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Toward more general hedonic estimation: Clarifying the roles of alternative experimental designs with an application to a housing attribute☆

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ABSTRACT

Traditional hedonic estimation approaches are known to be biased when exogenous shocks affect multiple product attributes, the market for the product's complements and substitutes, and aggregate quantity produced. Our research develops a more general hedonic model to recover the marginal willingness to pay for an attribute in the presence of such known hazards to identification based on randomized experiments. Three experimental approaches are introduced on how to estimate attribute demand that address known biases, have transparent identification assumptions, and are feasible to implement. We apply one of the estimators developed to measure the marginal value placed by householders on subsidized carbon monoxide detectors.

1. Introduction

Hedonic estimation and the measurement of marginal willingness to pay (MWTP) for product attributes are vital tools for quantifying the benefits of public policies that improve safety, environmental, school, or health care quality (Black 1999; Chay and Greenstone 2005; Cutler, Rosen, and Vijan 2006; Viscusi 1993, 1996). Hedonic methods are used to understand the demand for heterogeneous goods such as automobiles, computers, food, housing, and jobs (Bajari and Benkard, 2005; Hamermesh 1999; Kiesel and Villas-Boas 2007; Raff and Trajtenberg 1995; Sheppard 1999). They are also used to calculate the Consumer Price Index and one fifth of expenditures in the Gross Domestic Product (Landefeld and Grimm 2000; Moulton 2001). For the purposes of measurement and policy evaluation it is desirable to have robust hedonic estimators whose empirical results are correct generally. Our research demonstrates the identifiability of MWTP without the strong econometric restrictions often applied in earlier applications and presents straightforward estimators of MWTP and related measures for use in experimental empirical settings.

A cursory reading of the hedonics literature might yield the impression that MWTP cannot be identified without imposing highly restrictive assumptions about the equilibrium price function, even when a natural experiment is available. Models adopted often assume that

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unobserved product attributes either are uncorrelated with observed ones, or do not exist (Berry et al. (1995); Epple 1987; Rosen 1974). In addition, the adopted models generally assume that the product of interest has no complements or substitutes, so that a location-specific attribute, like weather, cannot affect the labor market and housing market simultaneously. Finally, adopted models also typically specify aggregate quantity consumed as exogenous and unresponsive to price changes.[1]

There is a widespread belief in the literature that the above restrictions are appropriate and necessary to estimate MWTP. Earlier applied studies of heterogeneous goods generally employ slight modifications of the hedonic frameworks, or measure reduced-form price effects without estimating MWTP directly. More recent empirical work in hedonic estimation focuses on quasi-experiments, and some innovative studies have incorporated quasi-experimental variation into existing hedonic models (Bayer, Ferreira, and McMillan 2007; Berry and Haile 2010; Boes and Nüesch 2011; Chay and Greenstone 2005; Klaiber and Smith 2009; Kuminoff and Pope 2012, 2014; Lewbel 2000; Parmeter and Pope 2013; Pope 2008a, 2008b).[2] To our knowledge, no previous hedonic frameworks simultaneously allow for unobserved product attributes that are affected by exogenous shocks, complementarity with the good of interest, and aggregate quantities that vary.[3]

Randomized experiments have become common in the economics literature to address possible biases when estimating important economic parameters (e.g., Bertrand and Mullainathan, 2004; Hanson and Hawley, 2011; Kling, Ludwig, and Katz, 2005; Landry, et al., 2006; Manning, et al., 1987). Such experimental approaches have not been widely adopted within the urban economics and hedonic literatures. In this study we describe the potential benefits of using randomized experiments to correct for biases that may arise when using traditional hedonic estimation strategies. We provide a theoretical framework and discuss the practical approaches of how researchers may utilize randomized experiments to more clearly identify MWTP, with particular attention to the urban economics field of study.

In what follows we first provide an intuitive discussion of the types of biases that endogenous omitted attributes, complement and substitute goods, and aggregate quantity effects generate in traditional hedonic approaches. Next, we present experimental estimators to address the biases. Of the estimators presented, we start with estimators with the least restrictive modeling assumptions, but have the most demanding data requirements. The modeling assumptions become more restrictive and the data requirements less demanding with successive estimators. We then focus on developing nonparametric experimental estimators that identify the entire distribution across consumers of the demand for a given product attribute. In particular, we present experimental estimators of the aggregate demand for a product attribute among a population of consumers. The experimental estimators we develop avoid the effects of endogenous omitted attributes and complement and substitute goods by offering products and subsidies to consumers.

It is important to emphasize that the estimators we develop here rely upon straightforward, transparent identification conditions that are feasible to implement in future research. Variations on the estimators have been previously applied in recent studies to estimate the value of freedom from jail, the demand for avoiding the Vietnam draft, the value of a statistical life, and the demand for class size reductions in elementary school (Abrams and Rohlfs 2011; Rohlfs 2012; Rohlfs, Sullivan, and Kniesner 2015; Rohlfs and Zilora 2013). The new class of experimental hedonic estimators, however, has not been widely applied within a housing context, where researchers often adopt hedonic estimators with the strongest econometric restrictions. As a final exercise, we illustrate how one of the proposed estimators could be used to estimate the marginal willingness to pay for a housing attribute. More specifically, we conducted a small-scale field experiment that randomly subsidized the price of carbon monoxide detectors offered to participants.

2. Discussion of possible bias in hedonic models

Previous hedonic and discrete choice research by Rosen (1974); Epple (1987); and Berry et al. (1995) discuss concerns with the types of bias which appear in the framework described here. Those models assume that a consumer purchases a single unit of a heterogeneous good represented by a vector $\mathbf{z}$ of characteristics z_k and spends remaining income or wealth on a homogenous consumption good. Traditional Rosen (1974) style hedonic models can effectively be decomposed into three primary steps:

(i) Estimate the price function and gradient for attribute z_k, $P(z_k)$.

(ii) Assuming the market is thick and that agents are optimizing, the gradient and a first order condition can be used to recover marginal willingness to pay (MWTP) at the point of consumption, $p'(z_k^*) = \text{mwtp}(z_k^*)$.

(iii) Estimate the full MWTP function, $mwtp(z_k)$.

