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*Nonmarket Valuation: Experimental Methods*

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To

The (Oxford) Handbook of the Economics of Food Consumption and Policy

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1. Introduction

As a method of research, controlled experimentation is universally used among scientists. They conduct experiments on a small scale to discover facts from which overall conclusions may be reached. In the gradual evolution of marketing research from an art to a science, some practitioners have endeavored to employ experimental methods where possible.

William Applebaum and Richard F. Spears (1950: 505)

In experimental valuation studies, participants make either consequential bids or choices with real products and real money. Test marketing is the earliest variant of such experiments. In test marketing, new products or marketing strategies are tested in a few stores or regions to expose problems that otherwise would go undetected until full-scale introduction of the product or strategy. It has primarily been used to investigate the effects of marketing mix strategies on the market shares of branded products. The results of test marketing seldom end up in scientific journals, but there are some exceptions. The first scientific article describing a test marketing experiment is Ginzberg (1936). He describes an experiment conducted by a mail-order company that distributed two versions of its catalog with different prices. Applebaum and Spears (1950) give a thorough discussion of the early test marketing methodology, and Hawkins (1957) describes a handful of the early experiments. Test marketing techniques have developed and are now a part of the toolbox used in the final stage of product development of new, branded, low-priced, and frequently purchased consumer products.¹

¹ Silk and Urban (1978) point out several problems associated with test marketing: it is time consuming and costly, researchers do not have control over changes in external factors, ideas are exposed to competitors, and it is difficult to gather information on the motivation for behavior. As an alternative to test marketing, they recommend simulated test marketing. Clancy, Krieg, and Wolf (2006) provide a review of current simulated test marketing methodology. Simulated test marketing has become a big industry with tests of about 6,000 concepts and repositionings each year. The methods used are mainly attitude, preference, and purchase intent questions combined with exposure to advertising. The results are calibrated on data from previous product launches in the same product category. However, some of the providers also use choice-based experiments as discussed later in this chapter.
During the last two decades and independent of the marketing literature, a second wave of experimental studies has been undertaken by applied economists with an interest in food consumption and marketing. Their focus has mainly been on willingness to pay (WTP), whereas the marketing literature has mostly concentrated on market shares. We address the second wave in this chapter.

When high-quality market data are available, they are usually to be preferred. However, such data cannot always be used to answer the questions of interest. As discussed in Louviere, Hensher, and Swait (2000: 21), there are several reasons why alternatives to market data may be preferred. If you want to investigate the WTP or potential market share for a new product, or a product with new features, there are often no relevant market data. If relevant market data do exist, there may be limited variability or a high degree of multicollinearity in the data. For example, competitors may match each other’s prices, leading to constant relative prices over time. New variables may also explain future product choices. For example, until new labeling requirements are introduced, we cannot use market data to predict the effects of these requirements. Finally, available market data often do not include any background information on consumers, so it is difficult to explain why they made their choices.

Alternatives to market data are stated preference and experimental data. Stated preference data are collected by using surveys with nonconsequential questions. Stated preference studies can be designed to avoid several of the problems in market data. The researcher can ask about new and nonexistent products or product attributes, vary all variables independently, and control most aspects of the study, including information, alternative products, and the sample. The main reason for the interest in experimental data is that they are generated using real economic incentives. In experimental markets using incentive compatible mechanisms, participants have real economic incentives to reveal their preferences truthfully. We are thus

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2 One could argue that experimental economists are creating markets that did not previously exist, so experimental data are really hypothetical market data.
avoiding the problem that participants are more willing to spend their money in hypothetical
than in real settings. This is a problem that has been found to be both product and context
specific (List and Shogren 1998). For a meta-analysis of the hypothetical bias problem, see

A second reason for the increased interest in experimental data is the rapid development of
new food products. In the peak year of 1995, manufacturers introduced 16,900 food and
beverage products in the USA (Nestle 2002: 25). With this stream of new food products, the
probability of failure in the market is high, and experimental studies of marketing strategies
may reduce the risk of market failure for a new product.

A third reason for this interest is that several new and controversial technologies have been
developed and adopted in food production. Such technologies include the use of hormones in
livestock production, the use of genetically modified (GM) plants, and irradiation of foods to
avoid microbiological hazards. In an environment where some consumers oppose a given
technology while others support it, policy makers have to regulate their use. WTP estimates
from experimental studies are used in cost-benefit analyses that are performed as a part of this
decision process.

A fourth reason is an ambition to test and further develop economic theories and valuation
techniques. Important examples of theoretical investigations of consumer theory include the
willingness-to-pay willingness-to-accept divergence (e.g., Shogren et al. 1994b, Plott and
Zeiler 2005), preference reversals (e.g., List 2002), coherent arbitrariness (Ariely,
Loewenstein, and Prelec 2003), and behavior under risk and uncertainty (e.g., Hayes et al.
1995). The use of experimental economics in understanding and testing consumer behavior in
general is beyond the scope of this chapter but is discussed in, for example, Davis and Holt
For further reading, Davis and Holt (1993) and Kagel and Roth (1995) provide good introductions to experimental economics. A more detailed examination of many of the auction topics covered in this chapter is found in Lusk and Shogren (2007). Louviere, Hensher, and Swait (2000) and Hensher, Rose, and Greene (2005) are good references for many design issues in attribute-based choice experiments. Kuhfeld (2009) and Street and Burgess (2007) are good references for state-of-the-art methods for creating fractional factorial design for choice experiments.

The outline of this chapter is as follows. Different incentive compatible valuation mechanisms used to evaluate food attributes are described and compared in sections 2.1 to 2.6. Section 3.1 discusses the relationships between laboratory and field experiments. Valuation mechanisms can be implemented in different ways, and section 3.2 examines the effects of differences in the implementation and suggested procedural refinements of the valuation mechanisms. Section 3.3 assesses the validity of laboratory results outside the laboratory. Some empirical results that illustrate the usefulness of experimental markets are briefly discussed in section 4. The chapter concludes with our suggestions for a “best practice” implementation of valuation experiments in food economics and marketing in section 5.

