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Identifying demand parameters in the presence of unobservables: A combined revealed and stated preference approach

Roger H. von Haefen, Daniel J. Phaneuf *

North Carolina State University, USA

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ABSTRACT

We develop a combined, revealed and stated preference approach to identify discrete choice demand parameters in the presence of unobserved determinants of choice. Our approach overcomes difficulties associated with small choice sets, multicollinearity, and endogeneity that arise with revealed preference approaches. To illustrate our framework, we revisit two Canadian moose hunting datasets. Our empirical results suggest the potential gains from fusing revealed and stated preference data, but they also suggest its limitations when the data-generating processes for the data sources differ.

1. Introduction

Estimating the demand for quality-differentiated goods, such as recreation sites or residential location, is a common problem in environmental economics. Typically, the analyst is interested in understanding the role of environmental quality in individual choice, and assessing how well being changes when exogenous attributes change due to policy or other interventions. This type of inference problem requires that the analyst estimate a structural model of behavior, and discrete choice random utility maximization (RUM) models have become the standard approach in much of this literature.

Within this context, Timmins and Murdock [26] and Bayer et al. [5] recently implemented a discrete choice econometric strategy that exploits revealed preference (RP) data to identify the behavioral and welfare effects of environmental quality while controlling for unobserved determinants of choice. Their RP approach is particularly appealing when significant preference heterogeneity exists and observed commodity attributes are endogenous, i.e., correlated with unobserved attributes. Three limitations with their strategy are that it requires: (1) datasets with many commodities (sites in the recreation context, houses or neighborhoods in the hedonic context); (2) variation in observed attributes across the objects of choice; and (3) instruments for endogenous variables. In this paper, we develop an alternative econometric strategy that exploits both RP and stated preference (SP) data to identify environmental quality's effect on behavior and welfare while controlling for unobserved attributes. The advantages of our proposed strategy are that it can be used in applications with small choice sets (14 and 11 in our empirical applications), exploits the experimental design embedded in the SP data to overcome multicollinearity problems often present in RP data, and does not require instruments for endogenous variables. Like Murdock [19], our strategy also controls for observable and unobservable preference heterogeneity. To our knowledge, our empirical models represent the most detailed accounting of both preference and attribute heterogeneity in combined RP/SP environmental applications.

* Corresponding author.
E-mail addresses: roger_von_haefen@ncsu.edu (R.H. von Haefen), dan_phaneuf@ncsu.edu (D.J. Phaneuf).

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The notion of 'fusing' revealed and stated preference data has gained broad acceptance in environmental economics, transportation, and marketing over the past decade [30]. Combining RP decisions made at current conditions with SP choices made under hypothetical conditions within a unified behavioral model can aid in identifying the role of observable attributes in preferences. Most applications of this type in environmental economics have focused on the problem of insufficient variation in natural conditions. Analysts in these instances have viewed combined models primarily as a tool for identifying the effects of observable environmental attributes while preserving some of the advantages of RP analysis [12,16,31]. More generally, combined RP/SP models have been advocated as a means of harnessing the complementary strengths and weakness of the two approaches [30]. In the discrete choice context, the SP data is often generated with choice experiments [15]. Choice experiments present individuals a series of hypothetical alternatives with randomly assigned attributes and ask them to state their preferred alternatives. Past experience with combining RP and choice experiment SP data suggests that combined RP/SP models can identify structural parameters associated with observed attributes that are not identified with just RP data [1,2]. For credible identification, however, it is critical that the analyst control for unobserved attributes associated with the RP objects of choice as well as observed and unobserved preference heterogeneity. To varying degrees, these issues have been dealt with in previous RP/SP studies. Our econometric strategy, however, addresses them in a more comprehensive and systematic manner and, more fundamentally, confronts the difficulties associated with endogenous attributes.[1]

To illustrate our proposed RP/SP econometric model, we reconsider two empirical applications related to recreational moose hunting in Canada [2,14]. The RP data consists of seasonal trip data to 14 wildlife management units (WMUs) in Alberta and 11 wildlife management zones (WMZs) in Saskatchewan. The SP data was constructed by presenting the same sample of individuals in the RP data a series of choice scenarios related to hypothetical moose hunting sites with exogenously varying attributes. Our empirical results suggest the potential gains from fusing these different data sources in terms of parameter identification. They also suggest how the inclusion of controls for unobserved site attributes as well as observed and unobserved preference heterogeneity can generate improvements in statistical fit. Our combined RP/SP identification strategy comes with a cost, however; tests for parameter consistency across the RP and SP data are routinely and strongly rejected. This finding runs counter to recent experimental evidence [18,25]. It also contradicts previous published findings that employ the same moose hunting data we use with more parsimonious specifications. Our findings raise important questions for how welfare evaluations should be conducted in these instances, and we consider a menu of plausible approaches to construct welfare measures that future researchers may find useful.

2. Identification with RP and SP data

In this section, we develop a general discrete choice econometric model and discuss identification issues arising with RP data, SP data, and combined RP/SP data. We adopt notation that closely follows Berry et al. [8] who first raised the identification issues we are concerned with in the context of RP applications. For concreteness, we couch our model in the context of travel cost models of recreation site choice, although the model applies in principle to many different choice settings such as housing or automobiles [20].

In the recreation site choice context, each individual is assumed to choose one of J sites as a recreation destination on a given trip. Recreation sites are differentiated in terms of their attributes and costs of visitation (travel costs) for each individual. To model such choices within an RUM framework, we begin by specifying the individual's conditional indirect utility function:

$$V_{ijt} = \boldsymbol{w}_{ijt}^{\mathrm{RP}}\tilde{\boldsymbol{\gamma}}_i + \boldsymbol{x}_j^{\mathrm{RP}}\tilde{\boldsymbol{\beta}}_i + \xi_j + \mu\varepsilon_{ijt}, \tag{1}$$

where i indexes individuals, j indexes the J sites, and t indexes trip occasions for individual i. The above specification assumes that utility is additive in four components: (1) $\boldsymbol{w}_{ijt}^{\mathrm{RP}}\tilde{\boldsymbol{\gamma}}_i$, an index based on observed site attributes that vary across individuals, trips, or both individuals and trips in the RP data, $\boldsymbol{w}_{ijt}^{\mathrm{RP}}$ (e.g., travel costs, perceived or time-varying environmental quality measures); (2) $\boldsymbol{x}_j^{\mathrm{RP}}\tilde{\boldsymbol{\beta}}_i$, an index based on observed attributes that vary only across sites in the RP data, $\boldsymbol{x}_j^{\mathrm{RP}}$ (e.g., time-invariant objective measures of environmental quality, availability of bathrooms or boat ramps); (3) ξ_j, an alternative specific constant (ASC) that controls for unobserved attributes that vary across sites and may be correlated with observed attributes; and (4) $\mu\varepsilon_{ijt}$, the product of an unobserved, idiosyncratic normalized error that varies across individuals, sites, and trips and a scale parameter μ. Although we allow for possible correlations among the first three components, we make the identifying assumption that ε_{ijt} is orthogonal to all other components in V_{ijt}. Furthermore, we specify the parameters in the $\boldsymbol{w}_{ijt}^{\mathrm{RP}}\tilde{\boldsymbol{\gamma}}_i$ and $\boldsymbol{x}_j^{\mathrm{RP}}\tilde{\boldsymbol{\beta}}_i$ indexes, $\tilde{\boldsymbol{\gamma}}_i$ and $\tilde{\boldsymbol{\beta}}_i$, to vary systematically and randomly

[1] For example, Haener et al. [14] employ alternative specific constants (ASCs) to control for unobserved commodity attributes for a selected set of commodities, account for observed preference heterogeneity by selectively interacting observed attributes and demographics, and do not allow for unobserved preference heterogeneity through random parameters. Boxall et al. [10] introduce random parameters in a combined RP/SP model with some observed preference heterogeneity but no ASCs. By contrast in our application, we allow all observed attributes to have random parameters, interact all observed attributes with a set of observed demographics, and include a full set of ASCs.