Typically, the literature refers to the combination of (a) and (b) as Rosen's first stage and (c) as Rosen's second stage. Our paper primary focuses on feasible methods to estimate the marginal willingness to pay at the point of consumption or Rosen's first stage. In addition, we offer some secondary information on how researchers might use these methods to estimate the entire MWTP function under certain conditions.[4] Given the sources of biases that may arise in traditional hedonic estimation strategies, it is important for researchers to understand how to correct these deficiencies using transparent estimation methods that are feasible to implement.

To illustrate the sources of biases that our research seeks to addresses, let P_{ht} be the average price of house h in year t. Let z_{1ht} be an observable attribute about house h, such as local school quality. Next, let the value of z_{1ht} be determined by a quasi-experiment so that it varies exogenously across locations and over time. Let z_{2ht} be an attribute about house h that is difficult to measure, such as the pleasantness of neighbors in the area. Finally, let P_{ht} be a linear function of the two attributes and an error term denoting unobserved attributes:

$$P_{ht} = \beta_0 + \beta_1 z_{1ht} + \beta_2 z_{2ht} + \varepsilon_{ht} \tag{1}$$

The aim of a hedonic price regression in this case is to identify β_1, the effect of attribute z_{1ht} on housing prices, holding all other attributes constant. Once identified, the hedonic price effect is used in a second-stage procedure to estimate MWTP for z_{1ht} (Epple, 1987; Rosen, 1974).[5] For the second stage to produce accurate estimates, the estimates from the first-stage hedonic regression (1) must be consistent.

[1] Rosen (1974) and Epple (1987) additionally require that markets are sufficiently thick so that every conceivable product is available and that supply is competitive. Berry et al. (2005) additionally requires specific functional forms for utility and firm costs, plus a specific distribution for heterogeneity in preferences.

[2] Some recent theoretical studies relax the functional form assumptions from earlier models but leave the frameworks largely intact elsewhere (Athey and Imbens 2007; Ekeland, Heckman, and Nesheim 2004; Heckman, Matzkin, and Nesheim 2010).

[3] Roback (1982) allows for one type of complementarity (housing and jobs), and Sieg et al. (2002) include area-specific dummy variables to proxy for the areas' job quality and public goods. Berry et al. (2005) allow for market shares (but not aggregate quantity produced) to vary. No single framework has addressed more than one of the biases simultaneously.

[4] In order to conduct non-marginal analyses it is desirable to recover the entire MWTP function and not just the value of that function at the point of consumption which is why provide some additional information on this topic. This is perhaps the area of most discussion about identification and estimation with many claims and counter claims made in papers such as Mendelsohn (1982), Bartik (1987), Epple (1987), and Ekeland, Heckman and Nesheim (2004).

[5] The procedure proposed by Berry et al. (2005) is different from that described here, but BLP require consistent estimation of the effect of the attribute on the decision to purchase the product.

Suppose the assignment of z_{1ht} is random and consequently uncorrelated with any predetermined factors that influence P_{ht}. An improvement in school quality, however, may cause affluent and more educated people to move into the area. If being affluent and educated is correlated with being a pleasant neighbor, then the school quality improvement will directly affect the pleasantness of neighbors. In other words, z_{2ht} is an equilibrium outcome and itself a function of z_{1ht}. If z_{2ht} is not included as a control in the regression, OLS estimates of (1) would only measure the reduced-form effect of the shock to school quality on P_{ht} and not the structural parameter β_1. The reduced-form effect includes the direct effect of z_{1ht} and the indirect effects of z_{1ht} through the mechanism of z_{2ht}.[6] Hence, OLS produces a biased estimate of β_1, and the magnitude of the bias is $\beta_2 * cov(z_{1ht}, z_{2ht})$. Even if data were available on z_{2ht}, the values of the other (z_{1ht}) attribute were not assigned experimentally and are likely to be correlated with ε_{ht}. Consequently, without an instrument for z_{2ht}, an OLS regression of P_{ht} on z_{1ht} and z_{2ht} will produce biased estimates of β_2, and by failing to adequately control for z_{2ht}, the regression will also produce biased estimates of the effect of z_{1ht}.[7]

However, the pleasantness of neighbors is not the only characteristic of additional concern. For example, many location-specific attributes are also influenced by the composition of local residents and businesses. Such neighborhood attributes will vary in response to any exogenous shock that causes consumers or firms to move. Similar biases arise in the markets for labor and schooling, where some workers' and students' behaviors affect the quality of the environment experienced by other workers and students.[8] In general, z_{2ht} should be treated as an endogenous variable that may change in response to a shift in z_{1ht}.

A similar form of bias arises within a hedonic setting if goods that are complements or substitutes with housing are ignored. One key interaction is between the markets for housing and labor. Consumers often decide where to live based upon job availability, and the types of jobs that are available in an area affect housing prices and the types of people who live there. Additionally, consumers who are considering buying a home in an area may also consider the local price level and the quality and variety of local goods. Hence, local job characteristics and local prices are location-specific attributes that should be included in (1). In addition to being difficult to measure in an exhaustive way, locational factors are probably correlated with ε_{ht}, and adequately controlling for them may require finding yet another credible instrument for each one.

As discussed above, the price of housing may vary at a local level because of local labor markets or amenities. This is a well-recognized issue in the urban economics literature and is typically addressed with the use of neighborhood fixed effects in hedonic regressions. Even when fixed effects are included in the regressions, there may still be some concerns with producing consistent estimates. For example, Abbott and Klaiber (2011) argue that the inclusion of spatial fixed effects does not consider the biases introduced by effects that overlap the zone of capitalization for nonmarket goods. If ignored or not adequately controlled for, these local effects can cause considerable bias in the coefficient estimates.

In the housing market, a minor extension to the conventional models would allow consumption to consist of multiple homogeneous goods such as the comparison of those purchased in Atlanta and as compared to in Boston. A home buyer would certainly take the price level into account when choosing where to live, and a change in weather or school quality in one of the cities could affect local prices for the homogeneous good as well as local housing prices. The full market response to the weather or school quality shock includes both the effect on housing prices, and the effect on local prices for everything else.

Complementarity also occurs between jobs and housing in the same location. A firm hiring in Syracuse, New York (which receives 125 in. of snow per year) must pay a higher wage to obtain the same level of talent than does a similar firm hiring in San Francisco (which has year-round pleasant weather). In the current model, one of the goods could be hours of work in a specific job in Syracuse, and another good could be hours of work in a specific job in San Francisco. Each consumer would be endowed with quantities of time that could be sold to the employer or consumed as leisure. The shape of the utility function would be such that selling hours to the firm in San Francisco (and failing to consume them as leisure) would greatly increase the utility benefit of housing in San Francisco. The two situations would also be strong substitutes in the utility function, so that selling hours of work in San Francisco would sharply increase the utility cost of selling hours of work in Syracuse, and working jobs in both cities would be rare.

