choice Experiments (VCEs) are designed to capture purchase quantities rather than a single, discrete choice. They can be seen as an extension of Discrete Choice Experiments (DCEs) where individuals decide how many units of a specific good or service to buy/use rather than deciding whether to buy/use it or not. There is different information in such integer count data than is contained in traditional binary or multinomial discrete choices, which presents new opportunities and interesting challenges. Like DCEs, VCEs have different components ranging from experimental design to modelling and our focus is on the overall process of implementation rather than detailed analysis of components. Our empirical examples come from large-scale VCEs embedded in surveys administered to samples drawn from Information Resources, Inc. (IRI) consumer panel for two product categories: single serve-coffee K-pods and canned tuna. The response for each alternative is a planned purchase count, possibly zero. These counts are fit using a negative binomial regression with a multilevel mixed-effects specification. Our VCE design allows for statistical identification of own- (brand by size) and cross-price elasticities, plus the effects of other attributes and demographics and their interactions with prices. The external validity of our approach is compared to results on actual canned tuna data purchases from the same IRI panelists. Advantages and limitations of VCEs as well as many unresolved research issues are discussed.

1. Introduction

This paper provides an overview of the design, implementation, and analysis of Volumetric Choice Experiments (VCEs). As a class of procedures, VCEs extend Discrete Choice Experiments (DCEs) to capture the number of times a consumer undertakes an activity. An obvious example, and the one motivating our empirical application, is the number of units of a specific product that a consumer buys during a single choice occasion. Another example, the number of times an individual plans to visit a local beach during July shows the applicability of VCEs is not limited to commercial products. The dependent variable in both these examples takes the form of integer counts, as such data is referred to in the econometrics and statistics literature. In applied work, and particularly in marketing, it is often referred to as volumetric data.

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https://doi.org/10.1016/j.jocm.2022.100343
Received 17 December 2020; Received in revised form 30 December 2021; Accepted 3 January 2022
Available online 5 January 2022
1755-5345/

VCEs are of potential interest to researchers across many fields. Researchers in fields like marketing are likely to be predicting how many units of a specified product are acquired at a single point in time, and how that quantity changes in response to changeable attributes like price. In addition, many choices, which are classically binary and multinomial discrete quantities at the choice occasion level, become count data if one introduces a time element such as an hour, week, month, quarter, or year. It is these counts per time unit that are often of more interest to decision makers. For example, the number of flights a traveller took on a particular route and airline over a specified time period. Product manufacturers may be interested in the number of technical support calls that a new product receives in the first quarter after it has been shipped. Researchers in fields like health and tourism are likely to be interested in quantity usage questions like how many doctor visits a health plan member made during the month and how many vacation trips someone takes in a year, respectively. It is not surprising that a plethora of statistical models for such data have been developed (Cameron and Trivedi, 2013). We show how to use experimental design techniques to collect such volumetric choice data for one or more competing goods. In doing so, we offer one solution to the ubiquitous endogeneity problem and a high degree of multicollinearity between predictors that characterize non-experimental data.

VCEs can be viewed as a special type of DCE where the outcomes are not discrete. Rather, they are integer counts that range between zero and some finite upper bound. For example, consider airline trips between a specific city pair, such as Boston to St. Louis, during a well-defined period like a calendar year. As only one round trip per day is possible, the dependent variable must take the form of an integer between zero and 365. The attributes of those flights matter to airlines starting with the carrier. There are two carriers, American and Southwest, with non-stop flights from Boston to St. Louis. Both generate their own counts of non-stop flights that individuals take on this route during the year. Examining individual flights likely is of interest to these airlines. There is one American flight and two Southwest flights from Boston to St. Louis. These flights differ on attributes like price and departure time. Expanding the set of airlines to include flights with one stop allows the analyst to look at price and time trade-offs. An experimental design can vary price and other flight attributes, in either an actual market context (revealed preference (RP) data), or a survey context (stated preference (SP) data), allowing estimation of the desired trade-off parameters. These parameters are all but impossible to consistently estimate from non-experimental data due to how airline revenue management systems work by endogenously increasing (decreasing) the prices consumers see when tickets for a flight on a specific day sell faster (slower) than a forecasting model had predicted. Not infrequently the price parameter estimate from a simple regression on tickets sold is positive, which is inconsistent with both economic theory and intuition. The focus of a DCE is on which of the three flights from Boston to St. Louis a potential passenger takes, if any, on a particular choice occasion where random assignment of price can overcome the typical endogeneity problem. The focus of a VCE is on how many flights individuals make on each carrier during a period like a year. While related, these two perspectives are quite distinct. Concentrating on how service frequency and pricing practices influence total traffic on a route rather than on filling individual flights is one the management practices that allowed Southwest to surpass all the legacy carriers to become the airline with the highest market capitalization.

VCEs use experimental design techniques to explore how agents choose the number of units of one item (or a relatively small set of related items) in a controlled context. That controlled context experimentally varies factor(s) of interest statistically, thereby statistically identifying at least some of the key parameters in a behavioral model. The controlled context is the ability, which is typically embedded in a survey, field/lab experiment, or test market, to randomly assign different agents to different treatments.

It is useful at the onset of this paper to define the canonical form of a VCE to parallel that of DCE using a single binary discrete choice response (Carson and Hanemann, 2005). With the canonical DCE, agents from the population of interest face a randomly assigned price and the action taken is defined in 0/1 terms (not taken/taken) is recorded. As the number of such agents who are randomly assigned to each specific price used in the DCE and the number of such price points over the relevant range increases, the probability that the action is taken at each price is traced out. In the canonical VCE, the response is the number of times the action is taken so that the expected number of such actions is traced out. It is the random assignment of price to agents that provides statistical identification of the effect of changing price in both DCEs and VCEs. Both canonical forms can be expanded by changing the price attribute to another attribute, adding other attributes, adding multiple actions, and obtaining repeat responses from the same agents under different conditions. What distinguishes between them is the response being measured and the information about preferences embedded in that response. The distinction between the canonical DCE and VCE is that the former is designed to estimate how the propensity to take an action like, buying a particular good, changes with price, while the latter is designed to measure how responsive the quantity purchased is to a change in price.

Changing price to a different attribute, adding additional attributes, considering multiple goods, and observing multiple choice occasions does not alter this fundamental difference between DCEs and VCEs, although they create distinct variants of VCEs. Both DCEs and VCEs are flexible in terms of handling a range of scenarios to allow different types of choice situations. Consider for instance a DCE where an agent, sent shopping for a holiday picnic, is given an instruction to buy only one type of grilling product and then asked whether they would buy hot dogs, hamburgers, or veggie burgers, where each type had a price and specific attributes. The same DCE if the agent did not face the buy only one-type constraint could have offered additional alternatives: hot dogs and hamburgers, hamburgers and veggie burgers, hot dogs and veggie burgers and hot dogs, hamburgers, and veggie burgers. The VCE with the original one-type constraint would have elicited a response in terms of standard quantities, where at most one of the quantity responses is positive. Without this constraint, a VCE allows for multiple positive quantities. Whether the scenario contains the one-type constraint clearly influences the sort of behavior models that one should consider for data from the relevant DCE and VCE. It does not influence, however, that the DCE response is measuring purchase propensities and the VCE is measuring quantity propensities. Throwing away information on whether a positive quantity response is greater than 1, converts VCE data to DCE data.

Before proceeding further, it will be useful to note what this paper is not about because these are all likely to be fruitful areas for future research. First, it is not about identifying which count data model is best for a particular application. While count regression models are useful to our enterprise, there is a large literature on such models. Just as different statistical models have been developed to exploit specific features of DCEs, we expect this to also happen with VCEs and that particular experimental designs will be useful testing competing specifications. Second, this is not a paper on the linkage between count data and consumer demand theory, although this is clearly an area that is underdeveloped relative to that for discrete choice data. Third, this also is not a paper on welfare calculations cast in terms of willingness to pay. While we use a commonly cited variant linking count data to an underlying economic model of demand, our VCE framework is amenable to being used with others. Fourth, this paper is not about experimental design, per se. The literature on experimental design is large in both the biomedical literature world, where clinical trials implemented using random assignment have long been the gold standard and outcomes of interest are often discrete, as well as on the industrial side, where outcomes of interest are more often continuous in nature. Designs for discrete choice experiments, where there is a long history and large literature, are more directly relevant. While the experimental designs used in our empirical examples are more than serviceable, they also are intentionally a bit pedestrian, so they are easy to explain. Finally, the work we report on addresses all the key elements needed to undertake VCEs in a straightforward manner using sets of products in categories people would see on a grocery store shelf. The basic scenario can be adapted to many other contexts. However, we eschew assigning priority as to who did the first VCE. That is probably lost in the ether of academic and commercial research scattered across many fields.

The remainder of the paper is organized as follows. In Section 2, we provide short overviews of some of the related literature that is useful in conceptually thinking about the issues involved in conducting a VCE. Next, in Section 3, we discuss econometric models available for modelling count data from VCEs. Section 4 lays out the specific experimental designs used in our empirical examples. Section 5 provides a description of the data, which was collected as part of a Social Sciences and Humanities Research Council (SSHRC) of Canada grant, as well as the model specification. Section 6 summarizes the model results. In section 7, we examine the external validity of our approach against revealed preference (RP) data from the same VCE panellists. Section 8 closes the paper by discussing advantages and limitations of VCEs and noting some of the research issues that remain.

2. A brief tour through relevant related literatures

The starting point for most readers will be a choice modelling perspective. This class of models starts with the classic binary decision between two alternatives and branches out to include multinomial choices with no natural ordering and choices with a natural ordering but without a well-defined commonly accepted quantity metric. In this paper, we consider count data as a form of choice data where there is a commonly accepted integer metric. For example, four rolls of toilet paper are twice as many as two rolls.

Similar counts in the form of the number of items purchased have been of interest to businesses and governments for as long as recorded history. In more modern times, the statistically appropriate analysis of count data lagged due to computational difficulties in fitting the requisite models. As a result, count data is often treated as a continuous response, a reasonable option when the magnitude of most of the counts is large. When almost all counts are very small, they can be examined with one of the standard discrete choice approaches ranging from binary probit models to ordered logit models, without much loss of information. There no longer is any need for either second-best practice. Now, count data models are routinely presented in standard graduate econometrics texts (e.g., Greene, 2017); and there are specialized texts solely focused on count data models (e.g., Cameron and Trivedi, 2013), and even on specialized subclasses of count data models (e.g., Hilbe, 2011). Software to estimate various types of count data models is readily available in statistical packages.

From our perspective, recognizing that one is collecting data from a sample that has been randomly assigned to an experimental treatment in a VCE is more relevant. Before one even gets to the choice tasks propagated by VCEs, the characteristics of "who" is assigned to the VCE matter in thinking about an appropriate count data model. Consider two possible samples, one a random sample of the general public and one a sample collected from patrons of the only venue in town featuring regular boxing matches. The VCE asks about how many times a week the respondent would attend boxing matches at the venue. The admission price is randomly varied, along with drink prices, dinner specials, and the quality of the boxers. In the first sample, a large fraction of the sample that opposes boxing on ethnical grounds will be completely unresponsive to varying the attributes in any plausible range (e.g., an admission price of zero might be of interest, but the venue is not going to pay people simply to walk in the door). These non-responsive people are known as "structural zeros". In contrast, through its construction, the second sample should not contain structural zeros.

Even though these two samples may receive the same VCE, they will require different count data models, with the first needing a type of zero-inflated model to accommodate the structural zeros. In our empirical VCE examples, we drew our samples from IRI panelists who have consumed the product of interest in the past, which allows us to avoid the need for distinguishing between structural and statistical zeros. However, we are only looking at the intensive margin, and focus on existing customers. Specifically, we focus on how planned purchase behavior changes in response to different stimuli. If interest lies in the extensive margin and the population of interest is the general public, changing product attributes to switch some non-purchasers to becoming purchasers is likely of interest. Then, the experimental control of a VCE can play a key role in helping to identify the role of attributes and covariates in distinguishing which people are structural zeros and which are not. There is now the possibility that the switch is not just from zero to one but zero to some count larger than one, which provides additional statistical power relative to the usual binary hurdle.

Having determined the population of interest, it is useful to think about whether RP or SP data will be collected. The VCE approach is agnostic on this issue as long as an appropriate experimental stimulus can be applied. The usual RP vs SP decision has RP cast as current choice behavior. The only difference here is that the "choice" takes the form of a count. This need not always be the case, particularly in an experimental economics context, where a choice made now can influence a later outcome. In such instances, a formal VCE structure can help in providing additional statistical power, which aids in the identification of parameters that are not well pinned down with traditional binary or multinomial choice information. What is important about RP data is that by itself, it cannot identify what is likely to happen if new brands or attributes are introduced.