2. Incentive Compatible Valuation Mechanisms

In an incentive compatible auction, each participant’s weakly dominant strategy is to submit a bid that is equal to his or her valuation of the offered product. Incentive compatible auction mechanisms commonly used in valuation studies are Vickrey-style sealed-bid auctions with endogenously determined market prices and the Becker–DeGroot–Marschak (BDM) mechanism. In addition to these mechanisms, several choice-based methods are incentive compatible. As discussed below, each mechanism has its pros and cons, and the choice of
mechanism depends on the situation. Some features of the mechanisms are summarized in table 1, which is a revised and expanded version of table 5.1 in Lusk and Shogren (2007).

2.1. Sealed-Bid Auctions with Endogenously Determined Market Price

In a second-price sealed-bid auction, also referred to as a Vickrey auction (Vickrey 1961), the participants submit sealed bids and one unit of the good is sold to the highest bidder for a price that is equal to the second-highest bid. Second-price sealed-bid auctions have frequently been used in food valuation studies (e.g., Buhr et al. 1993, Noussair, Robin, and Ruffieux 2002, Alfnes and Rickertsen 2003).

Strategically equivalent to the Vickrey auction is the \( n \)th-price auction, also known as the uniform-price auction. In an \( n \)th-price sealed-bid auction, the participants submit sealed bids and \( n - 1 \) units of the good are sold to the \( n - 1 \) highest bidders for a uniform price equal to the \( n \)th-highest bid. The second-price auction is an \( n \)th-price auction where \( n = 2 \).

There are two arguments for choosing an \( n \) that is close to half the number of participants in the auction. First, the price will be closer to the mean WTP than in a second-price auction. This may engage low-value bidders, who may view a second-price auction as nonconsequential since they do not expect to win the auction.\(^3\) However, a high \( n \) may disengage high-value bidders, who believe that they will never lose. The empirical results in Lusk, Alexander, and Rousu (2007) are in line with these arguments. They found that a median-price auction tends to provide the highest punishment from nontruthful bidding when one jointly considers low-, medium-, and high-value bidders.

Second, some participants may view an auction as a competition that they would like to win. In a second-price auction, there is only one winning bid, whereas there are \( n - 1 \) winning

\(^3\) This is particularly true in a multiple-trial second-price auction with posted prices as discussed below.
bids in an $n$th-price auction. The increased number of winners may reduce the utility from winning.

A potential problem for using the $n$th-price auction is that it can be difficult to obtain several identical items of a heterogeneous food product. Yue, Alfnes, and Jensen (2009) circumvent this problem in their $n$th-price auction by letting the participants investigate and bid on at least as many items as there are participants in each session, and then letting each participant draw his or her unique item as binding. Studies using the $n$th-price auction include Hoffman et al. (1993) who used a fifth-price auction; Knetsch, Tang, and Thaler (2001) who used a ninth-price auction; and Yue, Alfnes, and Jensen (2009) who used a fourth-price auction.

An extension of the $n$th-price auction is the random $n$th-price auction that was used in Shogren et al. (1994a) and formally introduced by Shogren et al. (2001). In a random $n$th-price auction, the monitor randomly draws a number $n$ (between 2 and the number of participants) after the participants have submitted their sealed bids. Then, as in the nonrandom $n$th-price auction, $n - 1$ units of the good are sold to the $n - 1$ highest bidders for a price equal to the $n$th-highest bid. A random drawing of $n$ should engage all participants. However, it further complicates an unfamiliar market mechanism, and the whole experiment may look more like a lottery for some participants. Studies using the random $n$th-price auction include List and Shogren (2002), Huffman (2003), and Rousu et al. (2004).

Either an endowment or a full bidding approach is used in sealed-bid auctions. In the endowment approach, the auction participants are endowed with one product and bid on an upgrade to another product. The difference in value between the two products is estimated by the bid. In the full bidding approach, the participants bid on two or more products simultaneously. One product is chosen randomly and sold to the highest bidder(s). The other products are not sold. The difference in value between any two products is estimated by the
difference in the bids. The endowment approach goes back to work by Shogren and Hayes and colleagues in the early 1990s (e.g., Hayes et al. 1995), and the full bidding approach to work by Hoffman et al. (1993). Corrigan and Rousu (2006b) find that endowing auction participants with a good leads them to submit higher bids for the subsequent units of the same good, and recommend the full bidding approach. For a discussion of the pros and cons of the two approaches see Lusk and Shogren (2007: 65–68) and Alfnes (2009).

Most experimental auctions are conducted with repeated bidding and bids that are truncated at zero. Due to this truncation, usually double-hurdle or Tobit panel models are used to investigate treatment and socioeconomic effects on the bids. Lusk and Shogren (2007: 95–112) provide a discussion on the estimation of these models.

### 2.2. Becker–DeGroot–Marschak (BDM) Mechanism

In the BDM mechanism (Becker, DeGroot, and Marschak 1964), each participant submits a sealed bid for a product. Afterwards, a sales price is randomly drawn from a distribution of prices in an interval from zero to a price that is higher than the anticipated maximum bid. If a participant’s bid is higher than the randomly drawn price, the participant purchases one unit of the product and pays a price that is equal to the drawn price. Because the bidders do not compete against one another, the BDM mechanism is not an auction. However, it is strategically equivalent to incentive compatible auctions (e.g., Noussair, Robin, and Ruffieux 2004a).

The main advantage of the BDM mechanism is that it can be conducted with only one participant. Therefore, it is often preferred in studies conducted at the point of purchase, such as grocery stores, or other places where it is difficult to run a full-scale auction with an endogenously determined market price. Studies using the BDM mechanism include Lusk et

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4 The limits of the distribution are typically not revealed to the participants to avoid anchoring.
al. (2001b), Lusk and Fox (2003), Wertenbroch and Skiera (2002), and Nunes and Boatwright (2004). The econometric models used to estimate treatment and socioeconomic effects on the bids are similar to those used for the sealed-bid auctions.

2.3. Price List Experiments

Cummings, Harrison, and Rutström (1995) used a real dichotomous choice (RDC) method where they asked a set of consumers if they would buy a commodity at a given price. If the respondents answered “yes” they were required to buy the product for the given price, and if they answered “no”, no sales were completed. Since it is in a respondent’s best interest to say yes if and only if the price is lower than his WTP for the product, the RDC method is incentive compatible.