In addition to ruling out substitutes and complements, some previous hedonic models require that each consumer purchases exactly one unit of the good whose characteristics are under study. In the market for housing, this restriction rules out any effects of prices or location-specific attributes on the decision to buy a home or the total number of home buyers. In addition to allowing for endogenous homeownership, relaxing the restriction helps to describe markets in which consumers often buy more than one good whose characteristics are important, such as automobiles and computers, or markets in which quantity consumed various continuously, as in foods, music, and vacations.

We propose to address the problem of endogenous variables experimentally. Specifically, we propose for the researcher to offer randomized products and subsidies to individual consumers to measure their response. The estimated coefficients resulting from the experiments, as discussed in the next section, should provide consistent estimates for researchers in comparison to previous, possibly biased estimation strategies.

3. Identification of MWTP densities

3.1. Experimental estimator 1: An idealized experiment

We begin our discussion of identification with an idealized experiment where the researcher applies a so-called *treatment technology* and charges randomly assigned prices for that technology to different consumers. Specifically, a treatment technology, $T_{\mathbf{Z_T}k}$, converts any bundle q_z of goods in $\mathbf{Z}$ (the set of all conceivable goods in the market of interest) into a new bundle $T_{\mathbf{Z_T}k}(q_z)$ of goods in $\mathbf{Z}$ where every unit of every good in the treated segment $\mathbf{Z_T}$ is replaced with the equivalent good plus an additional unit of z_k. The *treatment technology* effectively increases each z_k by one unit for every $\mathbf{z}$ in $\mathbf{Z_T}$ that i chooses to consume.

Knowing that the treatment technology will be applied to a bundle of goods alters a consumer's optimal choices. Let (q^*_{xi}, q^*_{zi}) denote i's optimal consumption bundle at equilibrium prices in the absence of any

[6] Notably, these indirect effects will be of second-order importance relative to the direct effects. Thus there must be a benefit–cost calculation on the part of the policy-maker or researcher on whether more precise identification of the direct effect is of value.

[7] If z_{2ht} is observable and data are available for some period preceding the shock to school quality, then one might consider instrumenting for z_{2ht} with the pre-treatment levels of the attribute. In general, however, the geographic variation in z_{2ht} will still be correlated with unobservable location-specific determinants of home value. Suppose, for example, that an additional omitted attribute z_{3ht} is the natural beauty of the area, which is time-invariant, positively correlated with z_{2ht}, and positively valued by consumers. Instrumenting for z_{2ht} with the pre-treatment value will produce an upward-biased estimate of β_2 that captures the effects of both pleasantness of neighbors (which changes in response to a shock to school quality) and natural beauty (which is time-invariant, does not respond to the shock, and should not appear in the set of controls). Supposing that $cov(z_{1ht}, z_{2ht})$, β_1, and β_2 are all positive, then the upward bias in the estimation of β_2 will generate a downward bias in the estimation of β_1 so that the researcher attributes too much of z_{1ht}'s effects on housing prices to the increase in the pleasantness of neighbors.

[8] Another problem particular to labor market studies is that individual wages are determined by workplace amenities and worker productivity, both of which are endogenous and difficult to measure. Note that even with an unconditionally random treatment, including endogenous outcomes as controls can produce conditional endogeneity bias of β_1 and in turn the MWTP.

intervention, where x denotes goods outside the market of interest. Let (q^k_{xi}, q^k_{zi}) denote the consumption bundle that i would choose to purchase at equilibrium prices, given the knowledge that the bundle of goods q^k_{zi} will be converted into $T_{\mathbf{Z}k}(q^k_{zi})$, where the treatment is applied to the entire set **Z**. If, for example, z_k is local school quality, then i might select a home in a relatively low quality school district with the understanding that the district will be improved by the *treatment technology*. The bundle q^k_{zi} includes the home in the low quality district, and $T_{\mathbf{Z}k}(q^k_{zi})$ represents the same bundle after the school quality improvement. Given this, we define the benefit to i of a one-unit increase in attribute z_k:

MARGINAL WILLINGNESS TO PAY: *Consumer i's Marginal Willingness to Pay (MWTP) for* z_k is scalar-valued, is denoted $MWTP_{ki}$, and equals $\theta_i(T_{\mathbf{Z}k}(q^k_{zi}), p^*_x, w^*_i, u^*_i) - p^*_z \cdot q^k_{zi}$.

$MWTP_{ki}$ is a dollar-denominated measure of the *consumer surplus* that i experiences due to the *treatment technology*—from consuming $T_{\mathbf{Z}k}(q^k_{zi})$ at the price $p^*_z \cdot q^k_{zi}$. The reservation price is denoted as $\theta(\bullet)$. The term p^*_x represents the prices for the consumption bundle q^k_{xi} and p^*_z represents the prices for the consumption bundle q^k_{zi}. Because consumer surplus is defined relative to the benchmark utility level u^*_i and wealth level w^*_i, the formula gives the change in surplus that i would experience from switching from the optimal untreated bundle, which provides zero surplus, to the optimal treated bundle.

For $MWTP_{ki}$ to accurately measure the benefit, it is essential that the final term, $p^*_z \cdot q^k_{zi}$, be subtracted off the reservation price $\theta(\bullet)$, so that the expression returns a surplus and not a reservation price. If i lives in a school district with high housing prices and z_k is school district quality, being offered the treatment technology could induce i to move to a more affordable area. After applying the treatment technology, the new area could be as desirable as or even less desirable than the old one. However, the treatment technology had a positive benefit by helping i to save money.

To formalize the concept of an idealized experiment we will use the following definition here:

IDEALIZED EXPERIMENT. To conduct the i*dealized experiment* the researcher draws a sample of N consumers from the population, where the draws are independent. Each consumer has the option to have the treatment technology for attribute z_k applied to every good consumed. To receive this treatment, the consumer must pay a treatment price τ_i, where τ_i is randomly assigned across consumers.

The *idealized experiment* can be applied to measure the MWTP for home improvements, product upgrades, and attributes that are "artificially tied" to specific houses or jobs, as with school district access or health care coverage. However, idealized experiments are limited in their ability to provide MWTP estimates for large attributes intrinsically tied to houses. For example, it is difficult to exogenously vary bedrooms and bathrooms across a large population.