SP data naturally yields planned (under VCE treatment scenarios) actions such as quantities purchased or trip counts. Thus, VCEs allow attribute levels that do not currently exist (but which could be provided) to be examined. It should be obvious that the nature of the data generating processes (DGP) for the error components in RP and SP models may substantively differ, even if the core preference parameters are statistically indistinguishable. Further, both RP and SP approaches are subject to information and strategic considerations (Carson and Groves, 2007) that should be considered in designing a VCE. An interesting research issue going forward is how these issues differ between VCEs, more traditional DCEs, and different ways of implementing VCEs. They are more transparent with the SP approach, but often pose challenges to interpreting RP data because the analyst is unable to fully control for variation in non-experimental factors that interact with the VCE treatments. Prime examples in a marketing context are stockouts facing some but not all consumers, as well as coupons (not part of the VCE) presented by some consumers that are not recorded. SP data, by itself, cannot identify what will happen in the more distant future, when a product involves learning or network effects.

Once one makes the RP vs SP decision with respect to the data collection effort, the next decision is whether interest lies in a unique choice occasion (Louviere and Hensher, 1983) or in how many times each option is chosen on a set of related choice occasions. A stylized example of the first would be a Taylor Swift concert at the Rose Bowl in Los Angeles, where interest lies in how many tickets each customer purchased online, with many attributes and associated levels that can be experimentally varied in a VCE. A second stylized example is a grocery store shelf with the competing products in a particular category like “canned tuna”, but other obvious examples abound. For instance, one can ask a respondent about how many times they plan to visit each of a set of beaches during the next month, where in addition to location, beaches might differ by crowding and parking cost. The key distinction between these two perspectives is whether the object of choice is competing only against a set of outside options or is competing against a set of other related products and the set of outside options. The latter case presents the possibility of estimating a set of parameters, such as cross-price elasticities, that describe the relationship between a set of goods, where purchasing multiple or no units of each is possible. Indeed, the set of goods that could be examined does not need to be restricted to sets of close substitutes and can be easily extended to incorporate complements. As an example, consider someone organizing a community event, where an initial decision has been made to serve only three items. Appropriate querying of community members may reveal that serving hamburgers, veggie burgers, and ice cream cones (each may have attributes like price, size, and condiments/flavours) is preferable, along some metric such as profit maximization, to offerings that included apple pie, fried chicken, hot dogs, salmon and/or snow cones. The choice of which of these two perspectives to implement has profound implications for constructing a VCE which we discuss in more detail later.

The next decision in constructing a VCE is the same as in a DCE, namely how many choice sets each agent will receive. Obviously, a researcher implementing a VCE in an SP context has considerable control over this aspect of the research design. However, even in an RP context, if one sees the same consumers on a reasonably frequent basis, then a set of offerings and the order in which they are presented can be implemented with a VCE. The two guiding principles are that for any given level of statistical precision, there is always a trade-off between obtaining choice information from more agents (in the limit, one choice occasion) or obtaining a large amount of choice information from a small number of agents. There are well-established arguments for one choice that revolve around mimicking the actual choice environment agents face, minimizing opportunities for strategic behavior, and maximizing the ability to characterize differences in preferences via interactions with observable covariates such as age (Carson and Czajkowski, 2014).

There are two main arguments in favor of collecting choice information from multiple choice sets. First, for those doing applied work, collecting choice information from multiple choice sets is often the only way to collect enough choice information to answer the research questions of interest, given typical research budgets. Secondly, having choice information from the same agent for multiple choice sets allows estimation of models that directly control for individual level heterogeneity via fixed effects, random effects, and/or the use of a latent class/random coefficients specification for preference parameters (Louviere et al., 2000). While a VCE can be used with any number of choices, the implementation requires picking some number of choice sets. How agents in a VCE setting respond to multiple choice sets is much less well-known, especially relative to DCEs (Louviere et al., 2013). Our empirical work presented later in this paper provides some initial evidence of this.

Now, the experimental design for a VCE comes into play. Since DCEs have been studied and applied for decades (and are well known to this journal), we will not review this literature beyond noting that designs for VCEs are clearly derived from them. Instead, we discuss aspects of it as needed, with a focus on statistical identification of key parameters and simple designs. For example, experimental designs for DCEs have been available and studied since Louviere and Woodworth (1983). The examples discussed in this paper are all variants of “Alternative-Specific Designs” (hereafter, “ASDs”) put forward by Louviere and Woodworth (1983) and Louviere et al. (2000), although other designs like “Generic DCEs” (Louviere and Woodworth 1983; Louviere et al., 2000) could be used if justified. ASDs “work” because they ensure the attributes of each alternative that can be chosen are orthogonal both within and between alternatives. In turn, this allows one to estimate “own-effects” (e.g., own-elasticities) within each alternative and “cross-effects” (e.g., cross-elasticities) between alternatives. In general, sample size and the number of choice sets used are going to force the use of an experimental design that falls short of enumerating all possible combinations of attribute levels like a full factorial design. In turn, this raises the issue of which interaction terms, if any, are statistically identified in the specific experimental design chosen for use in the study. Note that generic DCEs do not insure and usually do not allow statistical identification of any of the cross-effects (i.e., one assumes all effects are “generic”). Of course, one also can use designs that combine both alternative-specific and generic effects in one design, but such hybrid designs are uncommon in applied work. More generally, any design for a DCE is likely to have a VCE counterpart. We focus on illustrating ASDs in this paper because they can be applied in many ways, and naturally permit estimation of effects that can be captured by statistical models for counts.

The final topic addressed in this section before turning to modelling issues is the form in which the dependent count variable is obtained. For example, how information on the dependent count variable is obtained. At some level, this seems trivial for RP data, simply count-up the number of units purchased. However, for RP data collected using a VCE in an online environment, that may no longer be true and, in any case can be made into a testable element of the VCE. One randomly assigned set of online consumers is required to type the desired number into a box, while another statistically equivalent set consumers is faced with a (+-) counter where they move the number from "0" once they put the item in their shopping cart. The role of such elicitation formats has received considerable attention in the SP literature (Mitchell and Carson, 1989; Carson and Hanemann, 2005; Carson and Groves, 2007, 2011) where a variety of behavioral and neoclassical explanations investigate why different elicitation formats can produce different results have been advanced in the literature. Typing a number into the box is effectively a variant of the long-studied open-ended format, while the counter approach is a variant of the bidding game which is known to have a strong anchoring effect.

The main problem with the opened-ended count format is likely to be similar to that found in SP work eliciting a continuous response, namely a high non-response rate, something we have seen in exploratory work. The usual solution has been to move to something that looks like a choice format, with the payment card being a popular option. This presents respondents with columns of numbers and asks which number on the card, or any number in between, best represents their maximum willingness to pay (Mitchell and Carson, 1989). Researchers soon realized that such data was best represented not by a single number, but rather by intervals defined by the numbers on the payment card (Cameron and Huppert, 1989). The elicitation format used in our empirical work raises similar issues. One of these is a deep conceptual question that asks how consumers make decisions in certain scenarios. For example, choosing between 2 units and 3 units. We touch on this in the next section. The other issue is more direct. Any elicitation format that frames counts as a discrete choice between different numbers of units from a practical standpoint is likely to have an open-ended interval, e.g., choosing 10 or more units, and/or one or more closed intervals, e.g., 10–20 units, and 21 or more units. Such a format formally generates interval censored count data. Administrative data or exploratory research may be helpful to the researcher in determining which counts should be offered as discrete choices and which as intervals. The implications of how the zero-unit option is provided in a VCE are largely unexplored. We expect theoretical and experimental work on the role played by VCE elicitation formats to be an active area of research in the future.

3. Econometric models for count data and some initial efforts at modelling VCE data

Economic theorists initially thought about consumer demand in terms of continuous quantities for two reasons: 1) the underlying mathematics is much easier than alternatives that allow for various types of discreteness; and 2) the only available data for empirical work at the time was aggregate in nature, so the discrete aspects of demand did not seem to matter. When individual-level choice data became available, attention naturally turned to the modelling of discrete choices, with the random utility model (RUM) becoming the workhorse of empirical analysis (McFadden, 1974). Over time, those working with discrete choice data moved to address two key limitations of the aggregate conditional logit model. The first is the restrictive assumption that all agents have the same preferences except for an idiosyncratic error. These advances involved new functional forms that relax the conditional logit's independence of irrelevant alternative (IIA) assumptions and provide explicit ways to allow for preference heterogeneity in both a frequentist (Train, 2009) and Bayesian context (Rossi et al., 2005).

The ability to observe volumetric choices allows the estimation of a different, and, in some ways, richer set of consumer demand models. There are two generic approaches under which economic behavior intended to maximize utility yields count data as the outcome variable(s) of interest. The first is to simply take a standard model of continuous demand and assume that there are constraints that restrict quantities to integers. This strand of the literature takes the integer constraints as given and effectively assumes that differences in how agents solve the constraint problem, e.g., whether to pick 2 or 3 cans of tuna when the underlying latent (optimal) quantity to demand is 2.48, ends up in a relatively well-behaved error component (Pudney 1989). Rounding downward is not the obvious answer, e.g., it may be better to throw away some tuna salad rather than have someone go hungry at the picnic. How this constraint problem is solved may be context-specific. One can envision how a VCE could be used to study framing the quantity choice question where storability is either emphasized or not.

The second approach starts with a random utility framework (McFadden 1974) and poses a situation whereby agents effectively face what is known as the discrete/continuous choice problem. For example, where there are k competing brands and only one is chosen while a continuous quantity of it is consumed (Hanemann, 1984). Von Haefen et al. (2004), Bhat (2005) and a host of papers that followed provide various ways to effectively relax the constraint that consumers only purchased one brand using what has become known as a Kuhn Tucker demand system (Wales and Woodland, 1983), in various ways. The continuous part may be a truly continuous variable, like the quantity of chickpea flour scooped into a plastic bag at the local health food store or how much time is spent looking at different exhibits in a museum, or the outcome of a repeated discrete choice process over time. Howell and Allenby (2019) provide an interesting application of the latter where the first stage is picking one of several competing platforms. In their case the competing platforms were different brands of coffee makers. The second stage is how many units (e.g., K-pods) of different brands of coffee to consume in a week. Also, the second approach tends to put much more attention on the estimation of specific types of demand models. Concerns include the role of the budget constraint, definition of the outside good, the interpretation of specific statistics derivable from the parameters of such models, and whether restrictions implied by economic or psychological theories hold. We pursue the first approach in the context of an econometric model for counts because of its greater simplicity. It is important to note that VCEs are both agnostic on this issue and likely to be of considerable aid in discerning between competing specifications. That is because a VCE used to provide a clearly exogenous stimulus, not confounded by the multicollinearity problem that attributes of interest often move together in non-experimental RP data, can help get around the weak instrument problem that tends to inflate confidence intervals for the test statistics of interest.

Before moving to specific count data specifications, it is important to note that variations of discrete choice and count data models can and have been used in the past to examine the sort of count data we are interested in. One of the earliest examples of what we would now think of as a VCE is Carson et al. (1990) who offered choice sets concerning how many stamps a survey respondent would purchase, where each stamp allowed one Kenai King salmon to be caught. They fit a nested logit model to the data, where one of the main foci of the analysis is similar to the zero-inflated part of some count data models. The first nest in the model separates those in and out of the market to catch a Kenai King salmon, one of the world's premier trophy fish, in a carnival atmosphere during its relatively short spawning run on the Kenai River, where large gaffing hooks had to be eventually banned due to personal injuries. The other side of the nested logit model had the different number of stamps (capped) that could be purchased as outcomes. Their focus on how the quantity purchased changes with price is similar to that of many likely marketing VCE application.

Influenced by contingent valuation studies, another clear precursor is the contingent behavior recreational demand literature. Englin and Cameron (1996) asked respondents questions about how many times they would visit a site if it had an entrance fee of $X. A shift from asking about changes in cost to changes in water levels at a site is provided in Eiswerth et al. (2000). The nature of these contingent behavior questions, coupled with more extensive scenario elaboration than is usually seen with commercial products, led to a nature "intended" or "planned" actions (i.e., recreational trips) under the given scenario interpretation. These early contingent behavior studies often had a focus on augmenting count data-oriented travel cost models estimated with RP data by asking about the number of trips the respondent would plan to take under conditions at a site that were not identified in the existing RP data. Over time this literature moved toward consideration of multiple sites and multiple conditions (e.g., Alberini and Longo, 2006). Some of these contingent behavior studies are clearly applications of a VCE framework to elicit SP data in a specific context. More generally, they can be seen as eliciting a quantity response given some type of implicit cost, which naturally leads to casting results in terms of a metric like willingness to pay. Many health care utilization situations are similar.