An extension of the RDC is the multiple price list (MPL) format used by Kahneman, Knetsch, and Thaler (1990). In their MPL experiment, each participant was given a list with the following statement: “At a price of $8.75 I will buy______ I will not buy______”, with values ranging from $0.25 to $8.75. For each value, the participant was asked to check either “I will buy” or “I will not buy”. At the end, the monitor selected one row from the list at random, and the participant’s choice for that row was implemented. This procedure is easy to implement and explain. However, Andersen et al. (2006) discuss three possible disadvantages. First, only interval and not point valuations are elicited. Second, participants can give answers reflecting inconsistent preferences. Third, participants may be drawn to the middle of the ordered table irrespective of their true WTP.

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5 In the contingent valuation literature, the hypothetical version of this is usually referred to as the payment card method.
6 For the MPL to be incentive compatible, it is important that the answers given do not affect the prices presented in subsequent choice situations. Hence, the non-hypothetical version of the double-bounded DC method (e.g., Shogren et al. 1999; Nayga, Woodward, and Aiew 2006) is not incentive compatible. In the double-bounded DC method each respondent is asked a second DC question, which depends on the response to the first question. If the first response is “yes”, the second price is some amount greater than the first bid, while if the first response is “no”, the second price is some amount smaller. In this case, someone with a small positive surplus from the first question has incentives to answer no to get a second question with a lower price.
Corrigan et al. (2009) introduced an extension of the price list format, which they called an open-ended choice experiment, where each participant was asked how many units he or she wanted to purchase at different prices. They included one existing product offered at a constant price and a novel product presented with a price list. For each price on the price list, the participants were asked to indicate the quantity demanded for the two products. At the end of the experiment, one of the prices was randomly chosen, and the participants had to buy the quantities of the two products that they had indicated. By using the quantities chosen, Corrigan et al. (2009) calculated the aggregate demand and corresponding own-price elasticity for the novel product. For storable commodities, this procedure has a problem in common with test marketing experiments. Consumers tend to stock up on storable products when the price is low and both the calculated aggregate demand and own-price elasticity may be difficult to interpret. To solve this problem in test marketing, Hawkins (1957: 437) recommended that “the test should not be concluded until the normal continuing rate of purchase has been established,” which could take several months.

2.4. Real Choice Experiments

Choice-based consumer experiments conducted in a laboratory with posted prices and real sales were introduced by Pessemier (1959, 1960). In these early experiments, each participant was told how much money he had available to spend, shown samples of merchandise, and asked to make simulated shopping trips at normal shopping speed, choosing the items that would maximize their value of the mix of merchandise and money.7 After each experiment

7 In Pessemier (1959), the participants had to state their preferred brand of toothpaste and cigarettes before they went on simulated shopping trips for these two products. For each of ten trips, the prices of the preferred brands were increased. Thus, the participants were faced with deciding whether they would continue to purchase their preferred brands or switch to some other brands. See also McConnell (1968) for a slightly different design where the value of the least preferred alternative was increased by adding pennies. As with the price list format, it is important that the choices made in real choice experiments with multiple choice sets do not affect the prices in the later choice sets.
one respondent was randomly selected to get the actual items and change called for by the selections made during one of his shopping trips.

During the last two decades, there has been a rapid development in the design and analysis of stated choice experiments. In these experiments researcher can evaluate the impacts of several attributes simultaneously. A real choice (RC) experiment is a straightforward extension of a stated choice experiment by including real economic incentives. As in stated choice experiments, participants are asked to make choices in a series of choice scenarios. In each scenario, two or more products are presented with given prices. By varying product attributes between the choice scenarios according to an experimental design, the marginal utility parameters for the various attributes, the marginal rate of substitution between different attributes, and the relative impact of socioeconomic characteristics on the valuation of the different attributes can be estimated using discrete choice methods. To induce real economic incentives, one of the choice scenarios is randomly drawn as binding, and the choices made in that scenario are implemented. The participants pay the price and receive the product chosen in the binding scenario. For heterogeneous products, one can let the participants buy the product they evaluate by including at least as many choice sets as participants, and letting each participant choose a unique choice set (Alfnes et al. 2006).

A major advantage of RC experiments is that the choice tasks are similar to the choices consumers face every day in grocery stores. A weakness compared with the auction methods is that the WTP is not directly observable, but must be estimated based on the choices. Usually, this estimation is done by using the choices of all participants. Hence, the estimated WTP for each participant is affected by the responses of other participants and sensitive to the model specification. For example, if one participant does not consider price when making his or her choices, the estimated WTP for the other participants will be affected.
Recent examples of RC experiments in food marketing research include Lusk and Schroeder (2004), Ding, Grewal, and Liechty (2005), Alfnes et al. (2006), and Olesen et al. (2010). Lusk and Schroeder (2004) let all their products be available in each choice set and only varied the prices between the choice sets, while Alfnes et al. (2006) used a fractional factorial design to vary several product attributes between the choice sets.

In an RC experiment, the participants choose their preferred product and the ranking of the other products is unknown. To obtain the complete ranking of products, Lusk, Fields, and Prevatt (2008) suggested an incentive compatible conjoint ranking mechanism (ICCRM) that was also used by Chang, Lusk, and Norwood (2009). Each participant was asked to rank different bundles of the product and cash. One of the options was a cash-only option. To make the mechanism incentive compatible, a spinning wheel divided into a number of slices of differing size was used. The product ranked first was given the largest slice, the second-ranked product was given the second-largest slice, and so on. After the ranking was completed, the wheel was spun and the participant purchased the product indicated by a fixed pointer on the wheel. An easier method for making the ICCRM incentive compatible is to randomly select two or more of the products after the ranking and make the participants buy the one given the best rank.8 The ICCRM will give a full ranking of products, but the mechanism does not resemble the choices that consumers face in stores.