across individuals:

$$\tilde{\gamma}_i = \bar{\gamma} + z_i\gamma^0 + \boldsymbol{v}_i\gamma^v,$$
$$\tilde{\boldsymbol{\beta}}_i = \bar{\boldsymbol{\beta}} + z_i\boldsymbol{\beta}^0 + \boldsymbol{u}_i\boldsymbol{\beta}^u,$$

where $(\bar{\gamma}, \bar{\boldsymbol{\beta}})$ are parameter vectors of main or average effects, $\boldsymbol{z}_i$ is a vector of demographics, $(\gamma^0, \boldsymbol{\beta}^0)$ are parameter matrices of interaction effects, $(\boldsymbol{v}_i, \boldsymbol{u}_i)$ are normalized random effects that are independent of $\boldsymbol{z}_i$, and $(\gamma^v, \boldsymbol{\beta}^u)$ are vectors of standard errors for the random effects. The structure of preferences in Eq. (1) is very general in that it allows for systematic and random variation in preferences as well as unobserved site characteristics that influence choice. For later discussion, it is useful to collapse all elements in Eq. (1) that are common to site j and equal across individuals and time into a scalar δ_j:

$$\delta_j = \boldsymbol{x}_j^{\mathrm{RP}}\bar{\boldsymbol{\beta}} + \xi_j, \quad j = 1, \ldots, J. \tag{2}$$

We now discuss how the analyst can identify the structural parameters in Eq. (1) with RP, SP, and combined RP and SP data. Consider first the case where only RP data is available. Building on Ref. [6], Timmins and Murdock [26], Bayer et al. [5], and Murdock [19] propose a two-step approach. In the first step, the analyst estimates ($\bar{\gamma}$, γ^0, β^0, γ^v, β^u) and the δ_j's through maximum likelihood. This requires normalizing the scale parameter μ to one and one of the ASCs (say δ_1) to zero with no loss in generality. If several random effects are included in the empirical specification, simulation-based estimation will be required. Also, in applications with many sites, estimating the full set of ASCs may require the use of a contraction-mapping algorithm developed in Ref. [6].

If the analyst is concerned with policies involving site loss scenarios or changes in $\boldsymbol{w}_{ijt}^{\mathrm{RP}}$, the first-stage estimates will have recovered enough parameters for welfare analysis.[2] However, if the analyst's concern centers on exogenous policy shocks involving changes to observed attributes that only vary across sites, a second-stage regression based on the specification in Eq. (2) above will be necessary. In these instances, the estimated ASCs are regressed on the observed attributes $\boldsymbol{x}_j^{\mathrm{RP}}$. Implementation of this regression may be confounded by a number of factors that are of practical importance in environmental applications. First, for the regression to satisfy the rank condition, no observed attributes can be linear combinations of the other attributes, and the number of ASCs (i.e., the number of sites minus one) must be greater than the dimension of $\boldsymbol{x}_j^{\mathrm{RP}}$. Second, past experience suggests that the number of ASCs must be substantially greater than the dimension of $\boldsymbol{x}_j^{\mathrm{RP}}$ for parameters to be precisely estimated.[3] Third, if there are correlations between the unobserved site attributes ξ_j and $\boldsymbol{x}_j^{\mathrm{RP}}$, instruments will be required. The structure of the discrete choice model can be used to develop instruments for observed attributes that are determined through social interactions such as congestion [4,5,26]. Otherwise, the analyst will need to find other sources for variables that are correlated with the observables but uncorrelated with the unobservables. The use of instruments in the second stage, particularly if they have little identifying power, also exacerbates the need for a large choice set to precisely estimate the structural parameters. For all of these reasons, identification may be elusive in environmental applications that exploit only RP data.

SP methods can in principle be used to identify parameters in Eq. (1). For example, choice experiments can be designed that present respondents with a series of hypothetical recreation sites that vary exogenously in the level of measurable attributes. For each hypothetical choice set, respondents are asked to state which if any of the sites they would choose to visit. Recent experimental evidence suggests that the process by which experienced individuals make these hypothetical choices is very similar to how they make similar choices in consequential (i.e., real) situations except for a tendency to choose the 'opt-out' or 'no trip' alternative more frequently [18,25]. These findings suggest that SP data can be used to identify at least some of the relevant structural parameters in Eq. (1).

Identification with SP data is achieved as follows. Similar to the RP choice context, the analyst specifies a conditional indirect utility function for each hypothetical site j and choice question c of the form:

$$\begin{aligned} V_{ijc} &= \boldsymbol{w}_{ijc}^{\mathrm{SP}}\tilde{\gamma}_i + \boldsymbol{x}_{ijc}^{\mathrm{SP}}\tilde{\boldsymbol{\beta}}_i + \mu^*\varepsilon_{ijc}, \\ \tilde{\gamma}_i &= \bar{\gamma} + z_i\gamma^0 + \boldsymbol{v}_i\gamma^v, \\ \tilde{\boldsymbol{\beta}}_i &= \bar{\boldsymbol{\beta}} + z_i\boldsymbol{\beta}^0 + \boldsymbol{u}_i\boldsymbol{\beta}^u. \end{aligned} \tag{3}$$

There are three significant differences between the SP and RP preference specifications in Eqs. (3) and (1). First, due to the random assignment of all observed attributes, the site-specific observed attributes that fell in the $\boldsymbol{x}_j^{\mathrm{RP}}$ vector now vary across individuals, alternatives, and SP choice scenarios. This implies that *all* main and interaction effects in $\tilde{\gamma}_i$ and $\tilde{\boldsymbol{\beta}}_i$ can be identified from the exogenous variation in the experimental design. Second, the SP scale parameter, μ^*, for the idiosyncratic error may differ from the RP scale parameter, μ [23]. This possibility has little practical import when using just SP data to identify the structural parameters in Eq. (1) because arbitrary rescaling of all parameters has no effect on welfare analysis. It does, however, have important implications for pooling RP and SP data as we discuss below. Third, whereas the RP objects of choice consist of observed and unobserved attributes that may be correlated, the SP objects of choice consist only of

[2] A notable exception to this statement arises when there are observed site attributes that do not vary across individuals and trips that are endogenously determined by social interactions such as congestion. In this case, second-stage estimation will be required to construct general equilibrium welfare measures that reflect endogenous resorting arising from exogenous policy shocks (see Ref. [26]).

[3] For example, there are 569 sites and less than 20 site attributes included in the second-stage regression in Refs. [19,26].

observed attributes. The absence of a role for unobserved site attributes reflects that fact that environmental choice experiment applications do not 'brand' each choice alternative as a real (but experimentally altered) recreation site with which respondents have experience.[4] This implies that the ξ_j's in Eq. (1) cannot be identified with the SP data, and thus the full structure of preferences cannot be recovered from SP data alone. As a result, welfare analysis is confounded unless the analyst restrictively assumes that unobserved attributes have no impact on choice or are equal across all alternatives. Jointly estimating preferences with RP and SP data, however, permits identification of all structural parameters without relying on a second-stage regression or restrictions on the unobserved attributes' parameter values. To see this, recall that the identification difficulties with RP approaches arise because both the unobserved attributes and the main effects associated with observed attributes that only vary across sites are captured in the first-stage estimated ASCs. In other words, the linear combination $\boldsymbol{x}_j^{\mathrm{RP}}\bar{\boldsymbol{\beta}} + \xi_j$ is identified in the first stage of the RP estimation strategy, not the individual $\bar{\boldsymbol{\beta}}$ and ξ_j parameters. Recall that with SP data, all parameters in $(\tilde{\gamma}_i, \tilde{\boldsymbol{\beta}}_i)$—including $\bar{\boldsymbol{\beta}}$—are identified off of the exogenous variation in all observed attributes embedded in SP experimental design. However, if the RP data identifies $\boldsymbol{x}_j^{\mathrm{RP}}\bar{\boldsymbol{\beta}} + \xi_j$ and the SP data identifies $\bar{\boldsymbol{\beta}}$, the combination of RP and SP datasets jointly identify $\bar{\boldsymbol{\beta}}$ and ξ_j. This implies that welfare analysis is feasible for scenarios involving changes in all site attributes in applications with small choice sets, little or no variation in observed attributes, and unavailable instruments for the endogenous observed attributes.