The work we report here is a direct descendent of a 2010 study we did for the Australian wine industry, where the main interest focused on how purchased quantities would likely change if the Australian government implemented a major tax increase on alcohol that had been proposed (Corsi et al., 2016). The study was implemented using a survey administered to an online internet panel provided by Pure Profile. Respondents were shown photographs, with descriptive language, concerning 14 types of products (e.g., a package of beer) and told their preferred brands would be available. The proposed tax varied with alcohol content. Respondents were given sixteen choice sets and used a quantity counter with a plus and a minus sign to indicate their preferred quantities of each of the 14 types of alcohol. The first eight elicited quantities had pre-tax increase attribute levels, while the second eight shifted the price attribute (tied to alcohol content) to simulate various tax increases. A zero inflated count data model was fit to accommodate a non-trivial fraction of the sample who did not purchase alcohol.

Turning to the marketing side. Hardt et al. (2017) implemented a VCE for pizza choices where a respondent can choose quantities of six different alternatives in 12 choice sets that vary by the levels of price and six other attributes. It uses a normal hierarchical Bayes model with a Kuhn Tucker framework that ignores the integer nature of response, but combines both RP and SP data, looks at compensatory and non-compensatory decision rules and the role of consideration sets and budget constraints. In the opposite direction from assuming continuous demand, Ardeshiri and Rose (2018) implemented a meat choice experiment in which respondents were able to select more than one type of meat and a discrete quantity of each. Finding the quantities picked to be almost always zero, one, or two, they estimate an interesting variant of an ordered logit model. In a choice experiment involving beer, wine, and spirits, Lu et al. (2017) demonstrated the versatility of the DCE approach by bundling individual items to create new goods. For example, buying three bottles of wine described by price and attributes as displaying the type of alcohol (e.g., Chardonnay of a particular quality) vs. buying other bundles that could differ by quantity, type, and quality (e.g., four bottles of a particular brand of vodka each with a different flavour added at a specified price). This effectively converts quantity into an attribute and allows the examination of how non-constant unit pricing can influence purchase behavior. Models along these lines can still be usefully employed to look at data generated by VCEs, but two caveats need to be made: 1) the size of the counts in all these papers is quite small, which make a pure discrete choice approach more tractable, and 2) they tend to not exploit all the statistical information present in the counts.

In a standard exposition of the first approach of assuming underlying (latent) continuous demand and constraints that restrict quantities to integers, Hellerstein and Mendelsohn (1993) provided a theoretical foundation of count data models that can be used to perform consumer welfare. They showed that if the mean of Poisson is expressed as $\lambda = \exp(X\beta)$, consumer surplus equals $-\frac{\lambda}{\beta_{price}} = -\frac{\exp(X\beta)}{\beta_{price}}$, which is the same as the standard formula used in the continuous semi-log model. They also showed that a standard Poisson demand model can be derived from a linear (normal) model of continuous quantity that imposes a non-negative integer quantities constraint. Failure to impose this constraint can lead to biased results when continuous demand models are used. Hellerstein and Mendelsohn also show the same model can be derived as a repeated discrete choice model over a defined time interval. In contrast to standard RUM models, count data models provide an interpretable estimate of the absolute scale parameter, which has long been the primary confounding factor in choice modelling. Count data regression models (Cameron and Trivedi, 2013) retain this information and allow for estimation of key quantities, such as price elasticities, useful for product decision making.

The Poisson regression model is widely used as the baseline model of count data and is the counterpart of the conditional logit model for DCEs. We present this model but move to more general models that overcome the restricted assumptions of this baseline model. The probability density function of the Poisson model can be expressed as (Greene, 2017):

$$\mathrm{Prob}(Y_i = y_i|X_i) = \frac{e^{\lambda_i}\lambda_i^{y_i}}{y_i!},\ y_i = 0, 1, 2\ldots.\text{and i} = 1, 2, 3..\ \text{N}, \tag{1}$$

where, $y_i = 0, 1, 2\ldots.$ are the realized values of the random variable, λ_i is the mean and variance of y_i., and X_i is a covariate vector. The most common formulation of the conditional mean vector is:

$$\lambda_i = e^{X_i'\beta} \tag{2}$$

An illustrative example consistent with an underlying linear logistic model for each purchase/not purchase decision is: $\lambda = \exp[\beta_0 + \beta_1\text{Price} + \beta_2\text{Income} + \beta_3\text{Attributes} + \beta_4\text{Demographics}]$. Use of logged variables produces coefficients with an elasticity interpretation. With no income effect, the ordinary Marshallian consumer surplus estimate for the Poisson model in (1) is $-\lambda/\beta_1$. Marshallian and Hicksian welfare measures (e.g., maximum willingness to pay) for more complex count data models can be derived in a manner like that used for discrete choice models (Carson and Hanemann, 2005).

This base model is restricted because it assumes events occur independently over time and that the conditional mean and variance are equal. The Negative Binomial model overcomes these limitations by allowing inter-person heterogeneity (λ_i can vary randomly accordingly to a probability distribution). One way to add this unobserved heterogeneity in λ_i is to replace it with a stochastic equation, i.e., $\ln(\lambda_i) = X_i'\beta + \varepsilon_i$. For mathematical convenience, a gamma distribution is assumed for $f(\lambda_i)$ i.e., $\lambda_i \sim \varphi_i,\ \nu_i$; this assumption leads to the density of the most widely used Negative Binomial model (NB2) as (Cameron and Trivedi 1986, p. 33)

$$\mathrm{Prob}(Y_i = y_i|X_i, \varphi_i,\ \nu_i) = \frac{\Gamma(y_i + \nu_i)}{\Gamma(y_i + 1)\Gamma(\nu_i)}\left(\frac{\nu_i}{\nu_i + \varphi_i}\right)^{\nu_i}\left(\frac{\varphi_i}{\nu_i + \varphi_i}\right)^{y_i} \tag{3}$$

with $E[Y_i] = \varphi_i$ and $Var[Y_i] = \varphi_i + \frac{1}{\nu_i}\varphi_i^2$. Parameterization of ν_i follows the assumption that exponentiated its distribution follows a $\Gamma(1/\alpha, \alpha)$, where α is the unknown scale parameter. The range of α is often restricted to rule out the unlikely possibility of under dispersion, where the conditional variance is less than the conditional mean, and estimated in log form, reducing computational cost. It can be parameterized with covariates, but typically with considerable computational cost. This variant of the negative binomial shares a property with the quasi-ML Poisson model. Specifically, the shared property is that estimation of the preference parameters in the conditional mean part of the model is robust to misspecification of the variance component of the model, as long as the conditional mean is correctly specified.

In this formulation the variance exceeds the mean if both $\varphi_i,\ \nu_i > 0$. Non-negativity in the mean is obtained by expressing $\varphi_i = \exp(X_i'\beta)$. The NB2 model is the ‘apparent contagion’ model widely used in biometrics and avoids the independence assumption of the Poisson model. Cameron and Trivedi (1986) noted that “individuals have a constant but unequal probability of experiencing the events.” This model allows for over-dispersion arising from unobserved heterogeneity and temporal dependency. It is more straightforward in a count data model than a discrete choice model to deal with potential endogeneity if a suitable instrument is available. However, avoiding such endogeneity issues is a major advantage of SP data in both a DCE and VCE context. Like discrete choice models, one also can allow for different types of correlation, including choices by the same agent over time and across choices involving similar goods at the same time. Such correlations are the key to understanding why an agent might purchase positive quantities of more than one of the competing goods. They can be driven by heterogeneity of preferences within a household, e.g., parents like one type of cereal while the kids like another, as well as different contexts, e.g., taking tuna salad to work versus making it for a big picnic gathering, as there is satiation as the quantity of a particular good increases that leads to a shift to another or explicit variety seeking.

Assumption of a particular economic model, if it is a valid representation, can impose useful structure on a system of count data models for different goods, which facilitates clear statistical identification and interpretation of key quantities of interest (Bhat et al., 2015). The main threats to this approach are endogeneity, measurement error, and omitted variables that go beyond the usual correctness of specification issue and are well known in other contexts. Further, the restrictions with real content from micro theory hold at the individual level. They usually only transfer to the aggregate (e.g., average sample) context under very stringent additional conditions. VCEs should help to open a new front on which to explore these issues.

Count data models are available in both frequentist and Bayesian paradigms and allow for fixed and random effects, which can be useful when multiple observations on the same unit of analysis are available (Hausman et al., 1984). They are available for both individual level data and for aggregate data (Hellerstein, 1991). Models for systems of correlated count data processes oriented toward competing goods (Herriges et al., 2008) and across space (Bhat et al., 2014) have been put forward. Flexible semi-parametric and robust count data estimators are available as are latent class and random parameter variants (e.g., Chiou et al., 2019). Cameron and Trivedi (2013) provide an excellent overview of most of the issues involved in formulating and estimating count data models. A wide-range of standard and modified (e.g., censored, zero-inflated) count data models are available in statistical packages like NLOGIT, R, SAS, SPSS, and Stata. Relevant code to estimate different count data models for Julia, Mathematica, Matlab and Python also can be found easily.

4. Design of the volumetric choice experiments

In this section we look at a series of illustrative designs for different types of VCEs. We first look at a simple multivariate extension of the canonical VCE using an alternative specific design (ASD) for four airlines flying a specific route where the only attribute being varied is price and a respondent indicates how many times they would fly each airline during the time period on the route. The next two examples, single serve coffee (i.e., K-pods) and canned tuna, look at situations where responsiveness of the quantity purchased is the main focus.

4.1. Design and implementation of simple ASD VCEs

We begin by considering a simple ASD for a pricing experiment. The ASD is the type common in marketing, transportation, public policy, and many other areas. Consider potential price differences in airfares for cross-country flights in Australia between a major city pair like Sydney-Perth. Let the competitors be the airlines that used to fly this route: Qantas, Virgin Australia, Jetstar, and Tiger. Now assume each airline uses four fare prices on the route. The particular fare a prospective flyer sees varies with the airline's projected demand for the flight, the source of endogeneity that makes the estimation of price elasticities from actual flight purchase data unreliable. Fig. 1 illustrates one possible design for this problem based on a 4^4 orthogonal main effects design.

Table 1 lists the example fare levels, and Fig. 2 illustrates how a resulting VCE task might look if we use the number of flights for Sydney to Perth in the next quarter (of the year) for business purposes. We assumed, looking at RP data, that the vast majority of business travellers on that route make less than 7 flights per quarter.

We now move on to examples of two common consumer product categories on which we collected data on using VCEs for this paper: single serve-coffee and canned tuna. Example choice sets can be found in Appendix A (Figs. A.1-A.2). In these two examples, we switch from asking about the number of flights on the Sydney-Perth route over a defined period to the more typical for a DCE, asking a single purchase occasion, where the instructions effectively ask respondents to treat each choice occasion independently, a potentially testable assumption (Day et al., 2012) both in a DCE or VCE. The ability of a consumer to vary the quantity purchased as price varies allows for the possibility of stockpiling and with canned tuna being a prime example.

	Brands (factor levels 0, 1, 2, 3 are potential fare values)			
Set Number	Jetstar	Qantas	Tiger	Virgin
1	0	0	0	0
2	0	1	1	1
3	0	2	2	2
4	0	3	3	3
5	1	0	1	2
6	1	1	0	3
7	1	2	3	0
8	1	3	2	1
9	2	0	2	3
10	2	1	3	2
11	2	2	0	1
12	2	3	1	0
13	3	0	3	1
14	3	1	2	0
15	3	2	1	3
16	3	3	0	2

Fig. 1. 4^4 orthogonal main effects design.

Table 1
Fare levels used in the VCE design and resulting task.

Airline	Fare Level 0	Fare Level 1	Fare Level 2	Fare Level 3
Jetstar	275	350	425	500
Qantas	400	500	600	700
Tiger	225	275	325	375
Virgin	325	400	475	550

Scenario 1	**How many trips from Sydney to Perth will you likely make on each airline in the next 3 months?**			
	Jetstar	**Qantas**	**Tiger**	**Virgin**
	Fare: $275	Fare: $400	Fare: $225	Fare: $325
Choose **ONE &** **ONLY ONE** of the number of trips for each Airline (i.e., 1 choice per column)	☐ 0 Trips	☐ 0 Trips	☐ 0 Trips	☐ 0 Trips
	☐ 1 Trip	☐ 1 Trip	☐ 1 Trip	☐ 1 Trip
	☐ 2 Trips	☐ 2 Trips	☐ 2 Trips	☐ 2 Trips
	☐ 3 Trips	☐ 3 Trips	☐ 3 Trips	☐ 3 Trips
	☐ 4Trips	☐ 4Trips	☐ 4Trips	☐ 4Trips
	☐ 5 Trips	☐ 5 Trips	☐ 5 Trips	☐ 5 Trips
	☐ 6 Trips	☐ 6 Trips	☐ 6 Trips	☐ 6 Trips
	☐ 7 or more Trips	☐ 7 or more Trips	☐ 7 or more Trips	☐ 7 or more Trips

Fig. 2. A possible VCE task based on Fig. 1 and Table 1.