2.5. Some Other Valuation Mechanisms

In an English auction, the auctioneer announces an opening price and the bids are successively raised until only one participant remains. The last participant wins the object, paying the final bid. The English auction has been extensively tested and found to perform well in the experimental economic literature (e.g., Davis and Holt 1993), but has been used in

8 Suggested by Editor Jayson Lusk.
relatively few food evaluation studies. One exception is Lusk, Feldkamp, and Schroeder (2004).\footnote{There are many variations on the English auction. In the format most commonly used by auction theorists, the price rises continuously while participants gradually quit the auction (Klemperer 2004: 11). In this format, the participant with the highest valuation will win and pay a price equal to the second-highest bid, as in a second-price auction. Except for the highest bidder, the English auction is incentive compatible, i.e., it is each participant’s weakly dominant strategy to bid his true WTP. However, the English auction format typically used in auction houses may give little information about the participants’ values. To illustrate, let us assume five bidders A, B, C, D, and E with values 50, 60, 70, 80, and 90, respectively. Assume that A starts the auction with a bid of 40, E rises to 80, and the auction stops. The information we end up with is that A has a WTP between 40 and 80, B, C, and D have a WTP between zero and 80, and E’s WTP is above 80. We may also note that Dutch as well as first-price auctions are not incentive compatible. These auction mechanisms are not incentive compatible because the price paid by the winner is his own bid, i.e., the price is not independent of the winner’s bid.}

Several studies have used mechanisms that involve real choices between products in either an experimental store or a retail setting. Maynard et al. (2004) gave each participant a budget of US$20 and a unique price schedule, and let the participants choose between different beef products. The participants could choose to purchase as many units as they wanted within their budget. This method resembles Corrigan et al.’s (2009) open-ended choice experiment, but the latter placed no restrictions on the amount of money the participants had to spend during the experiment and, furthermore, let the participants indicate the quantity demanded at several price combinations.

Three variations of choice experiments adding money to some of the offered products are described in McConnell (1968), Lusk, Norwood, and Pruitt (2006), and Nayga, Woodward, and Aiew (2006). McConnell (1968) asked the participants to make repeated choices as he increased the money added to the least preferred product, while Nayga, Woodward, and Aiew (2006) used a double-bonded choice experiment where they endowed the participants with the perceived inferior product and a random amount of cash. Next, the participants were asked whether they wanted to exchange the endowed product and cash for the perceived superior product. Lusk, Norwood, and Pruitt (2006) let participants choose between three types of pork chops with different attributes. The perceived inferior products were offered with coupons.
that could be used to pay for grocery purchases. One potential problem with letting participants choose between gifts is that some participants may view this task differently than the choice between products.

In several experiments, the participants have been endowed with a base product to investigate how much of an alternative product it takes to make the participants switch to the alternative product. These experiments measure trade-offs in quantity rather than price. Examples of such quantity trade-off experiments (QTOE) include Masters and Sanogo (2002) and Marette, Roosen, and Blanchemanche (2008). Masters and Sanogo (2002) conducted a QTOE in a market in Mali. They gave mothers, accompanied by children under two years of age, a 400-gram can of a well-known brand of infant food. Next, they offered the mothers various quantities of a lesser known brand of infant food to measure how much was needed to make the mothers switch to this brand. A similar procedure was used by Marette, Roosen, and Blanchemanche (2008) in a study of consumers’ reactions to product information concerning the level of omega-3 fatty acids and methyl mercury in canned sardines and tuna. During their choice procedure, the participants had to choose between an endowment of six cans of sardines or tuna and a variable number of cans with the other type of fish.\(^{10}\)

### 2.6. Empirical Comparisons of the Mechanisms

On a theoretical basis, the sealed-bid auctions and the BDM mechanism result in identical bids and bid differences. However, many studies find that there are differences in practice with respect to the bids in experiments using induced as well as homegrown values. Unfortunately, it is impossible to test whether bids equal homegrown values, which are

\(^{10}\) As with the MPL, it is important that the choices the respondents make do not determine whether they will get a better offer or not. As discussed by Masters and Sanogo (2002: 982), their design failed in this respect and choices were therefore not incentive compatible. Marette, Roosen, and Blanchemanche (2008) used a predetermined order of 12 choice sets on a single sheet of paper, and the choices the respondents made did not affect the subsequent choice sets.
private values that are neither controlled nor known á priori by the experimenter. This is in contrast with induced values that are controlled by the experimenter. Most induced value experiments find that sealed-bid auctions produce more accurate WTP estimates than the BDM mechanism. In an induced value experiment, Noussair, Robin, and Ruffieux (2004b) found that the second-price auction generated initial average bids closer to the induced values and, moreover, led to more rapid convergence of the bids to the induced values. They concluded that the Vickrey auction is more effective as a WTP elicitation device than the BDM mechanism. They also concluded that the second-price auction generated bids closer to true values than the BDM mechanism all along the demand curve. These results are supported by the induced value experiments reported in Lusk and Rousu (2006), who compared the second-price auction, the random nth-price auction, and the BDM mechanism. They found that, on average, the two auction formats were significantly more accurate than the BDM mechanism.

In an induced value experiment, Shogren et al. (2001) studied disengagement and found that bidders with private values below the expected market price tended to submit lower bids than their true values in a second-price auction. In contrast, a random nth-price auction induced sincere bidding also by these participants. However, the random nth-price auction did not generate more truthful bids from bidders with bids close to the market clearing price. For these bidders, the second-price auction performed better. Some people have negative WTP for new products such as GM foods. Parkhurst, Shogren, and Dickinson (2004) investigated the effects of allowing for negative bids in induced value experiments. They found that the negative bids in a second-price auction were biased, while the negative bids in a random nth-price auction were imprecise.

Several studies have compared homegrown values across mechanisms, and the results are mixed. Shogren et al. (1994a) compared the second-price to a random nth-price auction for
irradiated pork sandwiches and found no significant differences in mean values. Knetsch, Tang, and Thaler (2001) found significantly higher bids for coffee mugs in a second- than in a ninth-price auction. Lusk, Feldkamp, and Schroeder (2004) compared the second-price auction, the random \( n \)th-price auction, the BDM mechanism, and the English auction. They found that first-round second-price auction bids were similar to the bids produced by the other mechanisms. However, bids in later rounds of the second-price auction were significantly higher. Furthermore, the random \( n \)th-price auction yielded lower bids than the BDM mechanism and the English auction.

Lusk and Schroeder (2006) compared WTP values found by auctions and real choice experiments and found that the WTP is lower in auctions than in choice experiments. This result may be explained by consumers using posted prices as quality indicators, or viewing the auction as a way of buying products cheaply. The explicit focus on prices in the bidding process may also play a role.

The above discussion demonstrates some of the trade-offs between the mechanisms. First, auctions seem to perform better than the BDM mechanism in induced value experiments. However, the BDM mechanism may be implemented on an individual basis, and is easy to use in a field setting like a grocery store. Second, the choices in RC experiments resemble real choices in a grocery store, and the different varieties of choice experiments seem to work well in many situations. However, RC experiments do not give independent individual specific WTP estimates. Third, the second-price auction seems to perform best for high-value bidders, while the random \( n \)th-price auction seems to perform best for low-value bidders. Although it is impossible to make any clear recommendations concerning the choice of \( n \) in sealed-bid auctions, most recent studies seem to use an \( n \) that is higher than 2.