In the absence of the treatment, i selects the bundle q^*_{zi} of all goods in the market of interest **Z** and obtains zero surplus. If the treatment is provided at a price $\tau_i = MWTP_{ki}$, then i is able to purchase the bundle $T_{\mathbf{Z}k}(q^k_{zi})$ at a cost of $\theta_i(T_{\mathbf{Z}k}(q^k_{zi}), p^*_x, w^*_i, u^*_i)$. At the *treatment price* $\tau_i = MWTP_{ki}$, consumer i is indifferent between selecting and not selecting the treatment. At any *treatment price* greater than $MWTP_{ki}$, purchasing the treatment would give i negative surplus, and at any *treatment price* less than $MWTP_{ki}$, purchasing the treatment would give i positive surplus.

Identification of $MWTP_{ki}$ in the *idealized experiment* is a straightforward application of a nonparametric discrete choice estimator (Pagan and Ullah, 1999, pp. 272–299). Consumer i selects the treatment option if and only if $\tau_i \le MWTP_{ki}$. At a given treatment price τ, the fraction of consumers who select the treatment option is $Pr_i(MWTP_{ki} \ge \tau)$, where the probability is taken over all consumers i. This probability can be rewritten in terms of the cumulative density function (F) as $1 - F^{MWTP}_k(\tau)$. Let $Treat_i$ be a binary indicator of whether i selects the treatment. A consistent kernel estimator for $F^{MWTP}_k(\tau)$ can be constructed following Li and Racine (2007, pp. 182–183, 209–210; 2008):

BANDWIDTH AND WEIGHTING KERNEL: A bandwidth h is defined as a decreasing function of the sample size N. For simplicity, let the value $h(N)$ be denoted h. This function satisfies the conditions that $\lim_{N\to\infty} h = 0$ and $\lim_{N\to\infty} N * h = \infty$. A weighting kernel ω is a symmetric, bounded pdf that integrates to one.

ESTIMATOR IN IDEALIZED EXPERIMENT: Given a sample size N, bandwidth h, and weighting kernel ω, for any MWTP value τ, our estimator $\tilde{F}^{MWTP}_{kN}(\tau)$ equals $\frac{\sum_{i=1}^N \omega\left(\frac{\tau_i - \tau}{h}\right) * (1 - Treat_i)}{\sum_{i=1}^N \omega\left(\frac{\tau_i - \tau}{h}\right)}$.

This idealized experiment estimator is similar to that adopted in earlier discrete choice research. Examples of such applications include contingent valuation, in which survey participants report how they might act if presented with certain hypothetical circumstances (Creel and Loomis, 1997; Crooker and Herriges, 2004; Kristrom, 1990).

3.2. Experimental estimator 2: Offer-restricted environments

A related strategy to the existing tradeoffs approach is to induce consumers to participate in an experiment that restricts their choices. In the offer-restricted experiment, consumers are invited to join the study and to restrict their choices in **Z** to come either entirely from $\mathbf{Z}_0$ or entirely from $\mathbf{Z}_{\boldsymbol{k}}$. In exchange for accepting the restriction, participants are given a payment δ. As with idealized experiment, there is a *treatment price* τ_i that is randomly assigned across consumers, and consumers must pay this price in order to consume goods from $\mathbf{Z}_{\boldsymbol{k}}$. Because consumers are paid to participate in the experiment, the offer-restricted MWTP is defined using utility $u_i(q^{\mathbf{Z}_0}_{xi}, q^{\mathbf{Z}_0}_{zi})$ rather than $u_i(q^*_{xi}, q^*_{zi})$ as the benchmark utility level. More formally,

OFFER-RESTRICTED EXPERIMENT: A sample of N consumers is drawn from the population, and each one is offered a large dollar payment δ to participate in the experiment. Each participant must select bundles in **Z** that either include only goods in $\mathbf{Z}_0$, or only goods in $\mathbf{Z}_k$. Hence, for each i, participation in the experiment requires that $q_{zi}(\mathbf{z}) = 0$ for every $\mathbf{z} \notin \mathbf{Z}_0$ or $q_{zi}(\mathbf{z}) = 0$ for every $\mathbf{z} \notin \mathbf{Z}_k$. The payment δ is sufficiently large that every consumer opts to participate. Each participant may choose between consuming goods in $\mathbf{Z}_0$ or goods in $\mathbf{Z}_k$. The researcher randomly assigns a *treatment price* τ_i across participants in the experiment. Let the *price schedule* in the *offer-restricted experiment* be denoted $(p^*_x, p^{\mathbf{Z}_\mathbf{T}}_z)$, where $p^{\mathbf{Z}_\mathbf{T}}_z(\mathbf{z}) = p^*_z(\mathbf{z})$ for all $\mathbf{z} \in \mathbf{Z}_0$ and $p^{\mathbf{Z}_\mathbf{T}}_z(\mathbf{z}) = p^*_z(\text{-}^{\mathbf{Z}_\mathbf{T}}_z(\mathbf{z}) = p^*_z(z_1, \ldots, z_k - 1, \ldots, z_n) + \tau_i$ *for all* $\mathbf{z} \in \mathbf{Z}_k$.

OFFER-RESTRICTED CONSUMPTION BUNDLE: The definition of the offer-restricted consumption bundle applies to an arbitrary set $\mathbf{Z_T}$ which we denote as **Z'**. The actual sets considered in practice are $\mathbf{Z_0}$ and $\mathbf{Z_k}$, the untreated and treated variants of the subset $\mathbf{Z_0}$ from **Z**. Thus, a set of goods **Z'** and a dollar payment δ, consumer i's *offer-restricted consumption bundle* for a set **Z'** and payment δ is i's optimal *consumption bundle* $(q^{\mathbf{Z}'}_{xi}, q^{\mathbf{Z}'}_{zi})$ given the restriction that $q^{\mathbf{Z}'}_{zi}(\mathbf{z}) = 0$ for all $\mathbf{z} \notin \mathbf{Z}'$ and supposing that i is given a dollar payment δ. Analytically, the consumption bundle is the solution to the following price and wealth constrained utility optimization problem:

$\max_{q_{xi}, q_{zi}} u_i(q_{xi}, q_{zi})$ subject to $p^*_x \cdot q_{xi} + p^{\mathbf{Z}'}_z \cdot q_{zi} \le w_i + \delta$ and $q_{zi}(\mathbf{z}) = 0$ for all $\mathbf{z} \notin \mathbf{Z}'$.