Table 2
Single serve coffee: Attributes and their levels.

Attributes\Levels		Level 0	Level 1	Level 2	Level 3
Brand/Price ($)	Starbucks	0.50	0.65	0.80	0.95
	Donut House	0.40	0.53	0.67	0.80
	Folgers	0.45	0.53	0.62	0.70
	Green Mountain	0.35	0.50	0.65	0.80
Number of Cups in a Package		12	32	52	72
Brewing Method		Keurig K-Cup	Keurig Vue Cup		
Blend		House Blend	Breakfast Blend	Columbian	French Roast
Organic Info		Non-organic	Organic		
Flavour		Not Flavored	Flavored		
Available in		Caffeinated	Decaffeinated		

Table 3
Canned tuna: Attributes and their levels.

Attributes\Levels		**Size (Oz)**	**Level 0**	**Level 1**	**Level 2**	**Level 3**	**Level 4**
Brand/Price ($)	Starkist	6	2.69	2.89	3.09	3.29	
		12	5.38	5.78	6.18	6.58	
	Bumble Bee	6	2.89	3.09	3.29	3.49	
		12	5.78	6.18	6.58	6.98	
	Chicken of the Sea	6	1.99	2.09	2.29	2.39	
		12	3.98	4.18	4.58	4.78	
	Store Brands	6	1.89	2.09	2.29	2.49	
		12	3.78	4.18	4.58	4.98	
	Any Other Brands	6	2.69	2.99	3.29	3.59	
		12	5.38	5.98	6.58	7.18	
Type			Albacore	Tuna			
Packed in			Oil	Water			
Form			Chunky	Solid			
Coupon($)[a]			0	0.50	0.75	1.00	1.50

[a] The proportions of levels differ with about 77.5% being $0 to be consistent with the RP data.

4.2. Product categories, their design attributes and designs

4.2.1. Single serve-coffee design

Table 2 shows the attributes and levels used in the VCE designs for single serve-coffee. Each of the four major brands has six attributes (brewing method, blend, organic info x flavour, caffeine, and package size x price).

The single-serve coffee (i.e., K-pods) design is an ASD. There are four major brands plus a fifth option described as "Some other Brand." The four major brands are Starbucks, Donut House, Folgers, and Green Mountain. Each of the four major brands has six attributes (brewing method, blend, country of origin x flavour, caffeine, and package size x price). The "Some other Brand" option has those six attributes plus four levels of brand names. Taken together the design is a $(2 \times 4^{\wedge}3 \times 8)^{\wedge}4 \times (2 \times 4^{\wedge}4 \times 8)$, or the total factorial is $2^{\wedge}5 \times 4^{\wedge}16 \times 8^{\wedge}5$. Additionally, an orthogonal 16-level column is used as a blocking factor. An orthogonal main effects design (from the factorial in 128 rows) is implemented, and the blocking column is used to make 16 blocks (versions) of eight choice sets. A property of this design is that all own- and cross-effects can be estimated independently of one another.

4.2.2. Canned tuna design

Table 3 below shows attributes and levels used in the VCE designs for canned tuna.

The canned tuna design used an ASD and focused on the three top-selling brands as revealed by IRI panel purchases: Starkist, Chicken of the Sea, and Bumblebee. These three brands represent over 90% of store purchase volumes. We also include a Store brands category (e.g., Walmart, Kroger) and "Any other Brand" category (e.g., Tonno Genova, Van Camp, Wild Planet) to ensure these alternatives (often store dependent) are included. Although canned tuna is available in cans of different sizes, the vast majority of sales are in 10–12 ounce and 4-to-6-ounce cans, which shows within brand size variation and may be related to input cost. We standardized the choice tasks for respondents using 6 and 12 ounces as the small and large size alternatives. This created 10 options in each choice set. Each brand had five attributes (price, coupon, type of tuna, packaging, and form). We varied price (combined with size as one attribute), what the tuna was packed in (oil or water), the type of tuna (Albacore or "Tuna") and the form of the tuna (Solid or Chunky). Finally, because coupons are an important feature of the canned tuna market, we varied whether there was a coupon available and, if available, its value).

RP data from the IRI panel showed that substantially less than 5% of all canned tuna purchases involved more than 6 cans. Therefore, we asked respondents about purchase counts ranging from 0 to 6 and then used "more than 6" cans as the upper category. Technically, this implies that a censored count data model should be used. We did not pursue this here, so we were able to provide estimates using readily available software, and we used 7 as the value for the fraction of the sample ($\sim$1%) that picked the largest quantity. This results in confidence intervals that are slightly too large. In many SP contexts, it would be possible to implement in a follow-up question asking respondents in this highest open-ended interval for an exact quantity.

Price and coupon are 8-level attributes, while the other attributes all have 2-levels. A 16-level orthogonal blocking column was used to create blocks or "versions" of the VCE. Taken together, this produces a $16 \times 8^{\wedge}10 \times 2^{\wedge}15$ factorial. We selected the smallest orthogonal main effects design from that factorial. This design produced 256 choice sets. IRI panellists were randomly assigned to one of the 16 blocks and then received the 16 choice sets in that block in a random order. This design allowed independent estimation of all own- and cross-effects. Due to the size of this design, we do not reproduce it in this paper, but it is available from the authors on request. The IRI panel members in our sample were asked to indicate the number of cans they would be most likely to purchase in each of the 16 choice sets.

5. Data, and model specification

5.1. Data description

We presented our IRI panelists with a choice task. For example, what they would see on a grocery store shelf when purchasing single-serve coffee or canned tuna. For single-serve coffee, 1 182 panelists viewed 5 alternatives at a time and answered 16 choice sets (randomly assigned to 1 of 8 blocks of 16 sets). This produced 94,560 choice occasions. For canned tuna, 750 respondents viewed 10 tuna options at a time and answered 16 choice sets (randomly assigned to 1 of 16 blocks of 16 sets). This produced 120,000 choices. A small number of respondents who did not provide a single positive response to any of the choice sets were dropped under the assumption that they effectively were not participating in the survey.

A natural question to raise asks how different the data we collected looks from the standard DCE where in each choice set a respondent pick at most one unit from a set of k alternatives. While there is no single way to examine this, there are four obvious summary statistics. First, we examine this by looking at the percent of respondents who pick more than 1 alternative in at least one

Table 4
Alternatives and counts inconsistent with a standard DCE.

Category	Respondents	Choice Sets	Respondents	Choice Sets
	Alternatives > 1	Alternatives > 1	Count > 1	Count > 1
Coffee	58.3%	21.6%	45.2%	11.3%
Tuna	72.9%	33.8%	86.3%	32.8%

choice set. By construction, this percentage is zero in a standard DCE. Second, what percent of choice sets have more than one alternative picked. This is analogue of the first measure defined at the choice set occasion rather than the respondent level. Third, what fraction respondents ever indicate a quantity greater than 1. Fourth, the same conceptual measure but now defined at the level of individual choice sets.

Table 4 displays these statistics. The first column shows, that for our three common grocery store categories, most respondents chose more than one alternative in at least one choice set, which is behavior they could not exhibit in a standard DCE. The second column suggests that, while this is not the norm, it is also not uncommon. A little over 20% of choice sets have more than one alternative picked for the coffee, with this fraction moving to a little over a third of choice sets for canned tuna. This fraction is substantially higher if attention is restricted to choice sets where at least one product had a positive quantity chosen. When picking a count greater than one, we see considerable variation among respondents by product category in a way that makes intuitive sense. A little over 45% of respondents engage in this behavior with respect to coffee (where it is important to keep in mind that the counts are on packages of K-pods not individual pods), and just over 86% with respect to canned tuna. For coffee, the number of choice sets that have at least one count that is two or more is just over 10%, while being over 30% for tuna.

5.2. Model specification

We focus our modelling and estimation attention on the canned tuna data because it provides a richer set of options to illustrate possibilities. The tuna VCE design allowed estimation of own-price elasticities for each brand-size combination as well as key cross-price elasticities. In the simplest model statistical identification follows from including the log price of each choice alternative in the model as well as the log prices of all the other goods that a respondent could have purchased in the choice set in the regression model, coupled with a VCE design that ensures the cross-price elasticities are not confounded with other quantities of interest. These cross-elasticities have long been estimated using aggregate data in regression models. These models, though, because of data limitations tend to impose strong restrictions (in prior empirical work with an appreciable number of competing options) on the relationship between the elasticities that effectively reduce the number of terms that need to be estimated (Liu et al., 2009). Our framework expands what can be practically modelled. We fit a Negative Binomial count data model to allow deviation from the usual Poisson equality restriction on the mean and variance. The eligibility restriction imposed for the random sample taken from IRI's consumer panel was that they purchased canned tuna at least once in the last year, which rules out the possibility that observed zeros are structural rather than statistical in nature.

Our model has multiple choice occasions by our respondents. There is less information in datasets of this sort relative to a simple random sample where one choice per respondent is observed. We account for this by allowing correlation between individual-level unobservable components along with robust standard errors clustered at the individual respondent level. This effectively reduces the sample size and prevents artificial inflation of statistical significance levels (Wooldridge 2010). Given the sizeable dimensions of our respondent sample size, choice alternatives, and choice sets, this allowed us to obtain precise estimates of the nature of preference heterogeneity with respect to coffee and tuna brands. We modelled preference heterogeneity in a way that likely will make the results more relevant for the decision-making process compared to the standard practice of sweeping all this heterogeneity into random parameters. We did this by using a multilevel mixed-effects specification, where many demographic covariates have fixed parameters. This accounts for much of the preference heterogeneity. Other variables, including some interactions with demographic measures, are represented by random parameters and absorb much of the remaining preference heterogeneity.

There are two standard ways of entering demographic covariates in a RUM model. Both these ways, interactions of a covariate with an attribute (e.g., men and women have different price elasticities) in a deterministic or random component context and interaction of a covariate with an alternative specific constant (e.g., a particular brand is more likely to be purchased by men than women), are also available with count data models. There is also a third way in count data models-directly add the demographic variable to the model as a covariate. Such a covariate would drop out of a RUM fit to data from a DCE, because the value for the covariate is the same for all alternatives. In a count model a covariate can shift the total quantity in category up and down. This can be particularly important for a variable like household size.

The standard part of the model structure involved the product attributes. These appear as indicator variables and include the brand-specific constants. The one exception involves coupons which is represented by two variables. The first is an indicator for whether a specific alternative has a coupon, while the second is the log of the coupon's amount if present (and zero otherwise). This allowed us to look at whether the coupon presence had an influence, distinct from its monetary value, and to look at whether respondents treated the coupon's value in the same way as a similar price change. Parameters in this part of the model were specified as random parameters with normal distributions. The availability of individual panellist demographics allowed us to explore their role in driving volumetric choices and understanding price sensitivity.

Demographic variables include the categories of Female, White, Hispanic, Presence of Children as binary indicators, household size, income (as a set of five categories), and Census Region (as a set of four categories). Each was entered by itself and interacted with its own price elasticity. This allowed a wide range in individual level variation of own-price elasticities tied to variation in observable covariates, which underpins the set of brand-specific own- and cross-price elasticities noted above. We also included a set of indicators for the specific block of choice sets a respondent received. This is done because some of these blocks idiosyncratically have sets of scenarios that, taken as a whole, the respondents saw as more or less attractive than the average or reference block. This effect should not be confounded with the preference parameters of interest.

6. Model results

The general model estimated for both categories (self-serve coffee and canned tuna) was a multilevel mixed-effects negative binomial regression model with robust (White) standard errors and clustering to account for multiple observations per respondent. The main specification involved a full set of own- and cross-price elasticities, brand-specific constants, and other attributes, with demographic variables as independent demand drivers, interactions between own price and demographic variables. The random parameters component for the canned tuna model was estimated using Stata menbreg procedure including a full covariance matrix for brands, log price per unit and the other attributes varied in the VCE (see Stata v. 16 for details; StataCorp, 2019). The model specification provides an estimate of the natural log of the scale parameter α, which helps to distinguish the negative binomial specification from that of the Poisson. Estimates for this negative binomial scale parameter for both categories rejects the equality of the conditional mean and variance, underlying the Poisson at the $p < 0.01$ level, in favor of over-dispersion. Complete model details are presented for canned tuna in Appendix B.