3. Internal and External Validity of Bids and Choices
In experiments, there is often a trade-off between context and control. An ideal study would use measures that have a low level of random errors, i.e., a high reliability, control for alternative causal explanations, i.e., a high internal validity, and give results that can be validly generalized to other populations and conditions, i.e., a high external validity.

3.1. Laboratory versus Field Experiments

It has been common in the marketing literature to define laboratory experiments as those conducted in a controlled setting and field experiments as those conducted in an actual marketplace, such as grocery stores or restaurants (e.g., McDaniel and Gates 2009). In laboratory settings, researchers have a high degree of control over factors that influence choices or bids, while in field settings it is difficult to control external factors. On the other hand, in field settings the decisions are made in a context that the participants are used to similar decisions in.

With their background in experimental economics, Harrison and List (2004) propose a refined categorization. They define laboratory experiments as experiments that employ a standard subject pool of students, an abstract framing, and an imposed set of rules. Field experiments recruit subjects in the field rather than in the classroom, use field goods rather than induced valuations, and have a field context rather than abstract terminology in instructions. Following Harrison and List (2004), field experiments can be divided into three categories. Artefactual field experiments are identical to conventional laboratory experiments but with a nonstudent subject pool. Framed field experiments are identical to artefactual field experiments but with field context in the commodity, task, or information set that the subjects use. Natural field experiments are identical to framed field experiments except that the subjects do not know that they are participants in an experiment.
Most of the experiments discussed in this chapter use real products, a growing fraction of recent experiments use nonstudent participants, and a small but growing number of experiments are conducted at the point of purchase, i.e., they are framed field experiments.

3.2. Internal Validity

The mechanisms discussed in this chapter provide incentives for the participants to make bids that are consistent with their WTP. However, several anomalies have been reported in laboratory experiments. We will briefly discuss some of these results and provide some plausible explanations for this lack of internal validity.

**Training**

Mechanisms used to elicit values may be unfamiliar to the participants. Some mechanisms may also be similar to mechanisms with which the participants are familiar, so subtle but important differences may go unnoticed. Plott and Zeiler (2005) discuss the importance of practice and training before actual bids are made. In a BDM experiment investigating the gap between willingness to pay and willingness to accept, they found that the gap disappeared when the participants were provided with (1) a detailed explanation of the mechanism and how to arrive at valuations, (2) paid practice using the mechanism, and (3) anonymity so participants are not concerned with how they are viewed by the other participants or the experimenter.

**Bid Affiliation and Posted Prices**

Induced value experiments have shown that it takes several rounds of auctions before behavior conforms to the theoretical predictions of WTP bidding (e.g., Noussair, Robin, and Ruffieux 2004b). To let participants understand the auction mechanism and discover their
“true” homegrown values for the nonfamiliar good, auctions with multiple trials have been commonly used in studies of homegrown values (e.g., Hoffman et al. 1993, Shogren et al. 1994b, Fox et al. 1998, Alfnes and Rickertsen 2003). In multiple-trial auctions for nonfamiliar goods, it is a common observation that bids increase over the trials. Increasing bids are likely because some participants may try to buy at a low price even when they are told that the optimal strategy is to bid their true WTP. Repeated rounds with price feedback send strong signals to these participants that bidding low, in the hope of buying at a low price, is not an optimal strategy. However, the price feedback may also send signals regarding unknown characteristics of the good and thereby cause bid affiliation, i.e., bids in later rounds may be affiliated with posted prices from earlier rounds (Lusk and Shogren 2007: 82). In this case, mechanisms that are demand revealing in induced value settings may not be demand revealing in settings with homegrown values (Harrison, Harstad, and Rutström 2004). If the participants are uncertain about the quality of a product, they may infer information about the value of the product by observing other participants’ bids so they can revise their own bids in subsequent rounds. Hence, they are not basing their bids solely on the values they had when they entered the auction.

Empirical evidence suggests that bid affiliation is a problem. Examining panel data on bidding behavior in over forty second-price auctions, List and Shogren (1999) found some bid affiliation for novel but not familiar products. However, the influence of affiliation on the median bids was small. Furthermore, Lusk and Shogren (2007: 80–92) point to a series of results in the literature and argue that bid affiliation is a minor problem in homegrown value experiments. Corrigan and Rousu (2006a) developed an experiment to test explicitly for the effect of affiliation by using so-called confederate bidders who were instructed to place bids that were much higher than the other participants’ WTP. Contrary to List and Shogren (1999)

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11 One exception is Knetsch, Tang, and Thaler (2001) using a ninth-price auction, where the bids decreased.
and Lusk and Shogren (2007), they found that for familiar items such as candy bars and coffee mugs, high posted prices lead to increased bids in subsequent rounds. Even though the very high bids of the confederate bidders introduced a hypothetical element into the auction, the results clearly demonstrate that bid affiliation is potentially a problem. Their conclusion is also supported by Harrison, Harstad, and Rutström (2004), who analyzed the data used in the seminal paper of Hoffman et al. (1993) and found statistical evidence of bid affiliation.

The interrelationship between auction mechanism and panel size is likely to affect the posted prices, and thereby the bids through bid affiliation. If there are many participants and few winners, the posted prices will be high. If there are few participants and relatively many winners, the posted prices will be low. Alfnes, Rickertsen, and Ueland (2008) argue that the use of an \( n \)-th price auction will reduce the affiliation problem by reducing the likelihood of price feedback that is much higher than the average bids, such as those found in Corrigan and Rousu (2006a). Finally, a statistically significant positive effect of panel size on the bids in a fourth-price auction is reported in Umberger and Feuz (2004).

There is no agreement in the literature on how to handle bid affiliation. Lusk and Shogren (2007) argue that it is a minor problem, while Harrison, Harstad, and Rutström (2004) conclude that one-shot auctions should be used. The latter recommend training participants in the auction mechanism by using another good such as chocolate. However, given the effects of incidental prices and coherent arbitrariness, as discussed below, the use of a different training product or procedure may not alleviate the problem. Most recent papers seem to use an \( n \)-th price auction with a relatively low number of rounds, but more than one.