OFFER-RESTRICTED MWTP: Let $\mathbf{Z}_0, \mathbf{Z}_k \subseteq \mathbf{Z}$ be two sets of goods such that $\mathbf{Z}_k$ contains the treated variant of every good in $\mathbf{Z}_0$. Let δ be a dollar payment offered to i. *Consumer i's offer-restricted MWTP for z_k from goods in $\mathbf{Z}_0$ with payment δ* is scalar-valued and equals $\theta_i(q_{zi}^{\mathbf{Z}_k}, p_x^*, w_i^* + -_i(q_{zi}^{\mathbf{Z}_k}, p_x^*, w_i^* + \delta, u_i(q_{xi}^{\mathbf{Z}_0}, q_{zi}^{\mathbf{Z}_0})) - p_z^{\mathbf{Z}'} \cdot q_{zi}^{\mathbf{Z}_k}$. *The pdf $f_k^{\mathbf{Z}_0}$ denotes the density of the offer-restricted MWTP, and the corresponding CDF is written as $F_k^{\mathbf{Z}_0\delta}$.*

The estimation strategy in the *offer-restricted experiment* is the same as in the idealized and existing tradeoff experiments. A nonparametric kernel regression of $1 - Treat_i$ on τ_i identifies the CDF of the offer-restricted MWTP. Often the researcher will not be able to offer a sufficiently high payment δ to induce every consumer to participate in the study. In such cases, the study will produce internally valid estimates of $F_k^{\mathbf{Z}_0}$ for a selected sample of consumers who are particularly receptive to cash incentives or the chance to receive the treatment.

One important example of an *offer-restricted experiment* is the RAND Health Insurance Experiment (Manning, et al., 1987). The researchers randomly assigned health insurance plans across participants, so that some consumers faced high prices for doctor and hospital visits and others faced low prices. The authors use the random variation in prices to estimate the willingness to pay for doctor visits and other types of medical care. Another example of an *offer-restricted experiment* is the Internet Demand Experiment (Edell and Varaiya, 1999; Varian, 2001). Consumers participating in that study agreed to have their internet service provided by the researchers. Every time consumers went online, they faced a menu of different amounts of bandwidth, each sold at a different randomly assigned price. A third example of *offer-restricted experiments* involves laboratory or field experiments to measure the discount rate (Harrison, Lau, and Williams, 2002; McClure, et al., 2004). In such studies, consumers are offered a cash amount to be paid now or a slightly larger amount to be distributed later (such as \$100 now or \$100 plus some additional amount in seven months). The additional amount that is offered later is randomly assigned across consumers.

3.3. Experimental estimator 3: Randomized product offers

In many cases, no specific consumer faces a choice between treated and untreated sets of goods, but researchers can learn about the demand for treatment by measuring the extent to which it affects total sales of the product of interest. Such estimators can be used to identify the marginal surplus, MS. One approach for estimating marginal surplus is to generate randomized product offers whose characteristics vary continuously across the consumers being studied:

OFFER DENSITIES. Let the *offer density functions* g_0 and g_k both be pdfs that assign density levels to each good in $\mathbf{Z}$. These density functions are constructed to satisfy $g_0(\mathbf{z}) = g_k(z_1, \ldots, z_k + 1, \ldots, z_n)$ for all $\mathbf{z}$, so that goods drawn from g_k have on average one more unit of z_k than do those drawn from g_0.

RANDOMIZED OFFER EXPERIMENT. A sample of N consumers is drawn from the population, where N is even. For the first $N/2$ consumers, a good $\mathbf{z}_i$ is randomly selected for each consumer from the distribution g_0; for the remaining $N/2$, $\mathbf{z}_i$ is selected according to g_k. Each consumer is offered a subsidy of δ per unit consumed of the offered good, where δ is constant across consumers. Additionally, the researcher randomly assigns a per unit tax τ_i across participants in the experiment, where $\tau_i < \delta$. Each good $\mathbf{z}_i$ is offered at a subsidized price of $p_z^*(\mathbf{z}_i) - \delta + \tau_i$.

In the randomized offer experiment, each consumer in the sample is offered a different good. In addition to randomly selecting the product offers, subsidies for the offered goods are randomly assigned across consumers.

Let h be a bandwidth and ω be a symmetric weighting kernel. Our parameter of interest and our estimator are defined as follows:

MARGINAL SURPLUS FOR THE AVERAGE OFFERED GOOD. The *marginal surplus* (MS) *for the average offered good at quantity level* Q is denoted $E_{\mathbf{Z}}[MS_{\mathbf{z}}^k(Q, s^*)]$ and equals $\int_{\mathbf{Z}} MS_{\mathbf{z}}^k(Q, s^*) g_0(\mathbf{z}) d\mathbf{z}$.

RANDOMIZED OFFER ESTIMATOR. The estimator $\tilde{E}_{\mathbf{Z}}[MS_{\mathbf{zI}}^k(Q, s^*)]$ of MS for the average offered good at quantity level Q equals $\text{argmin}_\tau \left| Q - \frac{\sum_{i=N/2+1}^{N} \omega(\frac{\tau_i - \tau}{h}) * Q_{\mathbf{z}_i i}(\tau_i - \delta, s^*)}{\sum_{i=N/2+1}^{N} \omega(\frac{\tau_i - \tau}{h})} \right| - \text{argmin}_\tau \left| Q - \frac{\sum_{i=1}^{N/2} \omega(\frac{\tau_i - \tau}{h}) * Q_{\mathbf{z}_i i}(\tau_i - \delta, s^*)}{\sum_{i=1}^{N/2} \omega(\frac{\tau_i - \tau}{h})} \right| + \frac{\sum_{i=N/2+1}^{N} p_z(\mathbf{z}_i) - \sum_{i=1}^{N/2} p_z(\mathbf{z}_i)}{N/2}$.