Most regressors are coded as 0/1 or multinomial indicator variables. Estimated effects thus take on the standard interpretation of being relevant to the baseline reference category. As with DCEs, other normalizations such as effects codes or casting covariates as standard deviations from demeaned variables. Such normalizations, which retain the same information, may aid in numerical stability of specific algorithms or facilitate a particular interpretation of model parameters. Other variables, such as household size and price have well defined natural metrics, people and dollars, and hence straightforward elasticity interpretations. More generally, the exponentiated form of the conditional mean specification (2) of most count data models needs to be taken into account in calculating many standard quantities of interest. For example, an exponentiated coefficient of zero in (2) indicates no influence on the conditional mean. Fortunately, some statistical packages such as Stata's margin command are designed to automatically take this into account. There is a 'shiny' simulation package (https://stefany.shinyapps.io/RcountD/) for R that allows considerable flexibility in defining how particular effect sizes are calculated.

6.1. Summary results for single-serve coffee

The five own-price and twenty cross-price elasticities for single-serve coffee are summarized in Table 5. As expected, all own-price elasticities are negative. It is important to note that these parameter estimates reflect the relative differences between own-price elasticities of brands rather than the actual brand price elasticity, because we allow individual demographics to interact with an individual's generic price elasticity. The actual model allows different people to have different base price sensitivities for purchasing any of these product categories.

Own price elasticities differ across brand, being highest for Other Brands ($\beta = -1.572$, $p = 0.001$) and lowest for Starbucks ($\beta =$

Table 5
Single-serve coffee: Own and cross price elasticities.

	Count	Coef.	Robust Std. Err.	z	P>|z|
Own Price Effects	Starbucks	−1.093	0.540	−2.03	0.043
	Donut House	−1.397	0.501	−2.79	0.005
	Folgers	−1.403	0.547	−2.57	0.010
	Green Mountain	−1.149	0.495	−2.32	0.020
	Other Brands	−1.572	0.489	−3.21	0.001
Cross Price Effects	Starbucks_Donut House	0.154	0.097	1.58	0.114
	Starbucks_Folgers	0.101	0.107	0.95	0.342
	Starbucks_Green Mountain	0.028	0.100	0.28	0.777
	Starbucks_Other Brands	0.165	0.132	1.26	0.209
	Donut House_Starbucks	0.196	0.092	2.13	0.034
	Donut House_Folgers	0.169	0.080	2.10	0.036
	Donut House_Green Mountain	0.115	0.068	1.68	0.093
	Donut House_Other Brands	0.208	0.077	2.69	0.007
	Folgars_Starbucks	0.216	0.198	1.09	0.275
	Folgars_Donut House	0.259	0.127	2.04	0.041
	Folgars_Green Mountain	0.229	0.142	1.61	0.101
	Folgars_Other Brands	0.025	0.159	0.16	0.873
	Green Mountain_Starbucks	0.029	0.083	0.35	0.727
	Green Mountain_Donut House	0.075	0.069	1.08	0.278
	Green Mountain_Folgers	0.222	0.076	2.91	0.004
	Green Mountain_Other Brands	0.063	0.081	0.78	0.435
	Other Brands_Starbucks	−0.043	0.059	−0.73	0.464
	Other Brands_Donut House	0.032	0.054	0.58	0.562
	Other Brands_Folgers	0.056	0.053	1.06	0.287
	Other Brands_Green Mountain	0.069	0.046	1.49	0.135

Table 6
Single-serve coffee: Brand, other attributes, and interactions with price.

	Brand	Coef.	Robust Std. Err.	z	P>|z|
Brands	Donut house	0.156	0.171	0.91	0.363
Base: Starbucks	Folgers	0.137	0.190	0.72	0.471
	Green Mountain	0.462	0.181	2.56	0.010
	Other brand	−0.345	0.184	−1.87	0.061
Brewing Method	Keurig Vue Cup	−0.559	0.065	−8.62	0.000
Base: Keurig K Cup					
Blend	Breakfast	0.021	0.052	0.41	0.683
Base: House Blend	Columbian	−0.062	0.058	−1.07	0.283
	French Roast	−0.129	0.057	−2.26	0.024
Organic + Flavor	Organic & Unflavored	−0.041	0.062	−0.66	0.507
Base: Organic & Flavored	Not Organic & Flavored	−0.099	0.057	−1.73	0.084
	Not Organic & Unflavored	0.037	0.065	0.56	0.574
Caffeinated	Decaffeinated	−0.449	0.066	−6.77	0.000
Packsize	32	−0.111	0.073	−1.51	0.132
Base:12	52	−0.196	0.097	−2.02	0.044
	72	0.206	0.112	1.83	0.067
Brewing Method * LnPrice	Keurig K-Cup	0.131	0.089	1.47	0.142
Base: Keurig Vue Cup					
Blend * LnPrice	House	0.067	0.096	0.69	0.487
Base: French Roast	Breakfast	0.032	0.100	0.32	0.750
	Columbian	0.025	0.105	0.24	0.810
Organic * LnPrice	Organic & Flavored	0.110	0.102	1.09	0.277
Base: Non-Organic & Unflavored	Organic & Unflavored	−0.026	0.109	−0.24	0.809
	Not Organic & Flavored	−0.023	0.101	−0.22	0.823
Caffeinated*LnPrice	Caffeinated	−0.019	0.087	−0.22	0.829
Base: Decaf					
Packsize *LnPrice	12	−0.209	0.229	−0.91	0.362
Base: 72	32	−0.470	0.194	−2.42	0.016
	52	−0.575	0.156	−3.68	0.000

−1.093, $p = 0.043$). Out of twenty cross-price elasticities, 5 are statistically significant at the 0.05 level. For example, the cross-price elasticity of Folgers with Donut House was 0.259 ($p = 0.041$), but cross-price elasticities are not necessarily symmetric. The cross-price elasticity of Donut House with Folgers was 0.169 ($p = 0.036$). As expected, all these cross-price elasticities are positive (except one) with varying degrees of substitution effects.

The brand-specific constants, the effect of other attributes, and their interactions with price are summarized in Table 6. We found no statistically significant differences for *brand* Donut House and Folgers versus the reference brand, Starbucks, but Green Mountain was more preferred ($p < 0.01$) to Starbucks and store brands somewhat less preferred ($p = 0.06$). Other attributes such as *brewing method*, *blend*, *caffeine content*, *package size* and their interactions with price impacted purchase choices. Our findings also show that the *Keurig Vue Cup* brewing method was much less preferred to the original *Keurig K Cup*, which was a painful lesson for Keurig. With respect to other standard coffee attributes, *French Roast* was less preferred to the reference *House Blend*, while it is statistically indistinguishable from other two blends, *Columbian* and *Breakfast.* Caffeine, on average, was preferred to decaffeinated coffee ($p < 0.01$), which is consistent with the market. Package quantities show an interesting pattern. The two intermediate sizes of 32 and 52 pods were less preferred to the base pack size of 12, while the largest size of 72 pods was weakly the most preferred size, suggesting many consumers tended to desire either a small or very large number of pods. This is further emphasized, as price sensitivity increased as one moves from 12 to 32 to 52 pods. However, this relationship did not continue for the 72-pod size.

We found household size and race influenced single-serve coffee choices. Single-serve coffee was consumed more often by one-member households than two member households ($p < 0.01$), which in turn consumed less single-serve coffee than three or four member households. These differences were not significant at the $p < 0.05$ level. Caucasians consumed more coffee pods than other races at the $p < 0.05$ level. There were some interesting (although insignificant) differences between men and women, Hispanics and non-Hispanics, households with and without children, and income groups. There were some minor differences in price sensitivity by age group.

6.2. Summary results for canned tuna

A complete set of parameter estimates for the canned tuna model is contained in Appendix B. The ten own-price effects for canned tuna were all negative, as expected. They were also highly significant ($p < 0.001$). It is important to note that these parameter estimates reflect the relative differences between own-price elasticities of brands rather than the actual brand price elasticity because we allowed individual demographics to interact with an individual's generic price elasticity. We estimated 90 cross-price effects. Similar to the way one specifies them in a "Mother Logit" model, they were included as covariates. Because the scale factor can be separately defined in a negative binomial model, the specification does not have the same theoretical issues raised by use of the Mother Logit specification, where the utility of an alternative depends on both its own attributes and those of other alternatives in the choice set (Timmermans et al., 1991). Of the cross-effects estimated, 74 were insignificant at the 0.05 level; they were generally positive and small. Sixteen of the cross-price effects were significant at the 0.05 level. Some were fairly sizeable, and, as a result, might play a potentially important role in pricing decisions. For example, the cross-price elasticity of a large size Starkist can of tuna with a large size Chicken of the Sea can of tuna was estimated to be 0.258 ($p < 0.001$). Also, there was an interesting price effect where the least expensive per ounce can in the choice set was associated with higher chosen quantities (conditional on all other covariates), which illustrates the possibility that VCEs could be used to model a wider range of pricing strategies.

With respect to the brand-specific constants, differences between the three major tuna brands estimates were surprisingly small. Most of the action involves price elasticities. It is worth noting that this straightforward ability to disentangle brand-specific parameters and price elasticities is a major strength of the VCE approach. The results of estimating the effects of the other product attributes indicated that smaller sizes were strongly preferred to larger sizes, tuna packed in water was greatly preferred to tuna packed in oil, Albacore tuna was weakly preferred to light tuna, and there was little difference in preference for solid versus chunk. The main stockout effect identified by the experimental design used involved water versus oil. The latter effect is asymmetric. Sales of the water variant increase if the other size of the same brand is oil.

Larger households tended to pick larger numbers of cans. It is possible to tease out more subtle differences, conditional on controlling for household size and the presence of children in a household did not significantly improve model fit. There were sizable regional differences with South and West panellists picking larger quantities than those in the Midwest and Northeast. Lower income households chose smaller quantities. Men and women did not differ in their choice behavior conditional on the other covariates. Nor did Caucasian and non-Caucasian households. However, Hispanics picked more cans. Price elasticities varied with income category, and as expected, lower income households were more price sensitive. They also varied with Census region, and Central and Eastern regions exhibited more price sensitivity than those in the West. An extensive set of random components for the brand indicators, own price elasticities, cross-price elasticities and the other canned tuna attributes help to characterize a considerable amount of predictable variation in respondent behavior.

7. External validity: evidence from revealed preference (RP) canned tuna data

Our sample for the SP experiment was drawn from IRI's consumer panel. We also obtained information from IRI on the actual canned tuna purchases made by our respondents for a three-month period before our VCE was implemented and a three-month period afterwards. This potentially allows for a direct comparison between the SP data from our VCE and RP data from the IRI consumer panel and hence an assessment of the external validity of our SP approach.

Such a comparison, however, is harder than it might first appear. This suggests caution in interpreting routine RP estimates made from similar datasets. Our SP data from the VCE is "clean" in the sense that all the choice sets are observed (even if nothing is purchased), the attributes are standardized, price is exogenous because it is randomly assigned, and the underlying covariate matrix is well-behaved. In contrast, use of the RP data is problematic along each of these dimensions. First, zero purchase occasions were not well-defined nor clearly observable, even though these are of critical importance with a commodity like canned tuna that can be stockpiled. To make a direct apples-to-apples comparison, we can compare choice occasions where positive quantities were chosen in either the RP or SP data. Second, standardizing attribute levels are particularly important for the size and type of fluid tuna is packed in. We only used small and large sized cans packed in oil or water and for the RP data need to engage in the usual reconciling of a lot of small differences in SKU information. Third, an indicator for coupons/deals/promotions and their values were often missing for canned tuna. Visits to shelves of canned tuna at multiple locations and times suggested a large fraction of canned tuna SKUs on a shelf usually had signs indicating a sale or promotion. Consequently, we did not use these indicators in the IRI RP data and, in parallel, did not use SP choice alternatives with money-off coupons.

There was an even larger problem, though, which makes any estimation of count data models using similar RP data highly problematic. We simply did not know what else was on the shelf when a consumer made their canned tuna choice(s). Given the large number of possible canned tuna configurations, many were missing, with no indication they were generally available to consumers. Again, visits to store shelves showed that "stockouts" involving a store's canned tuna configuration, whereby some SKUs with shelf space, were not available for purchase because the shelf had not been restocked, were frequently observed. This makes it impossible to use the modelling strategy we implemented for the SP data, which heavily exploits the fact that respondents simultaneously see ten products for which they make volumetric choices and that we know all the relevant product attributes involved in making those choices.