*Field Substitutes and Outside Options*

Most food products presented in experimental markets have field substitutes. In food experiments, these field substitutes will typically be food items that can be purchased in the
As pointed out by Harrison, Harstad, and Rutström (2004), no rational consumer will purchase a product in an experimental market that costs more than the price of the good in the market. In induced value auctions, this leads to bids truncated at the external price of the good (Cherry et al. 2004). In homegrown value experiments, the field substitutes to the product offered in the experiment may be a different product. As pointed out by Alfnes (2009), somebody that does not buy fish in an experimental fish market may end up buying chicken, pork, or beef for dinner that day instead of going to the fish market to buy fish. Hence, the real outside option affecting the optimal bids or choices in a fish experiment may be the consumer surplus from purchasing a nonfish alternative in the market. Alfnes (2009) set up a theoretical model for optimal bidding in a Vickrey auction with outside options and concluded that the effect of outside options on the optimal bidding strategies for food products is a problem for the bid levels, but not for the bid differences. Bernard and He (2010) examined the influence of field prices on bids by conducting auctions before and after the large food price increases in 2007. They found that bids were capped at given field prices and were significantly higher in sessions conducted after store prices increased. Percentage premiums, however, were not significantly different across sessions, and they concluded that the full bidding approach could still be used as long as bid differences are the focus, rather than bid levels.

Anchors, Coherent Arbitrariness, Incidental Prices, and Relative Bids

Empirical evidence suggests that participants use numbers that are presented to them as a part of the experiment as anchors for their stated WTP. Drichoutis, Lazaridis, and Nayga (2008) found that provision of reference price information, such as the field price of sandwiches, significantly affected the bids. Other examples of similar anchors include the distribution of prices in a price list experiment, the limits of the price distribution if revealed to the
participants in a BDM mechanism experiment, bids in training sessions, posted prices in repeated auctions, and prices in the first choice sets in choice experiments.

Arbitrary numbers presented to the participants may also serve as arbitrary anchors and affect the bids. For example, Nunes and Boatwright (2004), who investigated the effect of incidental prices on participants’ bids in experimental auctions, found that exposing the participants to prices of goods that were completely unrelated to the goods in the auction had a substantial effect on the bids. Similar conclusions are supported by Ariely, Loewenstein, and Prelec (2003), who demonstrated how consumers’ absolute valuations of ordinary products are affected by anchors such as the participants’ social security numbers. In one experiment, they asked participants to write down their social security numbers before they started the experiment. Participants with a high number bid more than participants with a low number in the following auction. However, consumers’ relative valuations of different products appeared to be coherent, i.e., as if supported by demand curves derived from stable preferences.

The presence of random anchors has two implications. First, training participants by using a good other than the good of interest may be no better than using the good itself. Second, relative WTP estimates derived using the full bidding approach are likely to be more stable than the bids themselves. In the full bidding approach, it is likely that random anchors affect the bids for all the goods in a similar way. The results in Umberger and Feuz (2004) confirm that experimental auctions succeed in determining the relative WTP values for close substitutes, while the actual bids are influenced by the experimental design. In situations with many zero bids, many observations will be lost by using relative bids, and bid differences may be used. In line with this reasoning, Hoffman et al. (1993) claimed that the most reliable estimates from experimental auctions are bid differences. Furthermore, Alfnes and Rickertsen (2003) found that bid differences calculated from bids that are increasing across trials are
stable. Finally, the results in Lusk, Feldkamp, and Schroeder (2004) indicate that bid differences are uninfluenced by the number of goods included in the auction.

3.3. External Validity

The validity of experimental results for actual retail behavior is important. The external validity of an experiment deals with the ability to generalize the results to other settings, treatments, measures, subjects, and occasions. External validity is increasingly the focus of experimental studies, and several articles have recently discussed why laboratory and field behavior may differ (e.g., Harrison and List 2004, List 2006, Levitt and List 2007). Low external validity of lab results can be caused by factors including (1) unfamiliar environment, (2) the nature of the decision task, (3) the participants not being representative, (4) the available information and attention given to information, (5) the presence of researchers that scrutinize participants’ behavior, and (6) high bids that do not necessarily imply repeated purchases. Further research is needed to identify particular boundary variables and crucial differences between laboratory and field settings affecting the external validity of experiments.

Nature of Environment

Most economic experiments take place in a laboratory or a classroom. However, a laboratory may be an abstract and unfamiliar environment for many participants, who may behave differently than they would in a retail setting (Levitt and List 2007). Furthermore, the participants do not take part in the experiment because they need to buy food, and the time and place of the experiment are decided by the researcher. In line with these arguments, Corrigan and Rousu (2008) found that consumers that intended to purchase bananas on the day of the experiment gave WTP values that were closer to the perceived field price of
bananas than those that did not intend to purchase bananas that day. Hence, even regular buyers of the product may behave as nonbuyers in an experiment. This can especially be a problem with highly perishable products that must be consumed within a short period of time after the experiment. Valuation studies conducted in a store setting may solve these problems. However, there are limited possibilities to train participants or collect background information in stores.

Nature of Decision Tasks

The decision tasks in an experimental market and a grocery store are typically quite different. The experiment usually includes only a small number of products, and the participants are usually only allowed to buy one unit of one product and no units of the other products. In an auction, the participants are asked to state the highest amount they are willing to pay for a product, instead of choosing among pre-priced products. In the choice-based experiments, the price of the products is varied within the experiment. Furthermore, some design features used in experimental markets may resemble lotteries. For example, most experiments involve random draws.

Representative Samples

Self-selected student samples are frequently used to keep the costs of experiments down. Such convenient samples are likely to be nonrepresentative of consumers in the food market. Student samples typically have lower age, lower income, and more education than the general population. The biases introduced by using student samples are likely to be product specific, and have rarely been investigated. One exception is Depositario et al. (2009), who used a fifth-price auction to investigate the WTP for golden rice among student and nonstudent participants in the Philippines. The mean bid of the nonstudent group was about 15% higher
than the mean bid of the student group, and the difference was significant at the 5 percent level of significance.

