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Does anybody like water restrictions? Some observations in Australian urban communities*

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Mandatory water restrictions continue to be the immediate response to urban water shortages in most major cities in southern Australia. Whilst generally rejected by economists on efficiency grounds, restrictions and the enforcement regimes used to invoke them are, nonetheless, viewed by some in the community as a positive way of dealing with water scarcity. Given the likelihood that urban water restrictions will persist for some time, there is value in understanding householders' attitudes in this context. The impact and acceptability of differing approaches to enforcement is of particular interest, because this has wider ramifications for the administration of policy generally. This paper uses the results from a choice experiment to investigate the interplay between different components of a water restriction regime. In stark contrast to prevailing views that focus on the community benefits from 'sharing the pain of water shortages', results point to the significance of being able to inform on ones neighbours as a component of the enforcement regimes.

Key words: choice experiment, consumer preferences, enforcement, urban water restrictions, water policy.

1. Introduction

The welfare costs of urban water restrictions are now well recognised, if not yet quantified with precision (for e.g. Edwards 2008). Mandatory restrictions can be time-consuming, costly to enforce and require a significant investment in education and marketing (White *et al*. 2003). Notwithstanding these costs, governments have proven reluctant to abandon them, at least until additional infrastructure is in place to support supplies, and even then have shown enthusiasm for their contribution. In addition, there is at least some evidence that political leaders and sections of the community see value in restriction regimes in their own right, in part, owing to the sense of shared hardship and the 'community building' response this purportedly invokes. Moreover, water restrictions are frequently cited as a means of encouraging greater awareness of water use and promoting greater environmental

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consciousness (for e.g. ABC News 2008a; City West Water 2007). Consequently, some form of behavioural constraint over the use of water is now applied in almost every major urban centre in Australia.

There is a legal obligation on citizens to comply with water restrictions that limit the outdoor use of water such that breaches can often be observable to others. Subsequently, private punishment has occurred in the form of threats, vandalism and even murder (The Border Mail 2007; ABC News 2008b), which clearly reduces social welfare. This type of behaviour has been reported on numerous occasions across NSW and Victoria, where those residents seeking complete compliance with water restrictions across their city have been coined 'water vigilantes' (The Border Mail 2007). Shavell (1993) has observed that private punishment is not likely to be efficient given that victims, generally, are concerned about the harm they have suffered and are not concerned about the benefits to the offender and potential effects on third parties. In the current context, understanding preferences for a water restrictions regime and hence likely compliance may assist in achieving deterrence of private punishment and confrontational elements. For instance, does society prefer to rely on water inspectors to detect breaches rather than social dimensions which may encompass alternative motives?

This paper considers these issues by presenting the results of a choice experiment study based on data from New South Wales and Victoria. The data from water-rich and water-poor regional and metropolitan communities offer insights into the preferred make-up of a water restriction regime, so that the influence of these variables over the preferences of water consumers can be considered.

The paper itself is divided into six parts. Section 2 explores several dimensions of compliance and delinquent behaviour. In section 3, we briefly consider the theoretical groundings of choice experiments, whilst section 4 presents the design employed for this study. The results of the choice experiment are reported in section 5. More specifically, we report the significance of various attributes of a compliance regime. The final section discusses the core findings before offering some brief concluding remarks.

2. Theoretical underpinnings of compliance

Amongst the most prominent theories regarding regulatory compliance are those stemming from calculated motivations for compliance. The seminal work by Becker (1968) proposes that the regulated will comply with a particular regulation when they perceive the benefits of compliance, including avoidance of fines and penalties, surpass the associated costs of not complying (see also Stigler 1970; Ehrlich 1972). Although the approach to this calculation may vary, depending on how an individual evaluates the benefits and costs of compliance, the process of selecting between complying and not complying is based on the expected utility in terms of net return (Becker 1968). Similarly, the instrumental perspective holds that individuals are motivated solely by self-interest and react to immediate incentives and penalties related to certain behaviour (Tyler 1990). Conversely, the normative perspective highlights that individuals give regard to moral and just behaviour, rather than simply considering what is in their immediate self-interest (Tyler 1990).

Several additional factors have been recognised as influencing consumers' choices (Stern 2000) and presumably some of these hold for choices around a compliance regime. First, numerous studies have identified attitudinal factors such as values, norms, beliefs and attitudes as potentially being strong influences of choices and behaviour (for e.g. Fishbein and Ajzen 1975; Seligman 1989). Secondly, Shafir (2007) suggests that the pressures applied by seemingly trivial situational factors can pose restraining forces or can lead to inducing forces that may be harnessed to great effect. Put differently, situational factors can have a large impact on behaviour.

Achieving compliance with restrictions policies and overcoming delinquent behaviour can be a challenging task that may be aided by an increased understanding of consumers' preferences for a compliance regime and motivations fostered by different regimes. The remainder of this paper is used to describe the choice experiment used to accomplish this task. More specifically, the core tasks of this research are to (i) identify the pertinent attributes that comprise a water-using compliance regime; (ii) understand individual's preferences for a compliance regime for water restrictions; and (iii) identify whether socioeconomic, situational and psychographic variables have a significant impact on preferences for a compliance regime surrounding urban water restrictions.