Due to these data limitations, we fit the simplest model compatible with both the RP and SP data. Non-conformable observations were dropped in the manner described earlier. This model is a quasi-maximum likelihood truncated Poisson regression model which is a linear function of the log price per ounce. This estimation exercise provides an estimate of the own-price elasticity of −0.340, with a 95% confidence interval of [−0.430, −0.249] for the RP choices (N = 4399) and an own-price elasticity of −0.373, with a 95% confidence interval of [−0.438, −0.308] for the SP choices (N = 13,128). The point estimates for these two own price elasticity estimates lie almost on top of each other and are statistically indistinguishable. This finding suggest that the SP choices are consistent with the RP choices made by IRI panelists with respect to information about the magnitude of own price elasticity.

Examination of purchases at individual stores suggested that the three main brands of tuna are typically available. Adding brand indicators suggested indifference between Starkist (the omitted reference brand) and Bumble Bee for both the RP and SP data. For the RP data, the relative preference for Chicken of the Sea (vs. Starkist) was 8.1% while it was 9.5% using the SP observations. The differences between the two estimates are not statistically significant at any conventional level. We can examine the other attributes, but the estimated magnitude of preference parameters depended critically on what was available to be chosen. Under the assumption that retailers were more likely to offer SKUs with attribute levels that consumers prefer more, there should be agreement on the signs between the RP and SP models, which is what we found. Small size cans were preferred to large ones, packed in water was preferred to packed-in oil, Albacore was preferred to regular tuna, and solid preferred to chunky. The one place where there was a difference was when the SP data suggested a large coupon effect. The actual sales data did not suggest a large coupon effect, although this may be due to the unreliability of the coupon data in the RP dataset, noted earlier.

8. Discussion and concluding remarks

Volumetric choice experiments (VCE) are a natural extension of DCEs. They mirror many real-world decisions, such as the decisions of how many units of a good to buy or how many times to undertake an activity in a specified period. VCEs can be fit using count data models which have well-developed theoretical and statistical foundations. These models can help overcome several long-standing issues with binary and multinomial choice data. They can separately estimate the scale parameter(s) and its parameters are directly interpretable in terms of the conditional mean specification of the standard exponentiated Poisson process. A count data model is often able to better incorporate a richer specification of preference heterogeneity into the models because of its ability to separate the conditional mean and scale specifications. Further, count data models are easily adapted to handle a variety of issues, such as censoring and truncation, which are often important aspects of a data collection effort.

VCEs can overcome many problems with non-experimental purchase data, such as avoiding endogeneity problems through randomization of attribute levels and clearly defined choice sets. By simultaneously offering a substantial number of distinct volumetric choices among competing products, a complete set of own-and cross-price elasticities can be estimated without the need to restrict the relationship between those elasticities. This can facilitate the examination of restrictions suggested by neoclassical and behavioral models and indicate that they are consistent with volumetric choice data. It has long been recognized that these elasticities are the key to making good pricing decisions (Shy, 2008). Development of experiment designs for volumetric choices are in their infancy. Useful lessons can be drawn from prior experience with DCEs and there is likely much to learn that is specific to VCEs.

Making comparisons between the RP data, on actual purchases by our sample IRI panelists, and the behavior of those same panelists when faced with the choices offered in our VCE was less straight forward than it may have first appeared. Indeed, the effort we undertook clearly suggests that simply expecting RP and SP estimates to be statistically equivalent if both are valid is naïve. Some issues involved in making the comparison revolve around the typical messiness of RP data. There is a proliferation of SKUs with minor differences and data fields for some attributes needed to make the desired comparisons are not reliably populated. However, the two largest problems are fundamental. First, only purchases of positive quantities are observed in the RP data. This can be dealt with using only positive purchase quantities combined with a truncated count model, albeit with potentially a substantial loss of information. More resolution could be gained if there is information on shopping trips where canned tuna was not purchased. Second, and largely uncorrectable, consumers, but not the researcher, know what else was on the shelf when an actual purchase was made. Casual visits to canned tuna shelves show frequent stockouts to be a dominant feature of the product category. In contrast, by construction, our VCE told us exactly what else was on the shelf, and indeed the role of such stockouts can be explicitly modelled by making them part of the experimental design. Nevertheless, with the weakest assumption that allowed identification of the own-price elasticity of canned tuna, we effectively found statistically equivalent estimates. Slightly stronger assumptions yielded similar estimates for preferences involving the three tuna brands usually available. Identification of preferences for other attributes that rely on whether a particular SKU, e.g., 12-ounce can of Bumble Bee chunky albacore tuna packed in oil, was actually on the shelf available for a consumer to purchase their desired quantity is considerably more problematic. With the assumption that retailers were more likely to offer products with attributes preferred by more consumers so that such products are on the shelf available for purchase, it suggests that the signs on different non-price attributes should be the same in the RP and SP data. This is confirmed in empirical estimates.

Our results for the negative binomial regression model estimated with the data obtained using the VCE appear quite promising. The set of own-price elasticities produced were reasonable in magnitude. One of the main messages from our results indicates that taking both differences in own-price elasticities and brand-specific constants into account provides a much richer picture about what is happening in the market than models that only allow for one price effect. For example, for 12oz cans of tuna, the own price elasticity for Starkist was almost 45% higher than the market leader, Chicken of the Sea, but only 10% higher for 6oz cans. Further, while store brands had price elasticities similar to those of Chicken of the Sea, the 6oz and 12oz store brand constants were much smaller than those for Chicken of the Sea. For single-serve coffee, Donut House and Folgers had higher own-price elasticity compared to Starbucks and Green Mountain, suggesting that most of the action is in the brand constants. Complete sets of cross-price elasticities were produced, and, with some minor exceptions, they were consistent with theoretical predictions. Generally, the cross-price effects were small in magnitude, but some are large enough to be important considerations in making pricing decisions. Other components of the model paint a rich picture of the underlying preference heterogeneity for different attributes and the heterogeneity in demographic drivers of demand.

More generally, VCEs will be a useful addition to the toolkit of researchers studying choice behavior. Fundamentally, they focus on a different relationship than DCEs, namely how does the quantity of a particular good change in response to changes in attribute levels, rather than whether that good is chosen in response to changes in attribute levels. They force consideration of whether the action of interest occurs at one moment in time or over the course of some time period and whether interest lies in what brand is chosen or how many of each brand is chosen is the choice object of interest. They allow consumer characteristics to drive quantity decisions in a natural way, e.g., larger households buy more rolls of toilet paper, without needing to influence what brand. They can draw much from the DCE parents, but present opportunities to look at a new range of choice behavior and to re-examine some old questions in a new light. Along the way much will be learned about how to best collect and model VCE data.

Author statement

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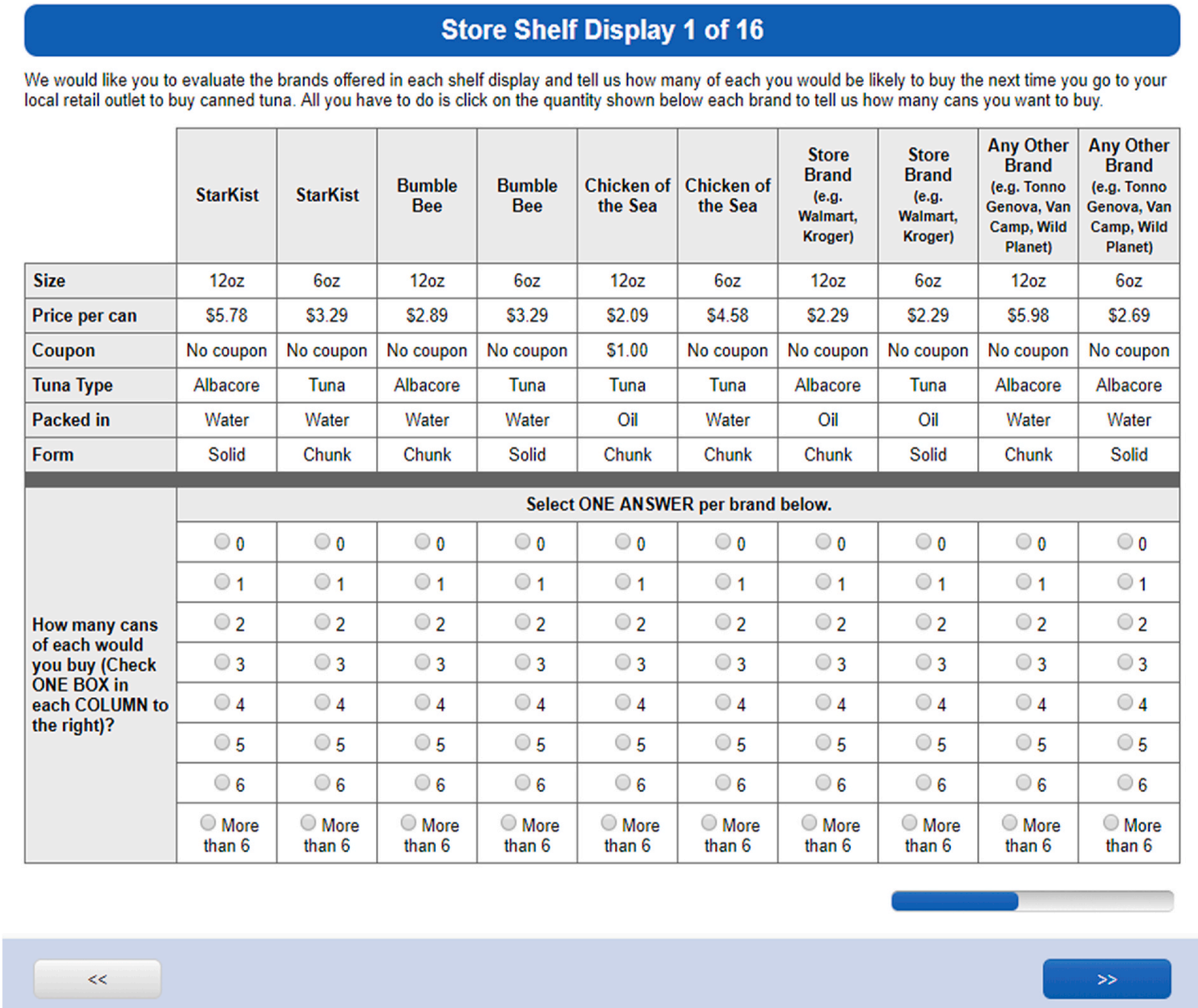
Acknowledgement

Social Sciences and Humanities Research Council (SSHRC) of Canada, Grant No. 430199 supported our work. A preliminary version of results using one of the product categories (canned tuna) collected for this project was presented at the Sawtooth Software Conference. We thank the Journal's referees and associate editor for comments and suggestions that considerably improved the paper. Emily Khossravi provided excellent research assistance.

Appendix A. Sample Choice Set of Single-Serve Coffee and Canned Tuna

	Starbucks	Donut House	Folgers	Green Mountain	Gevalia
Brewing Method	Keurig K-Cup	Keurig Vue Cup	Keurig Vue Cup	Keurig K-Cup	Keurig Vue Cup
Blend	Breakfast	House	Columbian	Breakfast	French
Flavored/Unflavored	Flavored	Unflavored	Unflavored	Unflavored	Flavored
Growing Method	Organic	Not Organic	Organic	Organic	Not Organic
Caffeinated/Decaffeinated	Decaffeinated	Caffeinated	Caffeinated	Caffeinated	Caffeinated
Pack Size (Number of Pods/Cups)	32	72	52	12	72
Price Per Cup/Pod	$0.75	$0.50	$0.55	$0.70	$0.45
How many of each brand would you most likely buy the next time you shop for coffee pods if these brands were offered where you shop? Select NO MORE THAN ONE BOX per brand. If you would buy none of the brands, then select 'I would not buy ANY of these brands'.	○ 1	○ 1	○ 1	○ 1	○ 1
	○ 2	○ 2	○ 2	○ 2	○ 2
	○ 3	○ 3	○ 3	○ 3	○ 3
	○ 4	○ 4	○ 4	○ 4	○ 4
	○ 5	○ 5	○ 5	○ 5	○ 5
	○ More than 5	○ More than 5	○ More than 5	○ More than 5	○ More than 5
	○ I would not buy ANY of these brands				

Fig. A1. Example Choice Set from the Single-Serve Coffee VCE.



Store Shelf Display 1 of 16

We would like you to evaluate the brands offered in each shelf display and tell us how many of each you would be likely to buy the next time you go to your local retail outlet to buy canned tuna. All you have to do is click on the quantity shown below each brand to tell us how many cans you want to buy.