It is worth emphasizing that, in addition to the experimental design embedded in the SP choice experiments, identification with combined RP/SP data depends critically on the assumption that a common data-generating process gives rise to both RP and SP choices. The cross-equation restrictions implied by the common data-generating process can be relaxed in some ways—differences in RP and SP scale for the idiosyncratic error term can be introduced, and the frequency of the 'opt-out' alternative being chosen in the SP choice experiments can be controlled. The parameters identified by both data sources (i.e., the main effects for site attributes that vary across individuals or time as well as all interaction and random effects) must be set equal for the approach to have identifying power. As stated above, some experimental evidence suggests that these parameters may converge across RP and SP data sources, especially with experienced respondents. The evidence from past RP/SP studies in environmental economics, however, is mixed [1,12].[5] From a statistical perspective, parameter convergence across RP and SP sources is a testable hypothesis, and we therefore conduct likelihood ratio tests to evaluate these cross-equation restrictions in our empirical applications.

3. Econometric specification

To transform the behavioral model outlined in the previous section into an estimable econometric model, we make two distributional assumptions. First, we assume the idiosyncratic errors in Eqs. (1) and (3) are independent and identically distributed type I extreme value draws. This assumption, omnipresent in the discrete choice literature, implies that the RP and SP conditional choice probabilities have the convenient logit form. We allow for differences in scale between the RP and SP data by incorporating the parameter ratio μ/μ^* in the SP conditional probabilities [23].[6] Second, we assume the individual-specific random effects in the parameter vectors, $(\boldsymbol{v}_i, \boldsymbol{u}_i)$ are independent and identically distributed standard normal. To move from the conditional choice probabilities implied by the type I extreme value assumption to the unconditional probabilities used in estimation, we employ simulation [27]. We then use the method of maximum likelihood to estimate the structural parameters. The structure of the simulated likelihood function is reported in Appendix, and our estimation code is available on request.

Our empirical applications involve choice sets that are relatively small (14 and 11 sites, respectively), and thus estimating a full set of ASCs along with the main, interaction, and scale ratio parameters is relatively straightforward using standard gradient-based search techniques. For large choice set applications, employing a contraction mapping to numerically solve for the implied ASCs may be necessary [6].

[4] In SP modal choice experiments in transportation, researchers have included ASCs to control for unobserved attributes associated with each modal type (car, bus, train) [9]. Such an approach implicitly assumes that there are a common set of unobservables that individuals take into account when considering a hypothetical alternative. In environmental applications where the objects of choice are far more disaggregated and numerous (e.g., recreation lakes or houses), such an assumption seems more tenuous. Although it is possible that respondents attach a set of unobservables to a hypothetical choice alternative with a given set of observable characteristics (i.e., unobservables associated with an actual recreation site with similar observable attributes), including ASCs is only appropriate to the degree that *different* individuals attach the *same* unobservables. Whether individuals in fact do this is certainly an interesting empirical question worthy of future research, but our view is that the assumption that the attached unobservables associated with a choice alternative are orthogonal across individuals—i.e., different individuals attach different unobservables to different hypothetical alternatives—is a reasonable starting point in environmental applications.

[5] It is interesting to note that other areas of structural microeconometrics generally do not test the validity of the cross-equation restrictions embedded in combined RP/SP models. For example, Berry et al. [8] could have tested whether the data generating process implied by their 'second choice' SP new automobile purchase data was consistent with their RP purchase data but do not. Likewise, Berry et al. [7] could have tested the validity of their instruments given their model was over-identified but do not. Our point is that environmental economists seem to be holding their models to a higher standard than researchers in other fields, and negative results with respect to pooling RP and SP data should not necessarily be interpreted as suggesting that combined RP/SP approaches are less appealing than the identifying strategies used elsewhere.

[6] Although the models we report assume that the RP/SP scale ratio is constant, we also estimated models where the scale ratio varied across SP choice occasions to allow for learning and fatigue effects. Although statistical tests suggested that this generalization improved model fits significantly, welfare estimates did not change qualitatively.

4. Data

To illustrate the logic of our approach and demonstrate its potential value, we reconsider two previously analyzed datasets [2,14]. The first examines preferences among moose hunters in the Canadian province of Alberta for visits to 14 WMUs and their associated site attributes. This dataset provides information on the actual visits made by 271 moose hunters to the WMUs as well as answers to a series of choice experiment questions (up to 16 in total) that solicited hypothetical choices among two generic hunting sites and an 'opt-out' option. The experimental design included attributes based on distance from home, road quality on which the person traveled to reach the site, access conditions, encounters with other hunters, forestry activity at the site, and the local moose population. Expected values for these variables at the 14 WMUs are also available corresponding to the time that the observed visitation behavior was recorded. The top half of Table 1 provides a summary of the variables and their RP means that are available in both the RP and SP data as well as information that is available on the individual survey respondents.

The second dataset examines preferences among Saskatchewan moose hunters for visits to 11 WMZs. Five hundred and thirty-two hunters provided information on the trips they made to the 11 sites as well as answers to a set of choice experiment questions (up to 14 in total) that were similar to the Alberta SP application. Attributes included distance, access conditions to the site, encounters with other hunters, forestry activity, the local moose population, and variables describing other species present at the site. The bottom half of Table 1 provides a description and summary of the variables as defined and used in the analysis.[7]

5. Results

The results of our analysis using the Alberta data are presented in Tables 2 and 3 and for the Saskatchewan data in Tables 4 and 5. Table 2 reports parameter estimates with the Alberta dataset for several systematically and randomly varying parameter specifications. Each row of the table corresponds to a separate variable that we wish to identify. These include a travel cost term, attributes of the 14 recreation sites, interactions between three individual characteristics and each site attribute, a scale ratio parameter, ASCs for each recreation site less one for normalization, and a random effect for each site attribute. The random coefficients reflect heterogeneity in attribute preferences not accounted for by the interaction terms, and can be interpreted as standard deviations for the normally distributed main effects parameters. We present parameter estimates and *t*-statistics for five models arrayed across the 10 columns. From left to right, these include an RP model without ASCs, an RP model with ASCs, an SP model, a combined RP/SP model without ASCs, and our most general model that uses both RP and SP data with ASCs.

In general, we find parameter signs and significance that are intuitive and match previous analyses using this data. For all five models, both the inclusion of interaction terms and random effects in the parameter specifications jointly add considerable explanatory power (see the reported *p*-values at the bottom of Table 2), although many of the interaction terms are not individually statistically significant.[8] Focusing first on the RP data model without ASCs in columns one and two, we find that a number of parameters (*No trail*, *4WD trail*, *On foot*, *On ATV*) are not identified due to multicollinearity in the RP data. The addition of ASCs in columns three and four results in a more severe identification problem as the main effects associated with all observed site attributes except *Unpaved* (which varies across sites and individuals because different individuals use different roads to access the sites) are confounded with the unobserved site attributes. Moreover, as we report in Appendix, a second-stage OLS regression of the ASCs on the observed attributes did not produce significant or plausibly signed estimates.[9] As a result, welfare analyses involving changes in any attribute besides *Travel Cost* and *Unpaved* are not possible. This finding might lead one to prefer the more parsimonious specification without ASCs, but a likelihood ratio test strongly suggests that the gains in statistical fit arising from the addition of the ASCs are significant (p-value <0.0001).

Columns five and six report parameter estimates for the SP data model. For this specification, an *Outside Dummy* variable is interacted with demographics and a random effect to account for the inclusion of the 'opt-out' option. Confirming our claim in Section 2, the experimental design embedded in the choice experiments permits identification of all main, interaction, and random effect parameters for the observed attributes. However, the SP data does not identify the role of unobserved attributes that are specific to the WMUs. Unless the analyst restrictively assumes that unobserved attributes have no influence on moose hunters' decisions, welfare analysis is not feasible with the SP specification.

Finally, columns seven through 10 report estimates for the combined RP/SP models. Fusing the data sources facilitates identification of all main, interaction, and random effects for the observed attributes. Comparing the combined RP/SP models with and without ASCs, we find a substantial and statistically significant improvement in fit generated by the

[7] In both SP datasets, no individual always chooses the 'opt-out' alternative. Thus, the single and double-hurdle models developed in von Haefen et al. [29] cannot be used to address serial nonparticipation.