Nonstudent samples may also be unrepresentative. Participants from one region may not be representative of the whole market. For example, Sawyer, Worthing, and Sendak (1979: 64) question the possibility of extrapolating their result for maple syrup collected in Massachusetts to “subjects outside the maple producing region of the Northeast and Great Lakes”. Alfnes and Rickertsen (2007) try to mitigate the problem with lack of geographic representativeness by using a national stated choice survey to extrapolate the experimental results to other areas.\textsuperscript{12} Samples recruited from the general population may also be nonrepresentative in other ways. In many families, one person has the main responsibility for food purchases, and this person should take part in the experiment. For many food products, the top ten percent of users by volume buy almost all the units sold of the product, and their preferences may differ significantly from the general population.

\textit{Information}

In grocery stores, consumers make many choices based on habits. They do not usually read the product description and often pay little attention to the price of the product. In experimental markets, consumers are usually exposed to various types of explicit information, either included in a PowerPoint presentation or on a sheet of paper that is given to them. Furthermore, the price usually plays a very prominent role in the experiment. In auctions, consumer must state their reservation price, and in the various choice-based methods, the prices are varied through the experiment. Finally, smaller sample sizes are required for

\textsuperscript{12} The method used in Alfnes and Rickertsen (2007) may also be seen as a method for calibrating the survey results to get WTP estimates that are consistent with experimental markets. The article includes a review of other studies estimating similar calibration functions for survey results. See Alfnes, Yue, and Jensen (2010) for an example of how auctions can be used to alter the behavior in surveys so they give results similar to experimental markets.
within-sample than between-sample tests, and therefore within-sample tests are popular among researchers. However, the within-sample design draws attention to the treatment in the experiments, and has repeatedly been found to give larger effects than the between-sample design (e.g., Johansson-Stenman and Svedsäter 2008).

Presence of Researchers
In a typical laboratory experiment, the participants are well aware that they are part of an experiment and that someone will examine their behavior. This scrutiny is likely to affect participants’ behavior. The participants may try to behave as they think the experimenter wants them to behave, or may try to give a socially desirable impression of themselves (e.g., Sawyer 1975, Levitt and List 2007, Lusk and Norwood 2009). Social desirability bias is most likely to be a problem in studies examining product attributes with a social dimension such as environmentally friendly, animal friendly, fair trade, or locally produced.

Repeated Purchase
Many studies find a relatively high price premium for new food products, however, a high premium does not necessarily result in repeated purchase of the food. Shogren, List, and Hayes (2000) discuss the origins of the high price premium. They investigated two possible explanations. First, experimental auctions are nonfamiliar for the participants, and the novelty of the experimental experience could result in high bids. Second, the novelty of a food product could result in high bids. Many participants may be willing to pay a premium for testing a new product, and the bid will reflect the consumption value of the good as well as the information value of learning how the product fits into their preferences.\(^{13}\) For a

\(^{13}\) By studying three goods that varied in familiarity, candy bars, mangos, and irradiated meat, and doing four consecutive auctions over two weeks, they concluded that the main explanation for the high price premium in their experiment was the value of preference learning.
theoretical discussion of the role of the information value in experimental auctions of new products, see Alfnes (2007).

*Empirical Results on External Validity*

Some studies have investigated the external validity of experimental methods. Brookshire, Coursey, and Schulze (1987) could not reject that the valuation of strawberries was identical in the laboratory and in door-to-door sales. Shogren et al. (1999) compared valuations elicited by a hypothetical mail survey and a nonhypothetical laboratory valuation with grocery store purchases of irradiated chicken. Their results concerning the external validity of the valuation exercises were mixed. Chang, Lusk, and Norwood (2009) used a hypothetical choice experiment, an RC experiment, and the ICCRM to predict retail market shares for different goods. Their results suggested that the nonhypothetical methods outperformed the hypothetical experiment while the ICCRM performed slightly better than RC. Also Ding, Grewal, and Liechty (2005) found that elicitation mechanisms, which properly aligned incentives, predicted behavior better than nonincentive aligned mechanisms.

Some further evidence of external validity with respect to existing products can be found in Lusk and Shogren (2007: 261). Studies conducted by industry providers are not usually published in scientific journals (Clancy, Krieg, and Wolf 2006).

4. Empirical Results

This section briefly reviews some empirical results that illustrate the usefulness of experimental markets. Detailed discussions of the valuations of specific attributes in different food products are provided in other chapters of this book, and case studies further illustrating the applications of experimental auctions are provided in Lusk and Shogren (2007).
Food marketing issues such as packing technologies (e.g., Menkhaus et al. 1992, Hoffman et al. 1993), the use of insecticides (Roosen et al. 1998), the use of different types of feed (Alfnes et al. 2006), and grading for tenderness (Lusk et al. 2001b, Alfnes, Rickertsen, and Ueland 2008) have been analyzed. A general conclusion is that there are segments of consumers with different preferences and WTP. Sometimes little or no difference between the WTP for the old and new product is found, however, usually some increases in the mean WTP for the improved product are reported. Typically, studies dealing with attribute-based food marketing limit themselves to calculating the consumers’ WTP for new products without addressing the bigger issue about the profitability of introducing them.

Some production technologies such as genetic modification are highly controversial (e.g., Lusk et al. 2001a, Huffman 2003, Noussair, Robin, and Ruffieux 2002, 2004a, Lusk et al. 2004), as are hormone treatment in livestock production (e.g., Buhr et al. 1993, Fox et al. 1994, Alfnes and Rickertsen 2003, Bernard and Bernard 2009), irradiation (e.g., Fox et al. 1998, Shogren et al. 1999, Shogren, List, and Hayes 2000), and use of antibiotics in feed (Lusk, Norwood, and Pruitt 2006). Controversial products have frequently been studied, and the results suggest that the average US participant views irradiation favorably, whereas there is substantial opposition, especially in European countries, towards genetic modification, use of antibiotics in feed, and hormone treatment. Furthermore, the preferences concerning these products are highly heterogeneous, again demonstrating the importance of market segmentation.

Labels and brands are used by food producers to highlight product attributes. Examples of experimental studies of high-value brand names are Lange et al. (2002) and Combris, Lange, and Issanchou (2006), who assessed the effect of product labeling information on the WTP for five brut nonvintage Champagnes. They found that the participants were unable to discriminate between the Champagnes after blind tasting, while significant differences in
WTP appeared when the labels were disclosed. These results led the authors to conclude that it may be more profitable for the big champagne houses to invest in marketing than in quality improvements.