3. Survey design and sampling

3.1. Survey development

This research generally followed the overall experimental design process established by Hensher *et al.* (2005), involving focus interviews, focus groups, survey pretesting and development of an efficient design. This process was designed to reveal the attributes of the 'product', an urban water restrictions compliance regime and relevant attribute levels. The sample was drawn from Sydney, Goulburn, Albury, Wodonga, Bendigo and Melbourne, providing scope for analysis on several dimensions: comparisons between Victorian and NSW jurisdictions; regional and metropolitan settings; and urban communities with differing levels of water scarcity.

Initially, 13 in-depth interviews were conducted with water industry experts from each of the water authorities, such as water restrictions operations managers, compliance team leaders, environmental consultants, public affairs managers and water patrol officers. In addition, group discussions were held with residents from the sample cities. These 1–1.5 hour discussions were conducted with five to 10 participants from community groups. The attributes chosen for pretesting were those that respondents raised and understood, that policy makers could influence and that could be measured and potentially improved.

3.2. The stated preference (SP) experiment

A SP experiment was used to collect data to examine the behaviour related to policies centred on urban water restrictions compliance regimes.[1] The experiment invited respondents to review three alternatives consisting of a status quo (SQ) alternative and two hypothetical SP alternatives. Based on the attribute levels of the alternatives, respondents were asked to select their preferred alternative from the three on offer.

The alternatives in each survey task were described by four attributes: a per annum cost where the payment vehicle would be an additional charge on their water bill; the number of inspectors per household to patrol householders' outdoor water usage; an attribute to act as a proxy for the value individuals place on exposure to information in the media was included in the form of 'frequency of exposure' to informative media advertisements regarding water restrictions; and the ability to report neighbours noncompliance via a hotline to a team who would process the complaint.

For each respondent, the attributes of the last alternative represented the SQ situation. In total, respondents were asked to review 12 choice tasks each. The four attributes and their levels developed for the main survey are presented in Table 1. An example choice screen is shown in Figure 1.

3.3. The underlying experimental design

The experimental design underlying a SP experiment can have an influence on the final results of the study. Exactly how analysts distribute the levels of the design attributes over the course of an experiment, as determined by the underlying experimental design, may play a big part in whether an independent assessment of each attribute's contribution to the observed choices can be determined. Further, the allocation of the attribute levels within the experimental design may also impact on the statistical power of the experiment, insofar as its ability to detect statistical relationships that may exist within the data. Given a set of attributes and attribute levels, the problem for the analyst is thus how best to allocate those levels over the course of the experiment.

A number of criteria for the allocation of attribute levels have been suggested in the past. Traditionally, the most common criterion applied has been that of orthogonality. An emerging literature has begun to question the appropriateness of using orthogonal designs (including those that force attribute-level differences across alternatives) for SP studies involving the

[1] In an attempt to address the potential challenge of adverse behaviour, that is respondents who breach water restrictions deliberately selecting compliance regime alternatives that will minimise the likelihood that they will get caught, a series of statements were included before the choice experiment. These statements highlighted the generally undesirable outcomes of people not complying with water restrictions (e.g. reduced water reliability in the immediate term, more severe water restrictions in the future and the costs attending the need to source alternative water supplies).

Table 1 Attribute levels used in the choice sets

Attributes	Descriptor	Status Quo	Levels
No. of inspectors	Ratio: inspector per household	1:10,000	1:1000; 1:2000; 1:5000; 1:8000; 1:50,000; 1:200,000
Information	Frequency of household exposure (days)	Every 90 days	Everyday; every 7 days; every 14 days; every 31 days
Increase in water bill (WTP)	$ per annum	$0 per annum	$2; $5; $10; $20; $50; $100
Able to report your neighbour	Yes; no	No	Yes; no

WTP, willingness to pay.

Which enforcement & education package would you choose?		Price of the enforcement package	Number of inspectors	Information	Able to report your neighbour
Package 1		$5 per year	1 per 8000 households	Every 14 days	Yes
Package 2		$50 per year	1 per 5000 households	Every 7 days	No
Neither		$0 per year	1 per 10 000 households	Every 90 days	No

Figure 1 An example of a stated choice screen.

estimation of nonlinear models (see, e.g., Scarpa and Rose 2008). This line of enquiry postulates that researchers willing to abandon orthogonality may be able to trade-off the ability to independently assess each attribute's influence on choice (which does not matter for nonlinear models under a nonzero parameter estimate assumption) against the statistical power of the design (i.e. the ability to detect statistical relationships at given sample sizes). This class of design is known as *efficient* designs.

For the present study, an *efficient* design was generated. Given a set of attributes and attribute levels, *efficient* designs are constructed such that the levels are allocated to the design in such a way that the elements (or subsets thereof) of the variance–covariance (VC) matrix are expected to be minimised once data are collected. Rather than work with the elements in the VC matrix directly, the literature suggests working with different measures that summarise the values that populate the VC matrix. One such measure is the D_p-error, which is given as:

$$\mathrm{Det}[I(\beta)^{-1}]^{\frac{1}{k}}, \tag{1}$$

which is the determinant of the inverse of the Fisher information matrix, I, for a design given a particular econometric model form and certain parameter estimates, scaled by the inverse of the number of parameters, k.

To calculate Equation (1) for a design, the analyst must first assume a set of prior parameter estimates. If these are not known with certainty (as would typically be expected), the analyst may use prior parameter estimates drawn from Bayesian distributions and calculate the Bayesian D-error statistic, D_b-error, which is represented as:

$$E_\beta[\det(I(\beta)^{-1})^{\frac{1}{k}}] = \int_{R^k} \det(I(\beta)^{-1})^{\frac{1}{k}} f(\beta)\mathrm{d}t. \tag{2}$$

where $f(\beta)$ is the distribution function assumed for β.