The first argmin term in our random offer estimator estimates the τ value at which consumption of the offered good would equal Q for the average good selected according to the g_k pdf. The term s^* is the set of all prices and quantities that denote the state of the economy in equilibrium. The argmin measures the *inverse aggregate treated demand* at quantity Q. The second argmin term estimates the τ value at which consumption of the offered good would equal Q for the average good selected according to the g_0 pdf. The second argmin measures the *inverse aggregate* (*untreated*) *demand* at quantity Q. To estimate the two argmins, the researcher first estimates two separate nonparametric kernel regressions of $Q_{\mathbf{z}_i i}(\tau_i - \delta, s^*)$ on τ_i, one for each of the two halves of the sample. Next, the researcher inverts the functions by conducting a grid search of τ values, or using Newton's method. The third term measures the average price difference between the offered goods in the two samples. In the definition of MS, the treatment technology is applied holding prices constant. In the experiment just described the prices are different for the average "treated" good drawn from the g_k pdf and the average "untreated" good drawn from the g_0 pdf. The third term in the formula for the estimator corrects for the price difference.

In one recent application of a *randomized offer experiment*, Bertrand, et al. (2010) offered subsidized loans to small business owners in South Africa. The researchers randomized multiple features of the loan, including response deadlines, advertising content, and interest rates. Other researchers have implemented *randomized offer experiments* to study the supply of charitable contributions (Karlan and List, 2007; Landry, et al., 2006). Specifically, subjects were approached and solicited for donations; the researchers randomized the physical characteristics of the solicitors and the extent to which the charity offered matching contributions or lottery incentives.

Table 1 provides a summary of the developed MWTP estimators.

4. Application to a housing attribute

To illustrate the idealized experiment estimation strategy (Estimator 1 in Table 1), we consider the economic value that consumers marginally place on a specific housing attribute, carbon monoxide detectors.[9] A typical hedonic study of the MWTP for carbon monoxide detectors would use a linear regression model as shown in Eq. (1) where carbon monoxide detectors are denoted as the z_{1ht} in the equation, and β_1 is the marginal effect of that attribute on housing prices, holding all other attributes constant. As discussed previously, such an identification strategy often ignores supply-side concerns, or has data limitations that

[9] Most individuals have a pretty good sense as to the value of CO detectors as they are sold individually in local retail markets. Furthermore, the MWTP for a CO detector would entirely be swamped by the error term in a typical hedonic regression model for housing prices. Small sample sizes, t-tests with low power, variation in the presence of CO detectors before the study, etc. are things that make this experiment more of an illustration to be used to answer more important economic questions in the future. Readers should thus be aware of these limitations when working through this example.

Table 1
Description of MWTP estimators developed.

	(1) Estimator	(2) Research design	(3) Key assumptions	(4) Identifies	(5) Applications described in text
1.	Idealized experiment	Offer consumers the option to "treat" all of their goods in the market of interest with an additional unit of the attribute z_k. Randomize the price for the treated option across consumers.	*Local non-satiation*, price-taking consumers.	Distribution across consumers of MWTP for an attribute z_k.	Home improvements, product upgrades, and attributes that are artificially tied to houses or jobs, such as school district access or health care coverage.
2.	Restricted offer experiment	Pay consumers to restrict consumption in the market of interest to "treated" or "untreated" versions of the same good. Randomize price for treated version across consumers.	Same as *idealized experiment*.	Distribution across consumers of "offer-restricted MWTP" for an attribute z_k.	The value of doctor visits and medical treatment, the value of internet bandwidth, the discount rate.
3.	Randomized offer experiment	Offer some consumers "untreated" goods other consumers "treated" goods, both at a subsidized rate, where the subsidy is randomly assigned across consumers.	Same as *idealized experiment*.	"Marginal Surplus" (the vertical difference between the treated and untreated demand curves) at all points along the demand curve.	The value of different characteristics of small business loans or solicitations for charitable donations.

Table 2
Summary statistics of participants offered carbon monoxide (CO) detector.

	Full sample	(1)	(2)	(3)	(4)
		Randomly offered price of CO detector			
		$5	$10	$15	$20
Housing Attributes					
House value	179,077.80	195,685.20	175,428.60	185,300.00	156,522.70
Number of bedrooms	3.22	3.15	3.24	3.25	3.27
Number of baths	2.26	2.20	2.29	2.30	2.27
Square footage	2069.32	2113.11	2037.33	2184.90	1941.05
Age of structure	32.83	33.22	34.38	28.95	34.41
Carbon monoxide risks					
Fireplace	0.80	0.71	0.83	0.86	0.71
Gas furnace	0.60	0.64	0.63	0.68	0.42
Gas stove	0.17	0.18	0.21	0.14	0.17
Presence of CO detector before study	0.50	0.39	0.63	0.46	0.58
Number of CO detectors	1.14	1.22	1.33	0.85	1.14
Purchased CO detector	0.18	0.29	0.13	0.18	0.13
Sample size	98	28	24	22	24

lead to omitted variable or endogeneity bias in the regressions. For instance, a hedonic estimation strategy might exclude some of the other safety features of the house in the regressions such as smoke detectors or home security systems. If some or all of other relevant attributes are not included as controls and correlated with the presence of carbon monoxide detectors, then the omitted variables might lead to biased estimates of β_1.

4.1. Overview of field experiment

To get around such identification issues, we conducted a small-scale field experiment between August and November 2014 that illustrates an example of estimator 1. The primary purpose of the study was to obtain unbiased estimates of consumers' MWTP for carbon monoxide detectors, without the estimation problems just discussed. The US Centers for Disease Control (CDC) reports more than 400 Americans die and 20,000 visit emergency rooms of hospitals from unintentional CO poisoning each year.[10] The CDC recommends all households to have a CO detector installed to prevent CO poisoning, especially those with fireplaces, gas furnaces, and gas stoves. In idealized experiment form, we offered carbon monoxide detectors that retail for $20 at Home Depot at randomly offered prices of $5, $10, $15, and $20 to homeowners in Lubbock, TX. Our example study provides a basis for work in the area and how researchers may want to set up larger-scale experiments in the future, particularly in the urban economics field of study.

Participants for the field experiment were recruited through mailing a survey to the first name listed on owner-occupied residential property tax records provided by the Lubbock Central Appraisal District (LCAD). The city of Lubbock is located in western Texas and had a population of 229,573 as of 2010 (United States Bureau of the Census, 2010), with an estimated 48,301 owner-occupied properties. There were 41,390 addresses of property owners identified as owner-occupied by their request of a homestead exemption on their property taxes, of which 1000 (2.4%) were randomly selected and mailed a letter inviting them to participate in the research study. Households selected were also sent a brief survey asking about their household composition and the current presence of safety features within the household. Individuals were offered $5 for completing and returning the survey in an included postage paid envelope.