[8] Parameter estimates for specifications where all interaction terms were restricted to zero and where all random effects were restricted to zero are available from the authors upon request. For the tests of the joint significance of the random effects, we constructed *p*-values using the approach developed by Self and Liang [21] and Andrews [3].

[9] Because credible instruments were not available for this regression, we restrictively assumed that the unobserved site attributes were orthogonal to the observed site attributes.

Table 1
Variable definitions

Variable	Definition	RP mean
1993 Alberta data (14 sites, 271 observations)		
Site characteristics		
Travel cost[a]	Round trip travel cost	$219.71
Road quality (excluded category—mostly paved, some gravel or dirt roads)		
Unpaved	Some paved, mostly gravel or dirt roads (effects coded)	−0.82
Hunter access (excluded category—newer trails, passable with two-wheel-drive vehicle)		
No trail	No trails, cutlines, or seismic lines (effects coded)	−0.21
Old trail	Old trails, passable with ATV (effects coded)	0.21
4WD trail	Newer trails, passable with four-wheel-drive vehicle	0.14
Hunter congestion (excluded category—encounters with other hunters on trucks)		
No hunters	Encounters with no other hunters (effects coded)	−0.64
On foot	Encounters with other hunters on foot (effects coded)	−0.64
On ATV	Encounters with other hunters on ATVs (effects coded)	−0.29
Forestry activity (excluded category—some evidence of recent forestry activity)		
No logging	No evidence of recent forestry activity (effects coded)	0.57
Moose variables (excluded category—evidence of 4 or more moose per day)		
<1 moose	Evidence of <1 moose per day (effects coded)	0.14
1/2 moose	Evidence of 1–2 moose per day (effects coded)	0.50
3/4 moose	Evidence of 3–4 moose per day (effects coded)	0.07
Individual characteristics		Sample mean
Total trips	Average number of trips to all sites	3.62
Gen hunt exp.	Years of hunting experience (count)	20.2
Edmonton	Dummy for Edmonton resident	0.45
HS diploma	Dummy for high school diploma	0.91
1994 Saskatchewan data (11 sites, 532 observations)		
Site characteristics		RP mean
Travel cost[a]	Round trip travel cost	$251.49
Hunter access (excluded category—access on foot or ATV)		
2WD access	Passable with two-wheel-drive vehicle (effects coded)	0.55
4WD access	Passable with four-wheel-drive vehicle (effects coded)	0.45
Hunter congestion (excluded category—encounters with other hunters on ATVs)		
No hunters	Encounters with no other hunters (effects coded)	−0.45
On foot	Encounters with other hunters on foot (effects coded)	0.09
Forestry activity (excluded category—some evidence of recent forestry activity)		
No logging	Little or no evidence of recent forestry activity (effects coded)	0.27
Moose variables (excluded category—evidence of 3 moose every 2 days)		
<1 moose	Evidence of <1 moose every 2 days (effects coded)	0.55
1 moose	Evidence of 1 moose per day (effects coded)	0.18
Wildlife species (excluded category—sightings of common wildlife, one to two previously unseen species, and possibly rare or endangered species)		
Common species	Only sightings of common wildlife (effects coded)	−0.09
Unseen species	Sightings of common wildlife and one to two previously unseen species (effects coded)	−0.55
Individual characteristics		Sample mean
Total trips	Average number of trips to all sites	1.37
Gen hunt exp	Years of hunting experience (count)	23.23
Urban	Dummy for residents of Whitecourt, Hinton, Edson, or Drayton Valley	0.81
HS diploma	Dummy for high school diploma	0.89

[a] For the Alberta SP data, travel costs were generated assuming travel distances of 50, 150, 250, and 350 km. For the Saskatchewan SP data, travel costs were generated assuming travel distances of 75, 250, and 425 km.

Table 2
Alberta parameter estimates

	RP data, no ASCs		RP data, ASCs		SP data		RP/SP data, no ASCs		RP/SP data, ASCs	
	Estimate	*t*-Statistic	Estimate	*t*-Statistic	Estimate	*t*-Statistic	Estimate	*t*-Statistic	Estimate	*t*-Statistic
Log-likelihood	−1,465.7		−1,386.4		−3,010.0		−4,817.8		−4,521.6	
RP/SP scale ratio		–	–	–	–	–	**0.448**	5.73	**0.240**	7.31
Travel cost	−0.615	−1.73	**−1.305**	−4.48	**−0.701**	−12.3	**−1.148**	−5.45	**−1.935**	−7.69
Unpaved	1.202	1.46	−0.200	−0.23	0.001	0.02	0.165	0.80	0.538	0.83
Gen hunt exp	−0.019	−0.68	0.030	0.82	−0.005	−1.29	−1.035	−1.30	−2.164	−1.78
Edmonton	–	–	–	–	**0.169**	2.34	0.274	1.78	**0.595**	2.17
HS diploma	**−3.026**	−2.15	**−3.355**	−2.26	0.008	0.06	−0.311	−0.93	−0.353	−0.60
Random effect	**1.898**	3.25	**1.974**	6.51	0.109	1.01	**0.356**	2.77	**0.554**	2.19
No trail	–	–	–	–	−0.105	−0.47	−0.444	−1.01	−1.936	−1.48
Gen hunt exp	–	–	–	–	0.001	0.11	0.927	0.51	0.622	0.19
Edmonton	–	–	–	–	**−0.763**	−4.25	**−1.366**	−3.23	**−2.219**	−3.32
HS diploma	–	–	–	–	0.149	0.45	0.015	0.01	1.679	1.46
Random effect	–	–	–	–	**0.820**	8.18	**1.039**	5.22	**1.861**	6.12
Old trail	0.758	0.80	–	–	0.313	1.51	0.253	0.40	1.861	1.84
Gen hunt exp	0.032	0.52	−0.023	−0.77	0.005	0.44	1.891	0.71	−1.079	−0.48
Edmonton	0.813	0.79	**1.536**	3.58	0.322	1.68	**1.301**	2.76	**1.997**	2.92
HS diploma	1.828	0.77	1.525	0.73	−0.201	−0.51	0.634	0.98	−0.693	−0.93
Random effect	**2.041**	3.14	**2.202**	5.96	**0.919**	9.30	**1.364**	8.02	**1.863**	6.77
4WD trail	–	–	–	–	0.150	0.87	−0.289	−0.49	0.737	0.98
Gen hunt exp	–	–	–	–	0.001	0.14	1.012	0.40	2.158	1.04
Edmonton	–	–	–	–	0.218	1.43	0.395	1.20	0.744	1.46
HS diploma	–	–	–	–	0.257	0.69	0.263	0.31	−0.658	−1.10
Random effect	–	–	–	–	**0.496**	6.23	**0.897**	3.93	**1.195**	4.61
No hunters	**−2.485**	−3.75	–	–	**1.272**	7.93	**2.293**	5.08	**3.357**	2.81
Gen hunt exp	0.001	0.07	−0.007	−0.28	**−0.017**	−2.85	**−2.750**	−2.08	**−4.725**	−2.05
Edmonton	**0.971**	2.41	**2.685**	4.30	−0.027	−0.20	−0.002	−0.01	−0.232	−0.47
HS diploma	−0.765	−0.71	−0.452	−0.30	0.134	0.38	0.356	0.53	0.998	1.13
Random effect	**0.925**	2.34	**1.298**	5.93	**0.397**	4.62	**0.510**	3.50	**0.940**	2.64
On foot	–	–	–	–	−0.127	−0.65	−0.180	−0.51	−1.216	−0.96
Gen hunt exp	–	–	–	–	0.003	0.34	0.293	0.19	1.107	0.40
Edmonton	–	–	–	–	**0.316**	2.11	0.524	1.73	0.909	1.77
HS diploma	–	–	–	–	−0.176	−0.50	−0.311	−0.48	0.750	0.69
Random effect	–	–	–	–	**0.382**	4.05	0.235	1.12	0.634	1.57
On ATV	–	–	–	–	**−0.430**	−2.27	**−1.541**	−3.92	−1.200	−1.16
Gen hunt exp	–	–	–	–	0.003	0.42	1.309	0.88	1.296	0.62
Edmonton	–	–	–	–	0.010	0.07	−0.022	−0.09	0.543	1.13
HS diploma	–	–	–	–	−0.223	−0.74	−0.460	−0.84	−0.591	−0.64
Random effect	–	–	–	–	0.251	1.87	**0.423**	2.66	**0.794**	2.88
No logging	1.243	0.85	–	–	−0.049	−0.39	0.158	1.04	−0.242	−0.67
Gen hunt exp	0.002	0.04	0.025	1.00	0.006	1.08	0.738	0.98	1.215	1.23
Edmonton	−0.469	−0.36	−0.405	−0.79	−0.013	−0.15	0.206	1.50	**0.538**	2.43
HS diploma	−0.149	−0.09	0.202	0.11	−0.211	−1.59	−0.292	−1.07	−0.126	−0.47
Random effect	**1.735**	3.38	**1.083**	6.00	**0.321**	3.97	**0.379**	3.22	**0.632**	2.79
<1 moose	**−1.791**	−1.96	–	–	**−1.801**	−6.95	**−2.187**	−4.23	**−5.702**	−5.31
Gen hunt exp	0.034	1.03	0.020	0.47	0.001	0.11	0.764	0.40	−1.795	−1.05
Edmonton	0.091	0.14	0.075	0.08	−0.080	−0.41	**−0.948**	−2.21	−0.391	−0.91
HS diploma	**−6.227**	−3.53	**−7.284**	−4.81	−0.396	−0.69	−1.674	−1.14	−0.197	−0.35
Random effect	**2.130**	4.54	**3.253**	4.78	**0.773**	7.86	**1.913**	6.34	**1.669**	7.06
1/2 moose	−0.485	−1.32	–	–	−0.101	−0.56	−0.002	−0.01	−0.990	−1.37
Gen hunt exp	0.015	1.00	0.005	0.20	0.001	0.07	−2.036	−1.69	−1.205	−0.86
Edmonton	**2.404**	5.70	**3.575**	5.21	−0.060	−0.42	**0.915**	3.26	**1.808**	5.30
HS diploma	0.215	0.24	0.727	0.59	−0.345	−0.97	0.642	1.03	0.088	0.15
Random effect	**1.558**	4.22	**1.661**	6.12	**0.517**	5.13	**0.890**	6.50	**1.721**	6.10
3/4 moose	−1.405	−1.67	–	–	**0.591**	3.57	0.384	1.26	2.052	1.52
Gen hunt exp	**0.068**	2.26	**0.065**	2.26	0.003	0.50	1.800	1.44	1.635	1.16
Edmonton	0.003	0.00	0.133	0.15	0.001	0.00	−0.234	−0.92	−0.134	−0.42
HS diploma	1.950	1.89	2.085	1.90	0.193	0.61	0.724	1.46	−0.042	−0.03
Random effect	**2.071**	3.40	**2.935**	6.16	0.041	0.30	**0.711**	5.55	**1.196**	6.11