To generate a D-*efficient* design, whether Bayesian parameter priors are assumed or not, different attribute-level allocations are tested, with attribute-level combinations that produce lower D-error estimates, representing more statistically *efficient* designs. Such designs are expected to produce data that will maximise the t-ratios for the design parameters (for further discussion on the generation of such designs, see, e.g., Ferrini and Scarpa 2007 or Scarpa and Rose 2008). In the current context, a D_b-efficient design was generated with 24 choice situations with a D_b-error with a mean of 3.000×10^{-05}. As recommended by Scarpa and Rose (2008), we computed the D-errors based on the asymptotic variance-covariance matrices for the final estimated models (see Section 5). The D-errors (normalised to $N = 1$) for the two models were 1.667×10^{-05} and 2.612×10^{-05} for the multinomial logit (MNL) and mixed multinomial logit (MMNL) models (both described in Section 4), respectively. Thus, the ratio of the design D-error to the D-errors computed for the two models were 1.800 and 1.149. Whilst, as noted by Scarpa and Rose (2008), these values are largely meaningless by themselves without the ability to undertake empirical benchmarking, a ratio close to 1 would suggest that the data collected closely align with the assumptions undertaken in generating the design. It is hoped that by reporting such values from multiple empirical studies, researchers and practitioners may in the future be better able to understand the relationship between the statistical design and the final model outputs, and also allow for future benchmarking in terms of how well the assumptions used in constructing SC experimental designs transfer to empirical data sets.

3.4. The sample

The data used in this paper were collected in Australia, in April 2008. The questionnaire consisted of four parts. The first part contained questions regarding respondents' attitudes towards water restrictions. The choice experiment was presented in the second section, and questions regarding

Table 2 Sociodemographics of the survey respondents

Metropolitan (Sydney, Melbourne)	40%
Rural or regional centres (Albury, Wodonga, Goulburn, Bendigo)	60%
New South Wales	48%
Victoria	52%
Average age	42 years
Average household income before tax	$978 per week
Own their home	30%
Male	40%
Completed a tertiary degree	34%
Have a lawn and/or garden that requires watering	85%
Have an outdoor pool or spa	15%

the respondents' socioeconomic status were presented in part three. The final section was used to probe respondents about their willingness to pay (WTP) to avoid water restrictions.[2] The focus of the remainder of this paper will be on the results and findings of the choice experiment.

The main survey was distributed by an online survey company via email to a sample of residents from the predefined study locations. The online data collection method carries with it potential biases (see, e.g., Fleming and Cook 2007). However, researchers who employ SP methodologies are increasingly employing online surveys (Morrison *et al*. 2005; Hensher *et al*. 2007), because they are proving to be a superior approach to this type of data collection on a number of dimensions, that is speed of data collection, cost-effectiveness and accuracy of data collection (Fleming and Cook 2007). The final data set consisted of 512 respondents (Wodonga: 54; Albury: 94; Melbourne: 106; Sydney: 102; Goulburn: 51; and Bendigo: 105), which represented a response rate of 59 per cent. The characteristics of the sample are presented in Table 2.

A χ^2 test was used to determine whether the proportions of the sample differed significantly from the distribution of the general population, as reflected in the 2006 census data. Some response bias was evident, but not unexpected considering the topic of the survey and the mode of data collection. It was concluded that there are significant differences between the age, highest education level attained, and income profiles of the sampled population and that of the general Australian population. The gender profile of the sample was not significantly different to the general population.

4. Model specification

4.1. Model specification

In this paper, we report the results of a MMNL with error components. To understand these models, let U_{nsj} denote the utility of alternative j obtained by respondent n in choice situation s. The utility U_{nsj} may be partitioned into

[2] See Cooper *et al*. (2010) for a review of the contingent valuation analysis conducted with this data.

three components, V_{nsj}, η_{nj} and ε_{nsj}. Under this representation, utility may be written as:

$$U_{nsj} = V_{nsj} + \eta_{nj} + \varepsilon_{nsj}. \tag{3}$$

V_{nsj} in Equation (3) is assumed to be described by a linear relationship between observed attribute levels of each alternative, x, and their corresponding weights (parameters), β.

Under the MMNL, the parameter weights are no longer assumed to be fixed over the sampled population. Rather the parameter weights are assumed to vary with density $f(\beta|\Omega)$, over respondents, n. The assumption that preferences vary between and not within respondents provides for a determination of preference heterogeneity after accounting for the pseudopanel nature of SP data (Revelt and Train 1998).

As such, V_{nsj} can be further partitioned as:

$$V_{nsj} = \sum_{k=1}^{K} \beta_{njk} x_{nsjk}. \tag{4}$$

As well as containing information on the levels of the attributes, x in Equation (4) may also contain up to $J - 1$ alternative specific constants (ASCs) capturing the residual mean influences of the unobserved effects on choice associated with their respective alternatives, where x takes the value 1 for the alternative under consideration or zero otherwise.

The second component of utility, η_{nj}, represents what are known as error components. Unlike the random parameters contained within V_{nsj}, error components are associated with subsets of alternatives, j, not attributes, x. To estimate the model, the analyst first specifies a set of dummy variables, with each dummy variable able to appear in the utility specifications of up to $J - 1$ alternatives. Next, normally distributed random parameters with means normalised to zero, represented as η_{nj} in Equation (3), are estimated for each of the defined dummy variables. By associating each η_{nj} with different subsets of alternatives, the parameters (which represent SDs set around a mean of zero) capture different common error variances associated with those alternatives for which they are estimated. This is similar to what can be done with a nested logit model, but much more flexibly in the case of the error components model.