Unsolicited mailed surveys are known to have a low response rate (Shih and Fan, 2009). To avoid potential biases due to expected low response rates correlated with the generosity of the randomly offered subsidy, we adopted a two-step design where only the households returning a survey and indicating they wished to participate in a second component of the study were randomized. More specifically, the subset of households returning a survey were called using a phone number they provided and asked additional questions about their housing attributes and any changes in regards to household safety features since they completed the original survey. All participants were told at the beginning of the phone call that they would receive $20 in total compensation for answering a couple of additional question about their housing attributes, or the original $5 they were entitled for returning at the mailed survey. At the conclusion of the call, individuals

[10] See the CDC website (www.cdc.gov/co) for more information.

Table 3
Effect of Ln(randomized price) on CO detector purchase.

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable is indicator for selects CO detector					
	All observations		Previously had a CO detector			
			No		Yes	
Ln (randomized price)	−0.105	−0.056	0.016	0.048	−0.209	−0.195
	(0.078)	(0.075)	(0.122)	(0.107)	(0.096)**	(0.093)**
Controls for CO risk?	No	Yes	No	Yes	No	Yes
R^2	0.021	0.168	0.000	0.225	0.125	0.228
Mean dep var	0.184		0.271		0.100	
	(0.039)		(0.065)		(0.043)	
Observations	98		48		50	

Notes: Standard errors robust to heteroskedasticity are listed below each estimate in parentheses. The controls for carbon monoxide risk are presence of a fireplace, gas furnace, or gas stove.
** $p < 0.05$.

were then offered the chance to use the $20 they received from participating in the study to purchase a carbon monoxide detector for the randomly offered price of $5, $10, $15, or $20. For example, individuals randomly selected to be offered a carbon monoxide detector for $5 had the option to be sent either $20 in the mail, or a carbon monoxide detector plus $15 delivered to their door.

Of the 1000 surveys mailed, we received responses from 18% of the individuals. Table 2 shows basic demographics and summary statistics for those who returned the initial survey. Of the participants who responded to the initial survey, 26% indicated they did not wish to be contacted, and we were unable to reach 19% after at least three attempts using the phone number they provided. Our final sample was therefore composed of 98 participants who completed all phases of the study. Table 2 below presents summary statistics of our sample.

The average participant in our sample reported a house value of $179,077 and lived in a home with 3.22 bedrooms and 2.26 bathrooms. Their home also had on average 2069 square feet of livable space and was approximately 33 years old. Before the study, 50% of participants reported they already had a carbon monoxide installed, and on average 1.14 were installed. Participants were also asked if they had a fireplace, gas furnace, or gas stove, each of which is listed as potential sources of CO poisoning on the CDC website. 80% of participants reported the presence of a fireplace, 60% a gas furnace, and 16% a gas stove within their home.

As mentioned above, we did not make participants aware of their subsidy condition until the end of the phone survey to avoid selection biases due to differential response rates. To test whether we implemented randomization correctly we conducted a t-test for each of the attributes listed in Table 2 to determine whether assignment of any of the subsidy conditions was greater than chance alone. Of the attributes, we only found that persons assigned to the no-subsidy condition (those offered a CO detector for $20) were statistically less likely (t-value = −2.01) to have a gas furnace than those assigned to the other three conditions.

4.2. Experimental estimates

Of participants offered a carbon monoxide detector in lieu of their full $20 in compensation, 18% accepted the offer. To obtain unbiased elasticity estimates on the attribute in question (CO detectors), we use the following linear probability regression equation:

$$Purchase\ CO_i = \beta_0 + \beta_1 Log(Randomized\ Price)_i + \mathbf{Z}_i'\boldsymbol{\delta} + \varepsilon_i \qquad (2)$$

where *Purchase CO* is an indicator variable equal to one if individual i purchased a CO detector (and zero otherwise), **Z** is a vector of control variables indicating four carbon monoxide risks within the household: fire place, gas furnace, and gas stove, and ε is a white noise error term. *Log*(*Randomized Price*) is the variable of interest and signifies the log of the offered CO detector price to consumer i. Thus, β_1 reveals the estimated elasticity from the idealized experiment.

Table 3 below presents the results for Eq. (2). The first two columns are of the full unrestricted sample of 98 participants. Column (1) portrays the regression results without controls and column (2) includes the full set of controls as described in Eq. (2). For the full sample, the estimates indicate that a 10% increase in price leads to about a one percentage point decrease in the probability of purchasing a carbon monoxide detector when controls are not included and a 0.5 percentage point decrease when all of the controls are included. Both of the specifications, however, are imprecisely estimated with neither of the coefficients being significantly different from zero at any conventional level.

The next four columns of Table 3 stratify the sample based upon whether or not the participant indicated they already had a carbon monoxide detector installed at the initial survey. Columns (3) and (4) present the estimates for observations that did not previously have a CO detector. Both estimates are positive and relatively small (0.016 and 0.048, respectively). In addition, neither is significantly different from zero at any of the conventional levels. Columns (5) and (6) show the elasticity estimates for individuals who already had a CO detector. The estimates are the most precisely estimated of the group, with both elasticities being significant at the five percent level. The CO adoption likelihood elasticity estimates are −0.209 and −0.195.

Fig. 1 illustrates graphically the empirical results presented in Table 3. The dollar price for a carbon monoxide detector is plotted along the vertical axis, and the fraction of the sample choosing to purchase the detector is plotted along the horizontal axis. The scatterplots are created by plotting the residual after differencing the observable control variables listed in Table 2. In other words, it is the unexplained portion of the outcome variable plotted against the unexplained portion of the outcome variable of interest. Plots are shown separately for respondents based on their prior ownership of a CO detector. Among respondents who did not previously have CO detectors (Panel A), we observe a roughly flat relationship, indicating that the price has little effect on the purchase decision for these households. Among respondents who did previously have CO detectors (Panel B), we observe the expected negative sloping demand curve. Panels A and B, therefore, provide a graphical description of the MWTP across participants in this experiment—in noted contrast to the single point estimates normally seen in traditional hedonic studies.