	StarKist	StarKist	Bumble Bee	Bumble Bee	Chicken of the Sea	Chicken of the Sea	Store Brand (e.g. Walmart, Kroger)	Store Brand (e.g. Walmart, Kroger)	Any Other Brand (e.g. Tonno Genova, Van Camp, Wild Planet)	Any Other Brand (e.g. Tonno Genova, Van Camp, Wild Planet)
Size	12oz	6oz	12oz	6oz	12oz	6oz	12oz	6oz	12oz	6oz
Price per can	$5.78	$3.29	$2.89	$3.29	$2.09	$4.58	$2.29	$2.29	$5.98	$2.69
Coupon	No coupon	No coupon	No coupon	No coupon	$1.00	No coupon	No coupon	No coupon	No coupon	No coupon
Tuna Type	Albacore	Tuna	Albacore	Tuna	Tuna	Tuna	Albacore	Tuna	Albacore	Albacore
Packed in	Water	Water	Water	Water	Oil	Water	Oil	Oil	Water	Water
Form	Solid	Chunk	Chunk	Solid	Chunk	Chunk	Chunk	Solid	Chunk	Solid
	Select ONE ANSWER per brand below.									
How many cans of each would you buy (Check ONE BOX in each COLUMN to the right)?	0	0	0	0	0	0	0	0	0	0
	1	1	1	1	1	1	1	1	1	1
	2	2	2	2	2	2	2	2	2	2
	3	3	3	3	3	3	3	3	3	3
	4	4	4	4	4	4	4	4	4	4
	5	5	5	5	5	5	5	5	5	5
	6	6	6	6	6	6	6	6	6	6
	More than 6	More than 6	More than 6	More than 6	More than 6	More than 6	More than 6	More than 6	More than 6	More than 6

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Fig. A2. Example Choice Set from the Canned Tuna VCE.

Appendix B. Complete Set of Parameter Estimates from Canned Tuna VCE Model

Table B1
Panel A - Own and Cross Price Effects

Choice	Coef.	Robust Std. Err.	z	P>|z|	[95% Conf. Interval]	
Own						
[1]Own_Starkist12oz	−3.052	0.181	−16.84	0.000	−3.407	−2.697
[2] Own_Starkist6oz	−2.976	0.160	−18.64	0.000	−3.289	−2.663
[3]Own_BumbleBee12oz	−2.826	0.148	−19.06	0.000	−3.117	−2.535
[4]Own_BumbleBee6oz	−3.067	0.204	−15.05	0.000	−3.466	−2.667
[5]Own_ChickenSea12oz	−2.111	0.131	−16.17	0.000	−2.367	−1.855
[6]Own_ChickenSea6oz	−2.718	0.137	−19.84	0.000	−2.987	−2.450
[7]Own_StoreBrand12oz	−2.167	0.145	−14.97	0.000	−2.451	−1.883
[8]Own_StoreBrand6oz	−2.616	0.139	−18.78	0.000	−2.889	−2.343
[9]Own_OtherBrand12oz	−2.831	0.139	−20.32	0.000	−3.104	−2.558
[10] Own_OtherBrand6oz	−2.920	0.151	−19.28	0.000	−3.216	−2.623
Cross						
Cross_Starkist6oz_Starkist12oz	0.176	0.155	1.13	0.257	−0.128	0.479
Cross_Starkist12oz_Starkist6z	0.045	0.051	0.88	0.377	−0.055	0.146
Cross_Starkist12oz_BumbleBee12oz	0.022	0.049	0.45	0.656	−0.074	0.118

(*continued on next page*)

Table B1 (*continued*)

Choice	Coef.	Robust Std. Err.	z	P>|z|	[95% Conf. Interval]	
Cross_Starkist6oz_BumbleBee12oz	0.051	0.052	0.98	0.329	−0.051	0.153
Cross_Starkist12oz_BumbleBee6oz	0.040	0.136	0.29	0.769	−0.227	0.307
Cross_Starkist6oz_BumbleBee6oz	0.070	0.158	0.44	0.659	−0.240	0.379
Cross_Starkist12oz_ChickenSea12oz	0.257	0.062	4.14	0.000	0.135	0.379
Cross_Starkist6oz_ChickenSea12oz	−0.019	0.054	−0.35	0.724	−0.125	0.087
Cross_Starkist12oz_ChickenSea6oz	0.073	0.045	1.61	0.108	−0.016	0.161
Cross_Starkist6oz_ChickenSea6oz	0.040	0.050	0.81	0.419	−0.058	0.139
Cross_Starkist12oz_StoreBrand12oz	0.032	0.035	0.91	0.365	−0.037	0.101
Cross_Starkist6oz_StoreBrand12oz	−0.112	0.042	−2.65	0.008	−0.195	−0.029
Cross_Starkist12oz_StoreBrand6oz	0.046	0.030	1.5	0.133	−0.014	0.105
Cross_Starkist6oz_StoreBrand6oz	−0.022	0.048	−0.47	0.640	−0.116	0.071
Cross_Starkist12oz_OtherBrand12oz	0.024	0.034	0.7	0.485	−0.043	0.090
Cross_Starkist6oz_OtherBrand12oz	−0.007	0.041	−0.16	0.870	−0.087	0.074
Cross_Starkist12oz_OtherBrand6oz	0.016	0.032	0.49	0.621	−0.046	0.077
Cross_Starkist6oz_OtherBrand6oz	−0.055	0.040	−1.36	0.172	−0.133	0.024
Cross_BumbleBee12oz_Starkist12oz	0.175	0.141	1.25	0.213	−0.100	0.451
Cross_BumbleBee6oz_Starkist12oz	0.213	0.154	1.38	0.167	−0.089	0.515
Cross_BumbleBee12oz_Starkist6oz	0.006	0.061	0.1	0.923	−0.114	0.126
Cross_BumbleBee6oz_Starkist6oz	0.142	0.050	2.84	0.005	0.044	0.240
Cross_BumbleBee6oz_BumbleBee12oz	0.136	0.072	1.88	0.060	−0.006	0.278
Cross_BumbleBee12oz_BumbleBee6oz	−0.071	0.137	−0.52	0.603	−0.340	0.198
Cross_BumbleBee12oz_ChickenSea12oz	0.196	0.044	4.43	0.000	0.109	0.283
Cross_BumbleBee6oz_ChickenSea12oz	0.058	0.060	0.96	0.337	−0.060	0.175
Cross_BumbleBee12oz_ChickenSea6oz	0.114	0.087	1.3	0.194	−0.058	0.285
Cross_BumbleBee6oz_ChickenSea6oz	0.073	0.069	1.05	0.293	−0.063	0.208
Cross_BumbleBee12oz_StoreBrand12oz	0.033	0.041	0.8	0.421	−0.047	0.113
Cross_BumbleBee6oz_StoreBrand12oz	−0.021	0.042	−0.5	0.619	−0.102	0.061
Cross_BumbleBee12oz_StoreBrand6oz	0.047	0.034	1.37	0.172	−0.020	0.114
Cross_BumbleBee6oz_StoreBrand6oz	−0.016	0.037	−0.43	0.666	−0.087	0.056
Cross_BumbleBee12oz_OtherBrand12oz	0.099	0.035	2.79	0.005	0.030	0.169
Cross_BumbleBee6oz_OtherBrand12oz	0.005	0.040	0.13	0.894	−0.073	0.084
Cross_BumbleBee12oz_OtherBrand6oz	−0.014	0.041	−0.34	0.734	−0.095	0.067
Cross_BumbleBee6oz_OtherBrand6oz	−0.043	0.037	−1.17	0.241	−0.115	0.029
Cross_ChickenSea12oz_Starkist12oz	0.126	0.131	0.96	0.339	−0.132	0.383
Cross_ChickenSea6oz_Starkist12oz	0.315	0.147	2.14	0.033	0.026	0.604
Cross_ChickenSea12oz_Starkist6oz	−0.031	0.043	−0.72	0.468	−0.116	0.053
Cross_ChickenSea6oz_Starkist6oz	0.025	0.041	0.61	0.540	−0.055	0.104
Cross_ChickenSea12oz_BumbleBee12oz	0.059	0.057	1.03	0.303	−0.053	0.170
Cross_ChickenSea6oz_BumbleBee12oz	0.081	0.059	1.37	0.171	−0.035	0.197
Cross_ChickenSea12oz_BumbleBee6oz	0.008	0.135	0.06	0.951	−0.256	0.272
Cross_ChickenSea6oz_BumbleBee6oz	0.075	0.143	0.53	0.598	−0.205	0.355
Cross_ChickenSea6oz_ChickenSea12oz	0.213	0.065	3.25	0.001	0.085	0.341
Cross_ChickenSea12oz_ChickenSea6oz	0.141	0.062	2.29	0.022	0.020	0.262
Cross_ChickenSea12oz_StoreBrand12oz	0.049	0.029	1.69	0.092	−0.008	0.107
Cross_ChickenSea6oz_StoreBrand12oz	0.037	0.035	1.05	0.293	−0.032	0.106
Cross_ChickenSea12oz_StoreBrand6oz	0.005	0.027	0.2	0.839	−0.047	0.057
Cross_ChickenSea6oz_StoreBrand6oz	0.050	0.043	1.17	0.242	−0.034	0.133
Cross_ChickenSea12oz_OtherBrand12oz	0.035	0.028	1.25	0.210	−0.020	0.089
Cross_ChickenSea6oz_OtherBrand12oz	0.070	0.042	1.67	0.094	−0.012	0.151
Cross_ChickenSea12oz_OtherBrand6oz	−0.017	0.027	−0.63	0.527	−0.070	0.036
Cross_ChickenSea6oz_OtherBrand6oz	0.029	0.037	0.78	0.435	−0.044	0.101
Cross_StoreBrand12oz_Starkist12oz	0.313	0.125	2.5	0.012	0.068	0.558
Cross_StoreBrand6oz_Starkist12oz	0.476	0.145	3.29	0.001	0.193	0.759
Cross_StoreBrand12oz_Starkist6oz	0.039	0.059	0.66	0.507	−0.076	0.154
Cross_StoreBrand6oz_Starkist6oz	0.038	0.050	0.75	0.455	−0.061	0.136
Cross_StoreBrand12oz_BumbleBee12oz	−0.020	0.072	−0.28	0.779	−0.161	0.121
Cross_StoreBrand6oz_BumbleBee12oz	−0.026	0.052	−0.51	0.613	−0.127	0.075
Cross_StoreBrand12oz_BumbleBee6oz	0.079	0.147	0.53	0.593	−0.210	0.367
Cross_StoreBrand6oz_BumbleBee6oz	0.142	0.166	0.85	0.393	−0.184	0.468
Cross_StoreBrand12oz_ChickenSea12oz	0.134	0.048	2.78	0.005	0.040	0.228
Cross_StoreBrand6oz_ChickenSea12oz	0.087	0.050	1.73	0.084	−0.012	0.186
Cross_StoreBrand12oz_ChickenSea6oz	0.111	0.073	1.53	0.126	−0.031	0.254
Cross_StoreBrand6oz_ChickenSea6oz	0.079	0.051	1.54	0.122	−0.021	0.180
Cross_StoreBrand6oz_StoreBrand12oz	0.157	0.050	3.11	0.002	0.058	0.256
Cross_StoreBrand12oz_StoreBrand6oz	−0.067	0.045	−1.5	0.134	−0.155	0.021
Cross_StoreBrand12oz_OtherBrand12oz	0.096	0.062	1.53	0.126	−0.027	0.218
Cross_StoreBrand6oz_OtherBrand12oz	0.149	0.054	2.77	0.006	0.044	0.254
Cross_StoreBrand12oz_OtherBrand6oz	−0.007	0.064	−0.11	0.910	−0.132	0.117
Cross_StoreBrand6oz_OtherBrand6oz	−0.084	0.048	−1.74	0.082	−0.179	0.011
Cross_OtherBrand12oz_Starkist12oz	0.017	0.139	0.12	0.905	−0.257	0.290

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Table B1 (*continued*)