If one does not specify error components as part of the utility functions of the model, then Equation (3) will collapse to:

$$U_{nsj} = V_{nsj} + \varepsilon_{nsj}, \tag{5}$$

Finally, the unobserved component of utility, ε_{nsj} , is typically assumed to be identically and independently extreme value type 1 (EV1) distributed.

Assuming that the ε_{nsj}s are EV1 independent and identically distributed (IID) and that preferences are homogenous within the sampled population, the probability, P_{nsj}, that respondent n chooses alternative j in choice situation s is given by the MNL model,

$$P_{nsj} = \frac{\exp(V_{nsj})}{\sum_{i \in J_{ns}} \exp(V_{nsi})}. \tag{6}$$

The MMNL model (i.e. including random parameters and error components) differs from the MNL model in that the model assumes that (some of) the parameters are random, following a certain probability distribution. In the panel version of the model, what is estimated is the probability of observing the sequence of choices made by each respondent. To this end, we define the probability P_n^* that a certain respondent n has made a certain sequence of choices $\{j|y_{nsj} = 1\}_{s \in S_n}$ with respect to the set of choice situations, S_n, by:

$$P_n^* = \int_\beta \int_\eta \prod_{s \in S_n} \prod_{j \in J_{ns}} (P_{nsj})^{y_{nsj}} f(\beta|\Omega) d\beta f(\eta|\theta) d\eta, \tag{7}$$

The probabilities in Equation (7) do not have a closed form and need to be approximated by simulating the distribution of random parameters and error terms (Train 2009). For convenience and to ensure coverage of the entire space of the parameter distributions, typically researchers rely on quasi-random Monte Carlo methods (for e.g. Bhat 2001; Train 2009). The expected probabilities derived from Equation (7) are then used to calculate the log-likelihood function for the model. Search algorithms are then used to find population parameter estimates that maximise this likelihood function.

4.2. Coding of variables and the SQ

Given the qualitative nature of the nonprice attributes, the possibility of nonlinearity in the marginal utilities between levels needs to be considered. Typically, such nonlinear relationships are represented using one of two data-coding structures, these being dummy coding and effects coding. Dummy coding utilises a series of 0s and 1s to relate each attribute level of the original variable to the newly created columns. Table 3 demonstrates the dummy coding concept for the information attribute used in the current experiment (see Hensher *et al.* 2005, p. 144 for a review of the dummy coding process employed here).

Effects coding is similar to dummy coding in that it allows the analyst to detect nonlinearities in the marginal utilities for levels of attributes rather than assuming a linear relationship between an attribute's levels and overall utility. However, effects coding offers a number of theoretical advantages over dummy coding. In particular, if two or more attributes are dummy

Table 3 Example dummy coding

Information attribute levels	Original code	Dummy code			
		Everyday	7 days	14 days	31 days
Everyday	0	1	0	0	0
Every 7 days	1	0	1	0	0
Every 14 days	2	0	0	1	0
Every 31 days	3	0	0	0	1
Every 90 days	4	0	0	0	0

coded, then each will have its own 'base' level where all dummy-coded columns are set at zero. In this way, the 'base' levels of several dummy-coded variables will be perfectly confounded with each other, or a model constant if one is present.

Effects coding overcomes this by changing the base level in the coding structure in such a way as to allow for a unique estimate for that level. This is carried out by changing 0 to −1 in each column for the base attribute level, as shown in Table 4.

The base level of an effects-coded variable will no longer be equal to zero. Rather, it will be equal to minus the sum of the remaining parameter estimates, as shown in Equation (8). In this way, the base levels of different effects-coded variables will have a unique estimate, unlike dummy-coded variables where the base marginal utility will be zero for all attributes.

$$\beta_{90\mathrm{days}} = -\beta_{\mathrm{everday}} - \beta_{7\mathrm{days}} - \beta_{14\mathrm{days}} - \beta_{31\mathrm{days}} \tag{8}$$

In the current experimental design context, however, an added complexity arises in that the 90 day information attribute level exists only for the SQ alternative. As such, this base level will be perfectly confounded with the SQ alternative and this level will effectively act as an additional ASC in the model if the effects codes are estimated as per Table 4. For this reason, we apply a hybrid coding approach, where we have, in effect, two base levels: one effects and one dummy. Table 5 demonstrates this coding structure where we use the '31 day' interval as the base effects-coded level and set the '90 days' level as the dummy base level.

As with the information attribute, the 'number of inspectors' attribute has a unique attribute level associated with the SQ alternative (1:10,000). As such, the same hybrid coding structure used for the information attribute was also applied to this attribute during model estimation. For the reporting attribute, the 'no' level appears in both the SQ and SP alternatives, and hence, a standard effects coding structure was applied in this case with 'yes' coded 1 and 'no' −1. The cost attribute was not transformed, and the actual values shown to respondents were used for modelling purposes.