Table 2 (*continued*)

	RP data, no ASCs		RP data, ASCs		SP data		RP/SP data, no ASCs		RP/SP data, ASCs	
	Estimate	*t*-Statistic	Estimate	*t*-Statistic	Estimate	*t*-Statistic	Estimate	*t*-Statistic	Estimate	*t*-Statistic
SP outside dummy	–	–	–	–	−**3.071**	−5.29	−**5.940**	−3.82	−**8.473**	−2.92
Gen hunt exp	–	–	–	–	−0.014	−0.53	0.242	0.06	3.473	0.50
Edmonton	–	–	–	–	−0.661	−1.47	−1.605	−1.49	−2.152	−1.27
HS diploma	–	–	–	–	0.494	0.77	**2.763**	2.16	−**3.330**	−1.98
Random effect	–	–	–	–	**2.518**	11.49	**4.533**	4.77	**9.200**	6.89
WMU #337 ASC	–	–	**2.443**	2.04	–	–	–	–	0.258	0.32
WMU #338 ASC	–	–	1.555	1.32	–	–	–	–	−**2.110**	−2.15
WMU #340 ASC	–	–	0.973	1.02	–	–	–	–	**1.508**	2.55
WMU #342 ASC	–	–	−1.526	−1.18	–	–	–	–	**5.417**	5.50
WMU #344 ASC	–	–	2.774	1.67	–	–	–	–	**7.149**	5.25
WMU #346 ASC	–	–	**3.653**	2.59	–	–	–	–	−0.916	−0.86
WMU #348 ASC	–	–	1.786	0.86	–	–	–	–	−**6.165**	−4.20
WMU #350 ASC	–	–	**3.816**	2.42	–	–	–	–	1.711	1.86
WMU #352 ASC	–	–	1.811	1.10	–	–	–	–	**5.609**	4.93
WMU #354 ASC	–	–	2.420	1.48	–	–	–	–	1.492	1.45
WMU #356 ASC	–	–	**4.598**	3.69	–	–	–	–	**4.317**	3.75
WMU #437 ASC	–	–	−2.784	−1.43	–	–	–	–	−**4.794**	−3.18
WMU #438 ASC	–	–	−2.907	−1.55	–	–	–	–	−**3.656**	−2.11
Heterogeneity test *p*-values										
H_0: Interact. = 0	<0.001		<0.0001		0.0062		<0.0001		<0.0001	
H_0: Random eff. = 0	<0.0001		<0.0001		<0.0001		<0.0001		<0.0001	
H_0: ASCs = 0	–		<0.0001		–		–		<0.0001	

Models w/	Interact., rand. eff., ASCs	Interact., rand. eff.	Some interact., rand. eff., ASCs	Interact., ASCs	Rand. eff., ASCs
RP/SP pooling test *p*-values					
H_0: Common RP and SP parameters equal	<0.0001	<0.0001	<0.0001	<0.0001	<0.0001

Boldface indicates statistical significance at the 5% level. All random coefficient estimates generated with 500 quasi-random draws. Alternative specific constant (ASC) for WMU #507 is excluded. For "Some interact., rand. eff., ASCs" specification in RP/SP pooling test section, all interactions that were not statistically significant at $\alpha = 0.05$ in the most general model in columns 9–10 were set to zero.

Table 3
Alberta welfare estimates (1993 Canadian dollars)

Specification	Loss of site WMU #344		Reduction in moose population at WMU #348 (>4 moose/day to 3–4 moose/day)		Increase in moose population at WMU #344 (<1 moose/day to 1–2 moose/day)	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
RP data—no ASCs	−$10.47	83.1	−$65.42	536	$8.82	123
RP data—ASCs	−$4.89	1.40	–	–	–	–
SP data	−$0.75	0.53	−$61.98	30.9	$2.88	1.40
RP/SP data—no ASCs	−$5.36	0.94	−$22.04	3.91	$2.99	2.37
RP/SP data—ASCs	−$4.18	0.47	−$17.01	1.88	$61.02	20.6
Additional results						
RP data—ASCs with SP data fill-ins	−$4.98	1.21	−$16.38	16.65	$1.10	3.00
SP data—ASCs from RP data	−$4.55	0.66	−$17.30	2.18	$33.23	9.54

Welfare measures are conditional on observed choice [29]; based on 3500 simulations with the first 500 discarded as burn-in. For the SP data specification, random parameters are simulated conditional on the SP data choices using a Metropolis–Hastings algorithm and then inputted into the standard 'log-sum' formula [27].