Choice	Coef.	Robust Std. Err.	z	P>|z|	[95% Conf. Interval]	
Cross_OtherBrand6oz_Starkist12oz	−0.021	0.163	−0.13	0.898	−0.340	0.298
Cross_OtherBrand12oz_Starkist6oz	0.002	0.051	0.05	0.964	−0.098	0.102
Cross_OtherBrand6oz_Starkist6oz	−0.072	0.061	−1.19	0.234	−0.191	0.047
Cross_OtherBrand12oz_BumbleBee12oz	0.141	0.063	2.22	0.026	0.017	0.265
Cross_OtherBrand6oz_BumbleBee12oz	0.036	0.057	0.63	0.528	−0.076	0.148
Cross_OtherBrand12oz_BumbleBee6oz	0.012	0.168	0.07	0.945	−0.318	0.341
Cross_OtherBrand6oz_BumbleBee6oz	0.017	0.198	0.09	0.931	−0.372	0.406
Cross_OtherBrand12oz_ChickenSea12oz	0.102	0.054	1.9	0.058	−0.003	0.208
Cross_OtherBrand6oz_ChickenSea12oz	0.065	0.048	1.35	0.176	−0.029	0.159
Cross_OtherBrand12oz_ChickenSea6oz	0.020	0.062	0.32	0.749	−0.101	0.140
Cross_OtherBrand6oz_ChickenSea6oz	−0.007	0.056	−0.12	0.901	−0.116	0.102
Cross_OtherBrand12oz_StoreBrand12oz	0.129	0.053	2.42	0.016	0.024	0.233
Cross_OtherBrand6oz_StoreBrand12oz	0.014	0.052	0.27	0.787	−0.087	0.115
Cross_OtherBrand12oz_StoreBrand6oz	0.036	0.047	0.78	0.435	−0.055	0.128
Cross_OtherBrand6oz_StoreBrand6oz	0.044	0.060	0.72	0.470	−0.075	0.162
Cross_OtherBrand6oz_OtherBrand12oz	0.180	0.067	2.69	0.007	0.049	0.312
Cross_OtherBrand12oz_OtherBrand6oz	0.013	0.050	0.25	0.799	−0.086	0.112
CheapestOz	0.070	0.029	2.42	0.015	0.013	0.127

Panel B: Brand, Attribute and Their Interaction with Price Effects

Brand	Coef.	Robust Std. Err.	z	P>|z|	[95% Conf. Interval]	
BumbleBee	−0.032	0.206	−0.15	0.878	−0.435	0.372
ChickenSea	0.690	0.209	3.30	0.001	0.281	1.099
StoreBrand	−1.234	0.239	−5.17	0.000	−1.702	−0.767
OtherBrand	−2.834	0.247	−11.47	0.000	−3.318	−2.350
hcoupon	0.433	0.036	12.05	0.000	0.363	0.504
hcoupon*lcoupon						
0	0.000	(omitted)				
1	0.400	0.041	9.86	0.000	0.321	0.480
SixOz	1.183	0.150	7.90	0.000	0.890	1.477
Albacore	0.095	0.053	1.80	0.072	−0.009	0.199
Albacore*Own_lnPriceoz						
0	0.010	0.033	0.30	0.765	−0.055	0.075
1	0.000	(omitted)				
Water	3.569	0.093	38.44	0.000	3.387	3.751
Water*Own_lnPriceoz						
0	0.001	0.041	0.03	0.977	−0.079	0.081
1	0.000	(omitted)				
Solid	0.016	0.059	0.27	0.787	−0.100	0.132
Solid*Own_lnPriceoz						
0	0.044	0.035	1.26	0.206	−0.024	0.112
1	0.000	(omitted)				
WatOil	0.069	0.019	3.66	0.000	0.032	0.106
OilWat	−0.001	0.026	−0.05	0.958	−0.052	0.049

Panel C: Demographics, Interaction with Price Effects, Scale Parameter

Demographics	Coef.	Robust Std. Err.	z	P>|z|	[95% Conf. Interval]	
Female	−0.036	0.050	−0.73	0.468	−0.133	0.061
Female*Own_lnPriceoz						
0	−0.004	0.042	−0.09	0.926	−0.087	0.079
1	0.000	(omitted)				
White	−0.064	0.052	−1.22	0.223	−0.166	0.039
White*Own_lnPriceoz						
0	−0.045	0.045	−1	0.317	−0.133	0.043
1	0.000	(omitted)				
Hispanic	0.108	0.077	1.41	0.158	−0.042	0.259
Hispanic*Own_lnPriceoz						
0	−0.063	0.065	−0.97	0.334	−0.191	0.065
1	0.000	(omitted)				
pres_child						
Yes (Children under 18 are in hh)	0.083	0.051	1.62	0.106	−0.018	0.183
pres_child*Own_lnPriceoz						
No (No children under age 18)	0.060	0.043	1.38	0.167	−0.025	0.144
Yes (Children under 18 are in hh)	0.000	(omitted)				
household	0.144	0.024	5.96	0.000	0.097	0.192
c.household*Own_lnPriceoz	−0.008	0.020	−0.38	0.707	−0.047	0.032
hhinc						
$25,000 to $49,999 per year	0.365	0.079	4.65	0.000	0.211	0.519
$50,000 to $69,999 per year	0.609	0.082	7.41	0.000	0.448	0.770

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Table B1 (*continued*)

Panel C: Demographics, Interaction with Price Effects, Scale Parameter						
Demographics	Coef.	Robust Std. Err.	z	P>|z|	[95% Conf. Interval]	
$70,000 to $99,999 per year	0.486	0.083	5.82	0.000	0.322	0.649
$100, 000 or Higher	0.757	0.083	9.14	0.000	0.595	0.920
hhinc*Own_lnPriceoz						
Under 25,000 per year	−0.723	0.069	−10.42	0.000	−0.859	−0.587
$25,000 to $49,999 per year	−0.395	0.054	−7.34	0.000	−0.501	−0.290
$50,000 to $69,999 per year	−0.338	0.059	−5.69	0.000	−0.455	−0.222
$70,000 to $99,999 per year	−0.199	0.058	−3.46	0.001	−0.313	−0.086
$100, 000 or Higher	0.000	(omitted)				
census						
East	0.131	0.064	2.05	0.041	0.006	0.256
South	0.641	0.058	11.13	0.000	0.528	0.753
West	0.599	0.068	8.78	0.000	0.466	0.733
census*Own_lnPriceoz						
Central	−0.335	0.058	−5.81	0.000	−0.448	−0.222
East	−0.151	0.058	−2.59	0.010	−0.265	−0.037
South	−0.013	0.053	−0.25	0.801	−0.116	0.090
West	0.000	(omitted)				
Constant and ln(α)						
Constant	−10.465	0.273	−38.31	0.000	−11.001	−9.930
ln(α)	−3.804	1.269			−6.292	−1.317

Panel D: Blocking Effects						
Block	Coef.	Robust Std. Err.	z	P>|z|	[95% Conf. Interval]	
2	−0.078	0.080	−0.99	0.325	−0.235	0.078
3	0.330	0.020	16.54	0.000	0.291	0.369
4	0.283	0.083	3.42	0.001	0.121	0.445
5	0.467	0.106	4.38	0.000	0.258	0.675
6	0.123	0.082	1.50	0.133	−0.037	0.283
7	0.052	0.105	0.49	0.621	−0.154	0.258
8	0.463	0.085	5.46	0.000	0.297	0.629
9	0.464	0.107	4.35	0.000	0.255	0.674
10	0.218	0.085	2.57	0.010	0.052	0.383
11	0.259	0.106	2.44	0.015	0.051	0.467
12	0.259	0.083	3.12	0.002	0.096	0.421
13	0.210	0.020	10.44	0.000	0.170	0.249
14	0.059	0.083	0.71	0.477	−0.104	0.222
15	0.232	0.018	12.63	0.000	0.196	0.268
16	0.243	0.080	3.02	0.003	0.085	0.401
Constant	−10.465	0.273	−38.31	0.000	−11.001	−9.930
ln(α)	−3.804	1.269			−6.292	−1.317

Panel E: Random Effects - Variances and Covariances						
Responseid	Coef.	Robust Std. Err.	z	P>|z|	[95% Conf. Interval]	
var(BumbleBee)	2.474	0.128			2.236	2.739
var(ChickenSea)	1.652	0.103			1.461	1.868
var(SBrand)	5.492	0.223			5.072	5.946
var(OBrand)	7.214	0.338			6.581	7.908
var(SixOz)	2.291	0.091			2.119	2.477
var(Albacore)	0.853	0.048			0.764	0.953
var(Water)	8.031	0.255			7.546	8.547
var(Solid)	0.982	0.062			0.868	1.110
var(own_lnPriceoz)	3.949	0.126			3.710	4.203
var(_cons)	16.417	0.486			15.492	17.397
cov(BumbleBee,ChickenSea)	1.170	0.088	13.37	0.000	0.999	1.342
cov(BumbleBee,SBrand)	0.854	0.070	12.23	0.000	0.718	0.991
cov(BumbleBee,OBrand)	1.412	0.084	16.81	0.000	1.248	1.577
cov(BumbleBee,SixOz)	−0.144	0.073	−1.99	0.047	−0.287	−0.002
cov(BumbleBee,Albacore)	0.186	0.047	3.95	0.000	0.093	0.278
cov(BumbleBee,Water)	−0.021	0.026	−0.79	0.428	−0.072	0.031
cov(BumbleBee,Solid)	0.103	0.045	2.30	0.022	0.015	0.191
cov(BumbleBee,own_lnPriceoz)	0.265	0.061	4.32	0.000	0.145	0.385
cov(BumbleBee,_cons)	−0.679	0.102	−6.67	0.000	−0.878	−0.479
cov(ChickenSea,SBrand)	1.365	0.076	18.06	0.000	1.217	1.513
cov(ChickenSea,OBrand)	1.536	0.090	17.05	0.000	1.360	1.713
cov(ChickenSea,SixOz)	0.031	0.062	0.50	0.617	−0.090	0.153
cov(ChickenSea,Albacore)	0.073	0.039	1.87	0.062	−0.004	0.149
cov(ChickenSea,Water)	0.013	0.020	0.67	0.502	−0.026	0.053

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Table B1 (*continued*)

Panel E: Random Effects - Variances and Covariances						
Responseid	Coef.	Robust Std. Err.	z	P>|z|	[95% Conf. Interval]	
cov(ChickenSea,Solid)	−0.055	0.034	−1.64	0.102	−0.121	0.011
cov(ChickenSea,own_lnPriceoz)	0.176	0.055	3.18	0.001	0.068	0.285
cov(ChickenSea,_cons)	−0.819	0.084	−9.73	0.000	−0.984	−0.654
cov(SBrand,OBrand)	5.159	0.202	25.55	0.000	4.764	5.555
cov(SBrand,SixOz)	0.106	0.106	1.00	0.319	−0.102	0.314
cov(SBrand,Albacore)	0.040	0.063	0.64	0.521	−0.083	0.164
cov(SBrand,Water)	0.345	0.041	8.39	0.000	0.264	0.426
cov(SBrand,Solid)	−0.018	0.065	−0.28	0.777	−0.146	0.109
cov(SBrand,own_lnPriceoz)	−0.392	0.095	−4.12	0.000	−0.578	−0.206
cov(SBrand,_cons)	−2.088	0.154	−13.56	0.000	−2.389	−1.786
cov(OBrand,SixOz)	0.000	0.114	0.00	1.000	−0.224	0.224
cov(OBrand,Albacore)	0.074	0.070	1.05	0.292	−0.064	0.212
cov(OBrand,Water)	−0.064	0.043	−1.50	0.134	−0.147	0.020
cov(OBrand,Solid)	0.218	0.071	3.05	0.002	0.078	0.358
cov(OBrand,own_lnPriceoz)	−0.482	0.096	−4.99	0.000	−0.671	−0.293
cov(OBrand,_cons)	−1.963	0.174	−11.25	0.000	−2.305	−1.621
cov(SixOz,Albacore)	0.010	0.042	0.24	0.809	−0.072	0.092
cov(SixOz,Water)	−0.005	0.027	−0.18	0.860	−0.058	0.048
cov(SixOz,Solid)	−0.031	0.044	−0.69	0.490	−0.117	0.056
cov(SixOz,own_lnPriceoz)	−1.553	0.067	−23.05	0.000	−1.686	−1.421
cov(SixOz,_cons)	−2.753	0.126	−21.79	0.000	−3.001	−2.506
cov(Albacore,Water)	0.202	0.016	12.39	0.000	0.170	0.234
cov(Albacore,Solid)	0.130	0.032	3.99	0.000	0.066	0.193
cov(Albacore,own_lnPriceoz)	0.007	0.033	0.21	0.836	−0.059	0.073
cov(Albacore,_cons)	−0.674	0.066	−10.21	0.000	−0.804	−0.545
cov(Water,Solid)	0.097	0.089	1.09	0.276	−0.078	0.272
cov(Water,own_lnPriceoz)	−0.281	0.059	−4.80	0.000	−0.396	−0.166
cov(Water,_cons)	−7.002	0.243	−28.84	0.000	−7.478	−6.526
cov(Solid,own_lnPriceoz)	−0.104	0.039	−2.70	0.007	−0.180	−0.028
cov(Solid,_cons)	−0.691	0.105	−6.57	0.000	−0.897	−0.485
cov(own_lnPriceoz,_cons)	5.441	0.189	28.76	0.000	5.070	5.812

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