Table 4 Example effects coding

Information attribute levels	Original code	Effects code			
		Everyday	7 days	14 days	31 days
Everyday	0	1	0	0	0
Every 7 days	1	0	1	0	0
Every 14 days	2	0	0	1	0
Every 31 days	3	0	0	0	1
Every 90 days	4	−1	−1	−1	−1

Table 5 Example of hybrid coding structure

Information attribute levels	Original code	Hybrid code		
		Everyday	7 days	14 days
Everyday	0	1	0	0
Every 7 days	1	0	1	0
Every 14 days	2	0	0	1
Every 31 days	3	−1	−1	−1
Every 90 days	4	0	0	0

Table 6 Covariates used in modelling

Water	Do respondents live in a water-poor or water-rich city	Water poor = 1 Water rich = 0
Lawn	Do respondents have a lawn/garden that requires watering	Yes = 1 No = 0
Home	Do respondents own their home	Yes = 1 No = 0
Metropolitan	Do respondents live in a metropolitan or regional centre	Metropolitan = 1 Regional = 0
E-values	Respondents' perception of their own environmental values	Factor score derived from 5 scale items

A number of covariates were also introduced into the utility equation for the SQ alternative. Table 6 provides a list of the covariates and the coding structure adopted for each.

4.3. Accounting for reference alternative (SQ) effects using the error components model

When faced with an SP choice task in which a SQ is present, there exists the possibility that respondents may treat that alternative systematically different to other alternatives present within the choice task. Systematic differences may arise as a result of (i) respondents being asked to choose either from the SQ, of which they have actual real-world experiences, or hypothetically constructed, nonexperienced alternatives, or (ii) from the fact that the SQ alternative is typically held constant across choice tasks, whereas the remaining alternatives are forced to vary by way of the experimental design. Theoretically, it is likely that the hypothetical alternatives of SP experiments involving SQ alternatives will be more highly correlated with each other than with the SQ alternative (see, e.g., Scarpa *et al*. 2007; Train and Wilson 2008). Traditionally, this correlation has been captured via the inclusion of additional error components that are shared across the non-SQ in the utility specification of the model, but is absent from the utility function of the SQ.

Status quo effects have been shown to have significant impacts on model results (see Scarpa *et al*. 2007 for a discussion of the literature), and hence, accounting for them is important. Ferrini and Scarpa (2007) conducted Monte Carlo simulations comparing different utility specifications and conclude that the use of an error component model approximating a nested logit model (c.f. Train 2009) is less prone to mis-specification than other model forms in the presence of SP experiments with a SQ alternative. The utility structure used in the current study is shown as Equations (9a–c).

$$U(sp_1) = \beta_{sp_1} + \tilde{\beta}x_{sp_1} + \beta_{\text{cost}}x_{\text{cost}_{sp_1}} + \eta_{sp} + \varepsilon_{sp_1}, \tag{9a}$$

$$U(sp_2) = \beta_{sp_2} + \tilde{\beta}x_{sp_2} + \beta_{\cos t}x_{\cos t_{sp_2}} + \eta_{sp} + \varepsilon_{sp_2}, \tag{9b}$$

$$U(\text{SQ}) = \tilde{\beta}x + \beta_{\text{cost}}x_{\text{cost}_{\text{SQ}}} + \delta z_n + \varepsilon_{\text{SQ}}, \tag{9c}$$

where $\tilde{\beta}$ are random parameters associated with the non-cost attributes, β_{cost} is a fixed parameter associated with the cost attribute, β_{spj} are ASCs, η_{sp} is a zero-mean normally distributed error component associated solely with the SP alternatives, δ are fixed parameters associated with covariates z_n, and ε. are unobserved influences on utility which are IID EV1 distributed. The presence of δz_n in the utility function for the SQ alternative allows for a determination as to whether different segments of respondents are more or less likely to choose the SQ alternative.

4.4. Obtaining individual-specific conditional parameter distributions

The preference heterogeneity parameters obtained from MMNL models represent population-level estimates. Such parameters, in the form of parameter distributions, do not allow the analyst to easily determine where any particular individual's preference lies in the distribution. Fortunately, it is possible to construct estimates of individual-specific preferences. To do so, the individual's conditional distribution based (within-sample) on their observed choices may be derived (for e.g. Hensher *et al*. 2006). Estimation is undertaken through simulation to produce maximum simulated likelihood estimates for the conditional mean for each random parameter, as given in Equation (10).

$$\hat{E}(\beta_n) = \frac{\int\limits_{\beta}\int\limits_{\eta_{sp}} \beta_n \prod_{s=1}^{S_n} \frac{\exp(\beta_{nj}x_{ni} + \eta_j)}{\sum_j (\beta_{nj}x_{ni} + \eta_i)} f(\beta|\Omega)d\beta f(\eta|\theta)d\eta}{\int\limits_{\tilde{\beta}}\int\limits_{\eta_{sp}} \prod_{s=1}^{S_n} \frac{\exp(\beta_{nj}x_{ni} + \eta_j)}{\sum_j (\beta_{nj}x_{ni} + \eta_i)} f(\beta|\Omega)d\beta f(\eta|\theta)d\eta}. \tag{10}$$

The approach in Equation (10) can also be used to estimate the conditional variance of β_n by Equation (11).

$$\mathrm{var}(\beta_n) = \int\limits_{\beta}\int\limits_{\eta} (\beta_n - E(\beta_n))^2 f(\beta|\Omega)d\beta f(\eta|\theta)d\eta. \tag{11}$$

The estimated conditional variance will be smaller than the average variance obtained simply by computing the sample variance from the estimated conditional means, as the latter is averaged over all data in the sample, whilst the former is averaged with respect only to the data for individual n (Hensher *et al*. 2006).