What our estimates indicate is that the subsidies acted as an effective price incentive to purchase additional detectors for those who already had them. Although the sample size somewhat limits the preciseness

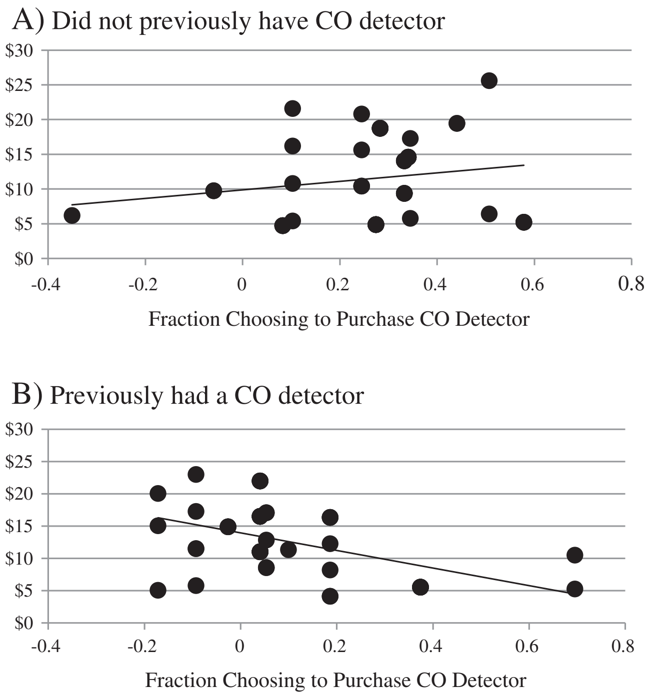

Fig. 1. Experimental estimates of demand curve of CO detectors. Notes: Each figure represents a scatterplot of the residual for each participant after subtracting the effect of the control variables listed in Table 2. More precisely, it is the unexplained portion of the outcome variable plotted against the unexplained portion of the variable of interest. The line in each figure represents the slope of a linear regression through those scatterplots.

of some of the estimates depending upon the specification type, we can still get a handle on how consumers react to changes in the price of such housing attributes.[11] The research design shown here mitigates the estimation issues previously discussed and provides a template for future work in this area. For example, researchers may utilize the methods described here to find MWTP estimates for other household attributes or safety features. While we primarily focus on concerns within the urban economics field, these methods may be able to provide estimates in other fields such as labor economics. One might imagine obtaining MWTP estimates for things such as fire extinguishers, smoke detectors, etc. with similar experiments. Combined with detailed data on deaths, researchers could back out more precise estimates on the value of a statistical life in comparison to traditional wage regressions which may be riddled with omitted variable bias problems. This is just one example of how these methods may be useful in other fields as well.

Furthermore, while the primary concern of our paper is Rosen's first stage, this application shows that researchers might be able to approximate the full MWTP function (or more generally referred to as Rosen's second stage) under certain circumstances. Unfortunately sketching out the full MWTP distribution by researchers would typically require a large amount of resources.[12] While not impossible, it would be cost prohibitive to implement in most cases.

[11] In results not shown, we use a standard hedonic model which regresses the dependent variable *Home Value* on the number CO detectors, holding other household attributes constant (for the same household sets utilizes in Table 3 and Fig. 1). The standard hedonic model estimated the MWTP for CO detectors to be roughly $14,000—a clear indication of omitted variable problems. These results are available upon request. The discrepancy between MWTP estimates from the standard hedonic model and those shown here underscores the importance of seeking and applying more robust MWTP estimators.

[12] For example, a researcher could offer varying subsidies to all consumers in a specific market to receive information on each of the consumer's MWTP for particular products. This would allow the researcher the ability to sketch out the full MWTP function since she would have an estimate for each individual's MWTP in the market. That stated, this type of estimation strategy could be very expensive to implement unless the product was very cheap and even in that case, there might be time restrictions in being able to carry out such an exercise across a large group of people.

5. Conclusion

We have extended the theory of hedonic estimation to incorporate three important aspects of markets for heterogeneous goods. First, many important product attributes are endogenous and change in response to exogenous shocks. Second, many heterogeneous goods have complements and substitutes, and exogenous shocks to the market of interest may affect the markets for those other products. Third, aggregate quantity supplied may change in response to an exogenous shock. For all three reasons, the benefits of an exogenous shock to one product attribute will not entirely be capitalized into the price of that product, and traditional hedonic estimators will produce biased estimates.

We began with an idealized experiment to highlight what assumptions are in principle necessary to identify MWTP. The new modeling concept serves as a theoretical benchmark to clarify the tradeoffs that researchers face between generality of the model and availability of experimental or quasi-experimental data. In the *idealized experiment* the researcher has the ability to effectively "treat" goods that a consumer purchases with an additional unit of the attribute of interest z_k. A small set of consumers is selected from the population, and each one is offered the treatment option at a different, randomly assigned price. Because the intervention only affects the small number of consumers participating in the study, it avoids biases from market-level changes to other attributes, other goods' prices, or aggregate quantity. Through such an idealized experiment it is possible to identify the distribution across consumers of the MWTP for the attribute z_k. In keeping with the policy focus of hedonic estimation, we paid exclusive attention to identification of the demand for product attributes, and did not impose extra assumptions to identify utility or cost functions.

We then developed alternative practical estimators that identify the distributions across consumers of policy-relevant measures of product attribute demand that differ slightly from MWTP. In the "offer-restricted experiment" the researcher artificially restricts consumers' options to the untreated and treated sets of goods and provides financial compensation to the participants to induce them to accept the restriction. The researcher then can identify the value of the treatment by randomly assigning subsidies for selecting the treatment option across consumers in the study.

The final estimator presented compares total sales of untreated goods with their treated variants. For the treated-untreated research designs, some consumers face decisions between untreated goods and their best outside options, and others face decisions between treated goods and their outside options. It is not possible to identify MWTP for any consumer, but it is possible to identify the effect of the attribute z_k on aggregate surplus. In a "randomized offer experiment" the researcher offers each participant in the experiment a subsidized good. The level of z_k in the offered good and the amount of the subsidy are randomized across consumers. In the *randomized offer experiment* the researcher compares the effect of z_k on the demand for the offered good to the effect of the dollar subsidy to measure the subsidy amount that would increase demand by as much as the attribute does.

We concluded by presenting a small-scale field experiment to illustrate an application of the *idealized experiment* estimator. Our experiment randomized the price of carbon monoxide detectors offered to households to show how to obtain robust local estimates of the MWTP. Although use of the *idealized experiment* and other estimators outlined in the paper may not be ideal in all research settings due to data limitations, we strongly encourage their adoption to provide more robust and transparent MWTP estimates of heterogeneous goods in future research.

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