Table 4
Saskatchewan parameter estimates

	RP data, no ASCs		RP data, ASCs		SP data		RP/SP data, no ASCs		RP/SP data, ASCs	
	Estimate	*t*-Statistic	Estimate	*t*-Statistic	Estimate	*t*-Statistic	Estimate	*t*-Statistic	Estimate	*t*-Statistic
Log-likelihood	−833.1		−828.1		−5,583.2		−6,658.2		−6,547.5	
RP/SP scale ratio	–	–	–	–	–	–	**0.310**	7.55	**0.115**	6.11
Travel cost	−**1.561**	−5.83	−**2.293**	−4.01	−**0.381**	−11.7	−**1.262**	−7.89	−**2.888**	−6.56
2WD access	0.874	0.19	–	–	**0.536**	2.41	0.893	1.47	**3.633**	2.25
Gen hunt exp	−0.225	−1.86	−0.149	−1.41	0.003	0.59	0.013	1.13	0.021	0.74
HS diploma	0.696	0.20	1.058	0.23	−0.213	−1.31	−0.337	−0.76	−0.792	−0.82
Urban	0.105	0.05	−0.212	−0.08	−**0.233**	−2.08	−**0.671**	−2.08	−**2.104**	−2.53
Random effect	**5.767**	4.05	**17.64**	2.73	**0.613**	10.2	**1.981**	6.78	**4.408**	6.15
4WD access	–	–	–	–	−0.107	−0.54	0.197	0.34	−0.683	−0.47
Gen hunt exp	–	–	–	–	0.000	−0.05	0.003	0.22	0.013	0.43
HS diploma	–	–	–	–	**0.283**	1.99	0.527	1.23	1.475	1.59
Urban	–	–	–	–	−0.017	−0.16	−0.067	−0.23	0.047	0.07
Random effect	–	–	–	–	**0.545**	9.75	**1.573**	6.92	**3.610**	5.90
No hunters	1.591	0.30	–	–	**0.790**	3.55	**2.168**	2.98	**5.929**	2.96
Gen hunt exp	0.023	0.31	0.012	0.23	−**0.014**	−3.40	−**0.041**	−3.02	−**0.105**	−2.83
HS diploma	−1.146	−0.31	−0.584	−0.21	0.213	1.38	0.778	1.66	1.843	1.48
Urban	4.219	1.44	4.381	1.59	−0.009	−0.09	0.084	0.24	−0.130	−0.14
Random effect	**3.962**	4.42	1.662	0.56	**0.637**	12.2	**1.716**	6.71	**3.678**	5.51
On foot	–	–	–	–	0.018	0.11	−0.425	−0.82	−0.416	−0.36
Gen hunt exp	–	–	–	–	0.003	0.86	0.018	1.93	**0.056**	2.34
HS diploma	–	–	–	–	−0.154	−1.22	−0.719	−1.79	−**1.827**	−2.00
Urban	–	–	–	–	0.054	0.61	0.463	1.60	0.650	1.01
Random effect	–	–	–	–	**0.245**	2.77	**0.952**	4.76	0.178	0.47
Forest	−1.626	−0.60	–	–	0.127	0.97	0.091	0.25	0.986	1.09
Gen hunt exp	0.050	1.02	0.036	1.03	0.001	0.54	0.005	0.58	0.020	1.07
HS diploma	1.107	0.59	0.708	0.42	**0.233**	2.75	**0.662**	2.78	**1.385**	2.24
Urban	−1.784	−1.34	−1.061	−0.86	−0.059	−0.71	−0.130	−0.55	−0.329	−0.70
Random effect	**2.825**	3.50	**8.822**	2.49	**0.384**	8.64	**1.129**	5.87	**2.227**	5.28
<1 moose	−4.128	−1.95	–	–	−**0.623**	−3.11	−**2.301**	−4.63	−**6.857**	−5.16
Gen hunt exp	−0.085	−1.73	−0.049	−0.84	0.005	1.24	0.008	0.86	0.011	0.65
HS diploma	0.356	0.26	1.668	0.53	−0.229	−1.50	−0.102	−0.34	−0.481	−0.96
Urban	**2.053**	1.98	1.730	1.39	−**0.230**	−2.30	−0.107	−0.44	0.166	0.39
Random effect	**4.707**	3.51	7.300	1.70	**0.577**	9.82	**1.523**	6.35	**2.780**	4.98
1 moose	–	–	–	–	−0.069	−0.40	0.278	0.57	1.101	1.11
Gen hunt exp	–	–	–	–	0.006	1.92	0.017	1.64	0.038	1.95
HS diploma	–	–	–	–	−0.006	−0.05	−0.055	−0.16	−0.325	−0.59
Urban	–	–	–	–	−0.019	−0.20	−0.360	−1.25	−**1.434**	−2.20
Random effect	–	–	–	–	**0.200**	2.09	**1.153**	6.50	**2.607**	6.17
Common species	−5.614	−1.94	–	–	−0.169	−1.01	−0.634	−1.27	−1.702	−1.30
Gen hunt exp	0.077	1.18	0.064	1.03	0.006	1.89	0.005	0.54	−0.001	−0.04
HS diploma	0.593	0.30	0.433	0.12	−0.192	−1.52	−0.192	−0.52	−0.383	−0.46
Urban	**2.873**	2.07	1.605	0.86	0.014	0.15	0.462	1.68	0.777	1.16
Random effect	**5.370**	2.62	1.741	1.42	**0.183**	1.97	**0.703**	2.18	1.394	1.88
Unseen species	–	–	–	–	−0.018	−0.11	−0.118	−0.23	−0.598	−0.45
Gen hunt exp	–	–	–	–	−0.003	−0.98	−0.011	−1.11	−0.019	−0.80
HS diploma	–	–	–	–	0.170	1.40	0.564	1.44	1.548	1.47
Urban	–	–	–	–	0.013	0.16	0.100	0.38	0.225	0.32
Random effect	–	–	–	–	**0.186**	2.20	**0.756**	3.18	0.700	0.40
SP outside dummy	–	–	–	–	−**2.377**	−3.37	−**6.574**	−3.64	−**17.95**	−3.33
Gen hunt exp	–	–	–	–	0.020	1.48	0.053	0.97	0.117	0.84
HS diploma	–	–	–	–	−0.688	−1.25	−**2.353**	−2.16	−**6.234**	−1.98
Urban	–	–	–	–	−**0.637**	−2.22	−**2.269**	−2.64	−4.478	−1.55
Random effect	–	–	–	–	**2.300**	18.21	**7.130**	7.05	**18.59**	5.65

Table 4 (*continued*)

	RP data, no ASCs		RP data, ASCs		SP data		RP/SP data, no ASCs		RP/SP data, ASCs	
	Estimate	*t*-Statistic	Estimate	*t*-Statistic	Estimate	*t*-Statistic	Estimate	*t*-Statistic	Estimate	*t*-Statistic
WMZ #55 ASC	–	–	**−19.92**	−3.18	–	–	–	–	−2.840	−1.87
WMZ #59 ASC	–	–	**−18.32**	−2.80	–	–	–	–	**−14.50**	−5.16
WMZ #60 ASC	–	–	**−8.429**	−2.40	–	–	–	–	**−3.786**	−2.90
WMZ #62 ASC	–	–	−10.12	−1.79	–	–	–	–	**2.705**	2.55
WMZ #63 ASC	–	–	**−17.25**	−2.66	–	–	–	–	**−2.900**	−3.33
WMZ #64 ASC	–	–	**−3.488**	−2.29	–	–	–	–	**−4.106**	−2.66
WMZ #65 ASC	–	–	**−5.052**	−4.66	–	–	–	–	**−3.701**	−2.32
WMZ #66 ASC	–	–	**−2.190**	−3.64	–	–	–	–	**−3.379**	−2.49
WMZ #67 ASC	–	–	**−4.628**	−3.05	–	–	–	–	−2.152	−1.71
WMZ #68 ASC	–	–	**−4.949**	−3.96	–	–	–	–	−2.332	−1.48
Heterogeneity test *p*-values										
H_0: Interact. = 0	<0.0001		0.2779		<0.0001		<0.0001		<0.0001	
H_0: Random eff. = 0	<0.0001		<0.0001		<0.0001		<0.0001		<0.0001	
H_0: ASCs = 0	<0.0001		<0.0001		<0.0001		<0.0001		<0.0001	

Models w/	Interact., rand. eff., ASCs	Interact., rand. eff.	Some interact., rand. eff., ASCs	Interact., ASCs	Rand. Eff., ASCs
RP/SP pooling test *p*-values					
H_0: Common RP and SP parameters equal	<0.0001	<0.0001	<0.0001	0.0022	<0.0001

Boldface indicates statistical significance at the 5% level. All random coefficient estimates generated with 500 quasi-random draws. Alternative specific constant (ASC) for WMU #507 is excluded. For "Some interact., rand. eff., ASCs" specification in RP/SP pooling test section, all interactions that were not statistically significant at $\alpha = 0.05$ in the most general RP/SP model in columns 9–10 were set to zero.