Of particular interest is the estimation of the conditional individual-specific WTP values. Typically, this would be computed by the ratio of the coefficients for the various attributes to the coefficient for price. However, the ratio of two randomly distributed terms tends to have a very large variance, such that the variance of WTP is often larger than reasonable, and in the case where the cost parameter assumes a random distribution that is not bounded at zero, the resulting WTP distributions may have undefined population moments (Daly *et al*. forthcoming). In the current study, we overcome this difficulty by maintaining a fixed cost attribute that alleviates issues with taking the ratio of two random parameters. Individual-specific WTP distributions are calculated using the method described by Scarpa *et al*. (2007), however, with a fixed cost parameter. The conditional mean of the individual-specific WTP distribution is thus calculated using Equation (12).

$$\hat{E}(\mathrm{WTP}_n) = \frac{\int\limits_{\beta}\int\limits_{\eta_{sp}} \frac{\beta_n}{\beta_{\mathrm{cost}}} \prod_{s=1}^{S_n} \frac{\exp(\beta_{nj}x_{ni} + \eta_j)}{\sum_j (\beta_{nj}x_{ni} + \eta_i)} f(\beta|\Omega)d\beta f(\eta|\theta)d\eta}{\int\limits_{\tilde{\beta}}\int\limits_{\eta_{sp}} \prod_{s=1}^{S_n} \frac{\exp(\beta_{nj}x_{ni} + \eta_j)}{\sum_j (\beta_{nj}x_{ni} + \eta_i)} f(\beta|\Omega)d\beta f(\eta|\theta)d\eta}. \tag{12}$$

5. Results

Table 7 presents the results for two models, an MNL model and an MMNL model with an error component associated with the two SP alternatives.

Table 7 Model results

	MNL model		Mixed multinomial logit model			
	Mean parameters		Mean parameters		SD parameters	
	Par.	(*t*-ratio)	Par.	(*t*-ratio)	Par.	(*t*-ratio)
Constant (SP1)	0.143	(1.24)	0.668	(0.82)	–	–
Constant (SP2)	0.284	(2.48)	0.913	(1.11)	–	–
No. of inspectors (1:1000)	0.337	(6.54)	0.545	(4.36)	0.967	(6.94)
No. of inspectors (1:2000)	0.518	(10.86)	1.126	(9.86)	1.024	(8.30)
No. of inspectors (1:5000)	0.356	(7.43)	0.921	(9.71)	0.736	(6.02)
No. of inspectors (1:8000)	0.154	(3.07)	0.576	(4.63)	1.116	(7.72)
No. of inspectors (1:10,000)	0.000	–	0.000	–	–	–
No. of inspectors (1:50,000)	−0.637	(−11.63)	−0.997	(−7.97)	1.006	(3.69)
No. of inspectors (1:200,000)	−0.727	–	−2.170	–	–	–
Informing (≤every 14 days)	−0.015	(−0.82)	0.086	(1.73)	0.458	(4.06)
Informing (every 31 days)	0.015	–	−0.086	–	–	–
Informing (90 days)	0.000	–	0.000	–	–	–
Reporting (yes = 1, no = −1)	0.292	(14.03)	0.513	(6.92)	1.168	(9.85)
Cost	−0.023	(−26.23)	−0.053	(−35.61)	–	–
Covariates in status quo alternative						
Home	−0.365	(−7.93)	−0.948	(−2.87)	–	–
Lawn	0.267	(7.06)	0.541	(2.00)	–	–
Metropolitan	0.474	(6.99)	1.302	(2.69)	–	–
Water	0.326	(4.52)	0.925	(1.78)	–	–
E-values	−0.365	(−11.56)	−1.053	(−4.98)	–	–
Error component	–	–	–	–	4.121	(17.45)
Model fits						
LL (0)	−6749.874		−6749.874			
LL (β)	−5791.899		−3805.641			
ρ^2	0.142		0.436			
Adj. ρ^2	1.140		0.432			
AIC	1.890		1.253			
BIC	1.907		1.301			
No. of observations	6144					
No. of parameters	15		44			

AIC, Akaike information criteria; BIC, Bayesian information criteria.

The MMNL was estimated assuming a multivariate normal distribution for the random parameter estimates. The elements of a full Cholesky matrix were estimated so as to impose a correlation structure between the multivariate random normal parameters of the utility function. The estimated elements of the Cholesky matrix are not reported here (but are available on request): instead, only the derived value for the SD of the random parameter is reported. One thousand Halton draws were used in the estimation process.

For the MNL model, the ASC for the second alternative is statistically significant and positive, suggesting that this alternative is chosen more often than the other two alternatives, all else being equal. The numbers of inspector parameters coded using the hybrid coding structure were all found to be statistically significant with magnitudes suggesting a preference for one inspector per 2000 households, with a decrease in preference after this number. Examining the marginal utilities for this attribute, there appears to be a significant change in preferences as one moves from one inspector per 8000 to one inspector per 50,000 households and only a marginal decrease in preferences is observed with the movement to one inspector per 200,000 households. This supports the need to treat the attribute as being nonlinear in the marginal utilities over the attribute levels.

A number of different coding structures were attempted for the attribute associated with the frequency with which households are provided with information, but without much success, including the coding scheme given in Table 5. In the model reported in Table 7, a single generic parameter was estimated for the provision of information ≤14 days whilst keeping 31 days as the effects code base and 90 days as the dummy code base. For the MNL model, the ≤14 days parameter was not significant, suggesting that this attribute is not statistically different from either 31 or 90 days information provision. The positive and statistically significant reporting parameter suggests a preference for respondents being able to report their neighbours for breaching water restrictions.