As discussed above, GM foods are controversial, and are required to be labeled as such in many countries. The effects of GM labeling may be complex, as illustrated by Noussair, Robin, and Ruffieux (2002), who studied the WTP for GM corn flakes. They found that when participants observed the products with the labels as seen in the supermarket, labels with information concerning the content of GMO had no effect on the WTP. However, when the list of ingredients was presented on large overheads, the average WTP for the GM product was reduced by 27%, suggesting that consumers do not automatically react to information provided by labels. Labeling of a product as GMO free or not hormone treated may also stigmatize the conventionally produced counterpart, as discussed by Kanter, Messer, and Kaiser (2009). They found that the introduction of hormone free and organic milk reduced US consumers’ willingness to purchase conventional milk.

Several studies have focused on the effects of scientifically balanced information, and moderate effects are usually reported (e.g., Fox et al. 1994, Roosen et al. 1998, Noussair, Robin, and Ruffieux 2004a, Lusk et al. 2004). However, the effects of information depend on the content as well as the source of the information. Substantial effects of information with either an environmental group perspective or an agricultural biotechnology industry perspective are reported (e.g., Huffman 2003, Rousu et al. 2004, Rousu et al. 2007). Furthermore, Fox, Hayes, and Shogren (2002) found that the effects of negative information dominate the effects of positive information.

It is quite common to let participants taste the product in evaluation experiments. Significant effects of tasting are frequently found (e.g., Melton et al. 1996, Alfnes and
Rickertsen 2003), and several studies have reported a strong correlation between taste scores and WTP (e.g., Lange et al. 2002, Kanter, Messer, and Kaiser 2009).

5. Conclusions

Some recommendations concerning the implementation of valuation experiments in food economics and marketing emerge from the previous sections. These recommendations are “best practice” advices, and we acknowledge that some of the advices may be controversial. Furthermore, a researcher may sometimes end up adopting second-best practice. Our recommendations are as follows.

1. Do not deceive participants or lie to them. Any information about the valuation mechanism and products should be true. Lies can affect behavior in experiments, and can have negative externalities on future experiments and other experimentalists. That does not mean that you have to reveal everything, but what you say should be true.

2. Use representative consumers. The researcher has to decide whether to use a sample of students or representative consumers, and to what extent the participants should be screened based on their consumption of the good. If the objective of the experiment is to test theory or compare mechanisms, student samples are usually satisfactory. However, students differ from other consumers. They are typically younger, have less money, are frequently without children, and so on. These differences are likely to result in different preferences concerning sensory qualities of food, different attitudes towards technology, environment, and animal welfare, and different sensitivity to price changes. We also recommend using a minimum consumption constraint to screen the participants. There is little point in including vegetarians in an experiment about beef quality. Including participants that consume the relevant product class once or twice a month will usually work.
3. **Make sure the participants understand the mechanism.** Theoretically incentive compatible mechanisms are only incentive compatible in practice when the participants understand how their choices or bids affect their outcomes in the experiment. Therefore, use mechanisms that are easy to understand, and avoid design elements that may make the experiment look like a lottery. Furthermore, clearly inform the participants about their best strategy. This is especially important in mechanisms that are not choice based. Finally, train the participants using exactly the same mechanism and the same number of goods as in the main experiment.

4. **Calculate relative WTP values.** According to microeconomic theory, only relative prices matter. Furthermore, given that anchoring and treatment effects affect the relative values of the included goods identically, they will cancel out. In experimental auctions, zero bids may make it difficult to calculate all the relative WTP values, and WTP difference may be used as a second-best option. This recommendation also implies that testing of value elicitation procedures should include tests of the relative valuations.

5. **Use a context free of scrutiny.** People act differently when they feel they are being observed, and it is important to create an environment where the participants feel relaxed and not scrutinized. The participants should be assigned a random number to identify themselves within the experiment, the forms should be completed in a room that allows for some privacy, and the choices made by the participants should be private, as far as possible.

6. **Use mechanisms with strong real economic incentives.** There are differences with respect to the strength of the incentives among the incentive compatible mechanisms. For example, in a second-price auction with 15 participants, a majority of the participants may sometimes change their bids substantially both upwards and downwards without affecting the outcome of the auction. In a median-price auction, relatively small changes may affect the outcome for many participants.
7. **Collect background information.** For producers, policy makers, and researchers it may be useful to have information about the WTP in different segments of the population. The background information should include socioeconomics, knowledge, attitudes, and purchase behavior.

8. **Let participants taste unfamiliar products.** If consumers prefer the taste of a new food product compared with the competing products in the same price range, then they are likely to adopt it. Tasting will also reduce any sensory uncertainty that may result in either lower WTP due to risk aversion or higher WTP due to people wanting to taste the new product. As a consequence, tasting is likely to improve the long-run external validity of the results of the experiment.

9. **Treat all products equally.** There is a large and growing literature on treatment and framing effects in economic as well as sensory experiments. To mitigate these effects, all alternatives should be treated equally. Equal treatment includes rotating the order in which products are presented between sessions, allowing participants to taste either all or none of the products, using the full bidding format in experimental auctions, and using a fractional factorial design with product ordering as one of the attributes in choice-based experiments.

10. **Delete participants with a nonresponse to all alternatives.** Nonresponse to all alternatives implies that the participant bids zero for all products or chooses none of the alternatives in all choice scenarios. Participants with nonresponse to all the alternatives do not reveal anything about their relative valuations of the products included in the experiment. There may be several reasons for nonresponses, including the possibility that the participant has already bought a similar product, finds it inconvenient to bring anything from the experiment, does not trust the people who run the experiment, or does not like any of the products.
11. *Let participants evaluate multiple items of heterogeneous products.* Unprocessed food products such as fish, meat, and vegetables frequently differ greatly in appearance. To avoid deception, the evaluated and sold products should be identical. This can be implemented for heterogeneous products by letting the participants bid on as many items as there are participants, and then letting each participant draw one item as binding. In choice experiments, there should be as many choice sets as there are participants, so each participant can draw a unique binding choice set.

**References**


Table 1. Some Incentive Compatible Mechanisms

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<th>Rule</th>
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<td>English auction</td>
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<td>Simultaneously submit sealed bids</td>
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<tr>
<td>n&lt;sup&gt;th&lt;/sup&gt;-price auction</td>
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<td>Choose alternatives in multiple scenarios</td>
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<td>Everybody pay</td>
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<td>Rank alternatives in multiple scenarios</td>
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<td>Everybody pay</td>
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