Table 5
Saskatchewan welfare estimates (1994 Canadian dollars)

Specification	Loss of site WMZ #59		Reduction in moose population at WMZ #59 (>3 moose/2 days to 1 moose/day)		Increase in moose population at WMZ #66 (1 moose/2 days to 1 moose/day)	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
RP data—no ASCs	−$72.92	45.1	–	–	–	–
RP data—ASCs	−$135.77	44.1	–	–	–	–
SP data	−$163.36	15.5	−$76.83	8.44	$34.83	3.99
RP/SP data—no ASCs	−$80.27	6.67	−$40.23	4.57	$26.57	3.68
RP/SP data—ASCs	−$50.51	5.60	−$42.72	4.58	$80.27	14.7
Additional results						
RP data—ASCs with SP data fill-ins	−$110.03	35.1	−$23.83	51.2	$28.48	39.0
SP data—ASCs from RP data	−$59.67	6.88	−$50.13	4.99	$43.09	8.71

Welfare measures are conditional on observed choice [29]; based on 3500 simulations with the first 500 discarded as burn-in. For the SP data specification, random parameters are simulated conditional on the SP data choices using a Metropolis–Hastings algorithm and then inputted into the standard 'log-sum' formula [27].

addition of ASCs (p-value <0.0001). Combined with the results in columns three and four, these results highlight the significant gains in statistical fit arising from controlling for unobserved site attributes in the RP data. Moreover, although the signs of the main effects parameters generally do not change with the addition of ASCs, their magnitudes change substantially (see, for example, the *Old trail*, <1 *Moose*, and 3/4 *Moose* variables). This finding suggests that correlations between observed and unobserved attributes are present in the data and lead to biased estimates.

On the basis of the RP, SP, and combined RP/SP results, it is possible to test whether the cross-equation restrictions embedded in the combined RP/SP model are statistically valid as is common in the RP/SP literature. A likelihood ratio test suggests that we can strongly reject the cross-equation restrictions (p-value <0.0001). This empirical finding contradicts earlier findings based on an application of more parsimonious models to the Alberta data

[2].[10] It also runs counter to experimental evidence that pooling the observed attribute parameters across the RP and SP data cannot be statistically rejected [18,25]. On discovery of this empirical result, we first conjectured that it may reflect that our rich models of observed and unobserved heterogeneity are over-fitting the RP and SP data and spuriously leading us to reject the cross-equation restrictions. We therefore considered a number of more parsimonious specifications where we dropped interaction terms selectively and in total (see the bottom of Table 2). *p*-values from these specifications similarly implied that the cross-equation restrictions could be strongly rejected. We also conjectured that measurement error in the RP data's observed attributes might explain our rejection of the cross-equation restrictions. Recall that the RP attributes are averages across relatively large land areas, often several thousand hectares in size. It is difficult for us to statistically evaluate this conjecture, although we suspect that it plays some role in our findings. Also, we recognized that one could logically conclude from our results that our maintained assumption that the RP and SP data-generating processes are the same is invalid, perhaps due to hypothetical or strategic bias in the SP data.[11] To the degree that differences in the behavioral models that generated the RP and SP data exist, they represent limitations for our identification strategy.

To further examine the differences between models, Table 3 reports partial-equilibrium[12] welfare estimates for three scenarios:

- *Scenario #1: Loss of site*—remove WMU #344 from the choice set (site #5).
- *Scenario #2: Reduction in moose population*—move from >4 moose per day to 3–4 moose per day at WMU #348 (site #7).
- *Scenario #3: Increase in moose population*—move from <1 moose per day to 1–2 moose per day at WMU #344 (site #5).

Following Ref. [28], each estimate is constructed conditionally on the observed choices.[13] For the specification using only RP data and no ASCs in row one, we find that all welfare estimates are identified but have large standard errors. This contrasts with the RP data with ASCs models in row two, which are only identified for the site loss scenario because the moose attributes' main effects are confounded with unobserved site attributes. The SP data estimates in row three are qualitatively similar and far more precise relative to their RP counterparts. They rely, however, on the restrictive and unrealistic assumption that unobserved determinants of sites do not influence choice.

Turning to the combined RP/SP models, rows four and five of Table 3 report estimates based on the specifications with cross-equation restrictions (columns 7–10 in Table 2). For the site loss and reduced moose population scenarios, we find qualitatively similar estimates. The reduced moose population scenario point estimates with the combined RP/SP data are smaller than those exploiting either RP or SP data only, but the latter have significantly larger standard errors. The increased moose population scenarios are qualitatively different with and without ASCs. The inclusion of ASCs results in much a larger point estimate ($61.02 versus $2.99), but the larger estimate also has a substantially larger standard error (20.6 versus 2.37). This empirical finding is consistent with Monte Carlo results that the exclusion of ASCs can lead to biased welfare estimates and spurious precision [19].

Because we strongly rejected the hypothesis that the cross-equation restrictions embedded in the combined RP/SP models hold, we also considered some alternative strategies for constructing welfare measures that relied on both data sources but did not impose cross-equation restrictions. The first strategy used the RP data estimates when available, and used the SP data estimates to 'fill in' unidentified estimates. This was accomplished by treating the ratio of travel cost parameters in the RP and SP models as an estimate of the scale parameter ratio, and using this estimated ratio to rescale the SP estimates and arrive at fill-in values for the missing RP estimates. The second strategy combines the SP estimates with estimates of the ASCs constructed with the RP data [24]. These ASC estimates were constructed by 'concentrated' maximum likelihood, i.e., fixing the main, interaction, and random effects at their estimated SP values and conditionally estimating a full set of ASCs and a scale ratio with the RP data. Welfare results based on these two strategies are reported in the additional results section of Table 3. We find that these estimates are qualitatively similar for the site loss and reduced moose population scenarios, but smaller and more precisely estimated for the increased moose population scenario. The divergence in the increased moose population estimates reflects the fact that the key parameters (<1 *Moose* and 1/2 *Moose*) are qualitatively different across the RP, SP, and combined RP/SP models. We do not take a stand

[10] Another notable difference between our and Ref. [2]'s specification is the weighting scheme used for the RP and SP choices. In our specification, every RP and SP *choice* receives equal weight. In their specification, every *individual* receives equal weight. Operationally, their strategy implies weighting each RP and SP choice by the inverse of the total number of RP and SP choices, respectively. We experimented with their weighting scheme and did not find qualitatively different results from what we report in Tables 2–5.

[11] Another explanation may be that the RP choices reflect group decisions made by multiple participants in a common recreation trip, while the SP choices reflect decisions made by a single individual.

[12] We do not consider general-equilibrium welfare measures that account for the endogenity of congestion as in Ref. [26] although our empirical specification includes a number of congestion-like variables such as *No Hunters*, *On foot*, and *On ATV*. We do this in part for simplicity but also because we do not have a good model for how changes in observable behavior arising from exogenous policy shocks can be linked to changes in our congestion variables.

[13] By this we mean that all unobserved heterogeneity that enters preferences on every choice occasion is simulated in a way that implies perfect in-sample prediction for every choice at baseline conditions. To accomplish this, we use a multi-stage iterative procedure that employs an adaptive Metropolis–Hastings algorithm to simulate the random parameters [28].

on which of these estimates is most defensible, but instead view the range of estimates as providing welfare bounds for this scenario.[14]

Tables 4 and 5 report a similar set of findings with the Saskatchewan data, which we briefly summarize here. Across the five different models reported in Tables 4 and 5, we find the inclusion of interaction and random effects to be strongly statistically significant. The parameter estimates reported in columns 1–6 in Table 4 also suggest that several parameters are not identified when either RP or SP data is used alone. Fusing the RP and SP data generates similar gains in terms of parameter identification (see columns 7–10), and the inclusion of a full set of ASCs generates a significant improvement in fit as well as qualitatively different welfare estimates (e.g., the moose main effects). Likelihood ratio tests of the cross-equation restrictions embedded in the combined RP/SP models are again strongly rejected.

Table 5 reports welfare estimates for three policy scenarios:

- *Scenario #1: Loss of site*—remove WMZ #59 from the choice set.
- *Scenario #2: Reduction in moose population*—move from >3 moose per 2 days to 1 moose per day at WMZ #59.
- *Scenario #3: Increase in moose population*—move from 1 moose per 2 days to 1 moose per day at WMZ #66.