Examining the parameters for the covariates that entered in the SQ utility function, respondents who own their own home are more likely to choose the SQ alternative than others. Likewise, respondents who produced a higher factor score representing a more favourable perception of values towards the environment are also more likely to choose the SQ alternative. Respondents who own a lawn and live in the metropolitan area or who live in a water-poor area are more likely to chose an alternative other than the SQ alternative, all else being equal.

The second model reported in Table 7, the MMNL model, appears to provide a statistically significant improvement in model fit over the MNL model (a model comparison produces a −2 log-likelihood value of 3972.516 which should be compared to a value for $\chi^2_{0.05,29} = 42.557$ at the 5 per cent level of significance) despite requiring the estimation of 44 parameters compared with 15 for the MNL model with a large increase in the adjusted ρ^2 value (computed against a zero parameters models) and nonmarginal decreases in the Akaike information criteria and Bayesian information criteria statistical values. Examining the ASCs, neither ASC is statistically significant in the model, suggesting that any bias in choices towards the second alternative is now accounted for by either the random parameter estimates or the error component.

All random parameters are statistically significant with magnitudes of the SD parameters being almost as large as the mean parameter values. For instance, the parameters associated with the number of inspectors per household being one in every 2000, 5000 or 50,000 almost double for the parameters associated with the number of inspectors per household being one in every 1000 and 8000. This suggests that significant amounts of preference heterogeneity exist within these data which were not accounted for in the

MNL model. Also, although not reported in the table, the diagonal and below diagonal values of the Cholesky matrix show strong evidence of correlated attributes, which makes an uncorrelated specification inappropriate. In particular, the model suggests that there exist strong positive correlations between the random parameter associated with number of inspectors (1:5000) attribute and the random parameters associated with number of inspectors (1:1000) and (1:2000) dummies (i.e. ρ(1:1000, 1:5000) = 0.854 and ρ(1:2000, 1:5000) = 0.846). The model further suggests strong negative correlations between the random parameter associated with number of inspectors (1:2000) and number of inspectors (1:50,000), and between the random parameters linked to the number of inspectors (1:8000) and informing < 14 days (i.e. ρ(1:2000, 1:50,000) = −0.727 and ρ(1:8000, informing < 14 days) = −0.767). This suggests that individuals who have a more positive marginal utility for number of inspectors (1:5000) are more likely to also have a more positive marginal utility for having a number of inspectors as a ratio of 1:1000 or 1:2000 in the population. Likewise, the model implies that respondents with a more positive marginal utility for having a ratio of number of inspectors equal to one per every 2000 households will more likely have a negative marginal utility for having a ratio of one inspector per 50,000 households. Finally, the results suggest that respondents who are more favourably predisposed to having one inspector for every 8000 households are less likely to support having information provided every 14 days or less.

Before discussing the results associated with the ratio of inspectors per household and the number of days that information is provided to each household, it is worth noting once more that in interpreting the marginal utility parameters for these attributes, the effects coding of these attributes implies that the associated value of the attribute should be interpreted relative to the design mean; however, because of the SQ effect and the hybrid coding, this is conditional upon selecting a non-SQ option. Examination of the 'number of inspectors' attribute suggests that, as with the MNL model, on average, respondents prefer one inspector per 2000 households. Unlike the MNL model, however, the results for the MMNL model suggest that on average, there exists a considerable decrease in the marginal utility in the movement from one inspector per 50,000 households to one inspector per 200,000 households. Examination of the mean information parameter confirms the finding of the MNL model, suggesting that on average, at a 5 per cent confidence level, respondents were indifferent between the time interval between the receipt of information, although at a 10 per cent confidence level, the model suggests a preference for information to be provided every 14 days or less. The relatively large and statistically significant SD parameter estimate suggests significant preference heterogeneity for this attribute, however, with many respondents appearing to prefer information being provided less frequently (i.e. less repetition of information). As with preferences associated with the ability to report neighbours derived from the MNL, the MMNL model suggests that on average, respondents also have a preference for being able to report breaches of water restrictions, although the large statistically significant SD parameter suggests that this preference is not universally held within the population. The parameters for the covariates conform with those of the MNL model, suggesting that those who own their own home and those who report being more environmentally conscious are more likely to select the SQ alternative, whilst those respondents with a lawn, who live in the metropolitan area or who live in a water-poor area are less likely to select the SQ alternative, ceteris paribus.

As is expected, the error component associated with the two SP alternatives is statistically significant. This is consistent with the findings of other researchers that respondents are more likely to substitute between the SP alternatives than they are to substitute between an SP and SQ alternative. The statistically significant error component also hints at greater levels of error heterogeneity with the SP alternatives than with the SQ alternative.

Comparing the mean parameter estimates from the MMNL model with those of the MNL model provides compelling evidence of scale differences. Although not definitive, all mean parameter estimates from the MMNL model are larger in magnitude than those of the MNL model, suggesting that after accounting for both preference heterogeneity and the correlation in substitution patterns between the SP alternatives, the MMNL model has higher scale (lower error variance) than the MNL model.

Table 8 reports the WTP values for the MNL and MMNL models. In the table, positive WTP values suggest desirable attribute levels, whilst negative WTP values suggest undesirable attributes. Two sets of WTP values are reported for the MMNL model. Firstly, the WTP values at the mean of the unconditional parameter estimates reported in Table 7. The last two columns of the table report the median and SD based on the WTP estimates calculated from the conditional parameter distributions. That is, these latter two values were derived from the population distribution formed from the simulated

Table 8 Willingness to pay outputs

		Mixed multinomial logit		
	MNL	Unconditional	Conditionals	
	Mean	Mean	Median	SD
No. of inspectors (1:1000)	$14.95	$10.20	$6.76	$12.87
No. of inspectors (1:2000)	$22.99	$21.08	$19.03	$14.35
No. of inspectors (1:5000)	$15.78	$17.24	$14.79	$10.24
No. of inspectors (1:8000)	$6.84	$10.78	$12.86	$14.45
No. of inspectors (1:50,000)	−$28.28	−$18.67	−$18.07	$12.23
No. of inspectors (1:200,000)	−$32.28	−$40.62	−$34.90	$28.57
Informing (≤every 14 days)	−$0.67*	$1.61*	$0.78*	$5.72*
Informing (every 31 days)	$0.67*	−$1.61*	$0.78*	$5.72*
Reporting (yes = 1, no = −1)	$14.95	$9.60	$6.05	$17.27

*Not statistically significant at the 5 per cent confidence level.

means of the individual-specific WTP values calculated using Equation (12). Because of the way in which the experiment was constructed, the WTP estimates for the 'number of inspectors' per households can only be interpreted relative to the WTP values associated with the other attribute levels. For example, the model suggests that respondents are on average willing to pay 10.88 to move from one inspector per 1000 households to one inspector per 2000 households (i.e. 21.08–10.20).

Examination of the table suggests that overall, the MNL tends to overestimate the mean WTP of the sample, although a few exceptions exist, such as one inspector per 8000 households and one inspector per 200,000 households. Examining the SD of the calculated WTP values based on the conditional parameter distributions suggests that significant WTP heterogeneity exists for these attribute levels.

6. Discussion and concluding remarks

Establishing people's preferences for compliance regimes is an important element to developing effective policy and may enhance social welfare, particularly if we accept that water restrictions are likely to be a medium- to long-term component of urban water management.

This study sought to identify the statistically significant attributes that comprise a compliance regime. Three attributes proved significant: (i) the price of a compliance regime, (ii) the number of water patrol officers, and (iii) a service to facilitate reporting others for not complying with water restrictions, whilst a fourth attribute, the frequency of exposure to information about water restrictions, was found only to have marginal levels of significance. Analysis reveals the average survey respondent values modifying the compliance regime to have a service that enables the reporting of others for what they might perceive as 'water abuse', although considerable heterogeneity for this attribute does appear to exist. This finding carries with it a number of implications. For instance, this may infer that respondents have the perception that there is merit in complying with water restrictions and therefore believe that people should be complying. This is consistent with the predominant view that there is value associated with 'saving water', regardless of whether such 'savings' are real (for e.g. Crase and O'Keefe 2009). Alternatively, respondents may opt to have a 'reporting mechanism' available to avoid private confrontation with those breaching water restrictions. Regardless of the underlying motivations that led to this result, this calls into question the popular notion of water restrictions being akin to a community strengthening exercise.

The results of the choice experiment also reveal that, on average, respondents have a preference for having one water inspector per every 2000 households, more so than having one water inspector per every 1000 households. However, the results show far less preference for having fewer inspectors than more. This suggests that they value formal deterrence mechanisms in relation to the regulation of water use. Notably, this type of incentive may also have a negative impact on social cohesion and thus potentially undermine moral and social incentives to comply with water restrictions.

The estimates are inconclusive as to whether respondents have a preference to increase the frequency of information appearing in the media regarding water restrictions. Given the nature of this attribute, it is reasonable to suggest that respondents do not wish to be inundated with information. Consistent with this notion are the consumer behaviour concepts of 'habituation' and 'advertising wear-out', which occur when consumers are overexposed to particular stimuli (for e.g. Blackwell *et al.* 2006). In the context of water restrictions in Australia, Watson (2005) has also bemoaned the negative impacts of the 'save every last drop' dogma promoted by governments. These results also cast some doubt over the persistent calls for 'educating the community' (for e.g. Victorian Women's Trust 2005) as a rational vehicle for dealing with this public policy dilemma.[3]

This study also aimed to identify whether socioeconomic, situational and psychographic variables have a significant impact on preferences for a water restrictions compliance regime. The results of the choice experiment support the view that these types of variables can, in part, explain individual preferences for moving away from the current SQ. Accordingly, different segments within society will have varied preferences regarding an optimum compliance regime for water restrictions.

The policy implications of this analysis are significant. Presently, state jurisdictions impose a range of constraints to limit household water use. Clearly, this approach is not unanimously supported, although many would appear to favour a more rigorous application. This is also consistent with the near-daily reports in the popular media promoting water restrictions as a positive force for social cohesion and an effective vehicle to achieve behaviour modification. The results from this work should be used to seriously challenge proponents of water restriction regimes and raise questions about the longer-term social impacts of their deployment. Put simply, it is hard to reconcile these results with the view that water restrictions make for a more cohesive society striving to overcome an inconvenient hydrology whilst simultaneously becoming more informed about environmental matters generally.

A useful extension of this work would involve the development of additional welfare estimates associated with avoiding water restrictions. This could further inform policy formulation and provide a basis for challenging the presumption that water restrictions have some intrinsic merit in their own right.

[3] It needs to be noted that it is a fine line between 'educating' the community and 'informing' the community. We contend that in this case, elements of the recent information campaigns about water use appear to have 'crossed that line', at least for a large number of the participants in this survey.

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