The pattern of these estimates is similar to what we found with the Alberta data. Only the site loss welfare estimates could be constructed with the RP data due to multicollinearity in the moose attributes, and the SP welfare estimates assume that unobserved attributes have no role in determining choice. Some differences between the combined RP/SP welfare measures arise for specifications with and without ASCs, especially in terms of the increased moose population scenario. Comparisons across welfare estimates for combined RP/SP models that impose cross-equation restrictions and those that do not suggest a range of values for all three scenarios, although the estimates based on the RP models with SP fill-ins are notably imprecise.

6. Conclusion

We draw four general conclusions from our conceptual and empirical findings in this paper. First, accounting for heterogeneity—whether it arises from unobserved site attributes or observed and unobserved preference variation—is an issue of substantial importance in non-market valuation applications. Results from our moose hunting applications as well as previous results by Refs. [19,26,5] strongly suggest that accounting for heterogeneity can improve statistical fit, reduce bias, and alter policy implications. Second, our combined RP/SP approach to identifying preference parameters in the presence of unobserved determinants of choice represents a feasible and in many ways attractive alternative to RP approaches. By fusing these data sources, we circumvented the limitations associated with RP two-step estimators that require large choice sets, variation in the observed attributes, and instruments for endogenous attributes.

These first two points have relevance beyond the context and analytical approach we have considered here. Indeed, all applications considering the demand for quality-differentiated goods must in some way address unobserved aspects of consumers' preferences and commodity attributes. While the transportation and marketing literatures have made substantial use of combined RP/SP approaches other areas such as public finance, labor economics, and industrial organization have historically relied on RP data alone. Our sense is that problems in these areas could benefit from our approach. For example, measuring the demand for school quality through residential location decisions has a long history in public finance, but is made difficult by the endogenous nature of school quality and unobserved determinants of household sorting. Combining RP data on residential location choices with SP data on school quality preferences may help identify these confounding effects. Likewise, estimating the value of statistical life (VSL) implied by wage/mortality risk tradeoffs from labor market data is difficult due to selection effects and correlation between risk measures and unobserved job attributes. A systematic accounting for unobserved job and person-specific characteristics within the context of a combined RP/SP approach may be fruitful in this area as well [13]. Finally, analytical approaches beyond RUM analysis may benefit from the ideas we've presented. For example, count data models and hedonic regression equations may suffer from the same identification problems we've described in the context of RUM models. Versions of our solution tailored to these contexts may constitute areas for further research.

Our third concluding point acknowledges the limitations of our combined RP/SP approach. Most importantly, our identification strategy requires additional SP data that, with few exceptions, must be gathered in the same behavioral context as the RP data. In addition, the assumption of a common data-generating process across the RP and SP data sources must be maintained. Since this is a testable hypothesis, it is possible that our identification strategy will fail if the pooling hypothesis is rejected, as was the case in our application. We provide two remarks related to this issue. First, the state of knowledge on SP data gathering has advanced substantially in the 15 years since the first RP/SP fusing methods appeared, and recent literature seems to suggest that consequential data-generating processes can be effectively mimicked in experimental settings [22]. Our sense therefore is that ex ante steps can be taken during the design and implementation of the SP choice experiments that maximize the likelihood the RP and SP data can be pooled. While the specific steps will be context dependent, they are likely to involve focus groups, cognitive interviews, and pretests [15]. Second, even if

[14] One possible approach to refining these bounds would be to iteratively search for the largest number of parameters that are consistent across the RP and SP datasets [11,12]. Given our very rich specifications of observed and unobserved heterogeneity, such an iterative approach is computationally intensive. Moreover, iteratively searching for the most general pooling specification is subject to path-dependency and other pre-test estimation errors [17].

identification fails, pragmatic ex post steps may allow the analyst to arrive at a combined RP/SP characterization of preferences that is still preferred to RP or SP alone. For example, professional judgment and accumulated knowledge might suggest that some parameters are more credibly identified from one or the other data source. In the recreation context, we may have more confidence in the travel cost parameter estimated off the RP data, but prefer non-price attribute parameters estimated off the experimental SP design. In our application, we explored some of these pragmatic options and presented them as sensitivity analyses. Our conjecture is that pragmatic and transparent judgments on how to proceed if pooling is rejected will in general still be preferred to the alternative of ignoring unobserved attributes of the quality-differentiated goods. Indeed, for roughly half of the policy scenarios we considered, we found qualitatively similar estimates across three plausible approaches to welfare construction. For the other scenarios, our three approaches to constructing welfare measures implied bounds that may be sufficiently informative for policy.

Both the RP approach and the combined RP/SP approach to identifying preference parameters in the presence of unobserved attributes have strengths and weaknesses. We believe that neither approach is strictly preferred to the other; data environments will likely dictate which approach is adopted in future applications. Having said this, we also believe that future research should empirically compare both approaches with a common dataset to ascertain whether they generate qualitatively similar parameter and policy inferences. Such a comparison would demand a very rich dataset—one with SP data as well as RP data with many objects of choice, sufficient orthogonality in attribute space, and instruments for endogenous attributes. It would, however, have the potential to clarify the performance of both approaches in a relatively controlled data environment.

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Appendix

Likelihood function

Assume individual i takes T_i trips to one of J sites and responds to C choice experiments each consisting of K choice alternatives. Person i's implied simulated likelihood function is

$$l(i) = \frac{1}{R}\sum_{r}^{R}\left\{\prod_{t}^{T_i}\left[\prod_{j}^{J}\left[\frac{\exp(\boldsymbol{w}_{ijt}^{\mathrm{RP}}\tilde{\boldsymbol{\gamma}}_i^r + \boldsymbol{x}_j^{\mathrm{RP}}\tilde{\boldsymbol{\beta}}_i^r + \xi_j)}{\sum_m^J \exp(\boldsymbol{w}_{imt}^{\mathrm{RP}}\tilde{\boldsymbol{\gamma}}_i^r + \boldsymbol{x}_m^{\mathrm{RP}}\tilde{\boldsymbol{\beta}}_i^r + \xi_m)}\right]^{\mathbf{1}_{ijt}^{\mathrm{RP}}}\right]\prod_{c}^{C}\left[\prod_{k}^{K}\left[\frac{\exp(s(\boldsymbol{w}_{ikc}^{\mathrm{SP}}\tilde{\boldsymbol{\gamma}}_i^r + \boldsymbol{x}_{ikc}^{\mathrm{SP}}\tilde{\boldsymbol{\beta}}_i^r))}{\sum_n^K \exp(s(\boldsymbol{w}_{inc}^{\mathrm{SP}}\tilde{\boldsymbol{\gamma}}_i^r + \boldsymbol{x}_{inc}^{\mathrm{SP}}\tilde{\boldsymbol{\beta}}_i^r))}\right]^{1_{ikc}^{\mathrm{SP}}}\right]\right\},$$

where $(\tilde{\gamma}_i^r, \tilde{\boldsymbol{\beta}}_i^r)$ are one of R sets of simulated random parameters from the multivariate normal distribution, $(\mathbf{1}_{ijt}^{\mathrm{RP}}, \mathbf{1}_{ikc}^{\mathrm{SP}})$ are indicator functions identifying the chosen alternative for each RP trip and SP choice experiment, and s is the ratio of RP and SP scale parameters, μ/μ^*.

Additional results

Additional results are shown in Table A1.

Table A1
Second-stage OLS regression results for RP models with ASCs

Parameter	Estimate	Bootstrapped t-statistics
Alberta data ($N = 13$)		
Constant	**−1.252**	−2.04
Old trail	**2.075**	3.05
No hunters	**−4.159**	−4.02
Logging	0.011	0.02
<1 Moose	−1.203	−1.44
1/2 Moose	−0.506	−0.82
3/4 Moose	−1.341	−1.58
Saskatchewan data ($N = 10$)		
Constant	−4.910	−1.19
2WD access	−1.220	−0.22
No hunters	2.272	0.51
Forest	−1.863	−0.81
<1 Moose	**−5.818**	−2.05
Common species	−3.837	−1.57

Boldface indicates statistical significance at the 5% level.

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