Assessing animal priorities: future directions

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Matthews (1998) concludes his response to our paper (Mason et al. 1998) by arguing that measuring demand is a valuable and important tool in animal welfare research, and we whole-heartedly agree. A danger with exchanges such as these is that rather than stimulate, they depress and confuse those active in the field. We would therefore like to respond to Matthews (1998) and also to Sherwin & Nicol (1998) by emphasizing the many unexplored research areas that our three papers identify, and by making more positive recommendations as to how the many techniques available for assessing animals' priorities should best be used. We also acknowledge here, with apologies, an error we made in interpreting the method of Matthews & Ladewig (1994): their pigs were indeed in a closed economy with respect to physical social contact (if not all aspects of social interaction) when tested with conspecifics as reinforcers. However, we would also like to correct some errors made by both Matthews (1998) and Sherwin & Nicol (1998).

We begin with our areas of continued disagreement. In our paper, we argued that manipulating access fees for unlimited periods of interaction is not a suitable method for constructing demand curves. Sherwin & Nicol suggest that in this, we were benefiting from hindsight, and are also guilty of selective quotation. However, the distinction between costs on switching and costs on performance has long been recognized (e.g. Larkin & McFarland 1978). Furthermore, Sherwin & Nicol's response that they used the term 'luxury' to refer to activities that drop out when time is rationed is a weak defence, as the experiment they discussed used fees (traverses of water) that took little from the animals' time budgets. Such quibbles aside, we emphasize that we pointed out

the dangers of inappropriately using costs of switching to generate demand curves for a constructive reason: because this error has been made independently in four laboratories (Collier et al. 1990; Cooper & Appleby 1995; Sherwin & Nicol 1995; Sherwin 1996; G. Mason, unpublished proposal) and therefore may well be made again. We agree with Sherwin & Nicol (1998) that the resulting data on behavioural reorganization are interesting theoretically, but for studies aiming to discover what resources would most improve the welfare of captive animals, such data would simply be of little use.

To solve this problem, we suggested two means by which experiments could yield valid demand curves, while also allowing animals to schedule their own behaviour (e.g. not involving the experimental interruption of activity bouts). Matthews (1998) claims that these two methods violate the requirement that the amount of cost borne and the amount of resource used co-vary, but he is mistaken. For example, our first suggested method was to use an operant set-up in which a price (e.g. 10 lever presses) yields a unit of reward (e.g. 5 min access to a substrate), and in which the animal can, if it chooses, pay this price repeatedly to have an extended, uninterrupted period of access (e.g. 50 lever presses for 25 min of access). At this price, 1 min of access always requires two lever presses however the animal schedules its behaviour; and thus total price paid and amount of resource gained do co-vary. This technique may be difficult to implement (we concur with Sherwin & Nicol), but it is certainly not invalid. The same is true of our second suggested technique (making the actual performance of behaviours costly, e.g. by imposing simultaneous thermoregulatory costs).

Where we agree with our respondents is that there are many exciting research avenues still to be explored. Building on the suggestions of Sherwin & Nicol, these include investigating how constraints on behavioural frequency (e.g. as might

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result from social competition) affect welfare; how behavioural priorities are affected by the subjects' social context, and other aspects of housing such as the number of alternative activities available (see Mason et al. 1997); and how behavioural prioritization is affected by the stimulus properties of local resources: for example, if animals working to enter empty compartments do so merely to monitor them, this suggests not that these animals 'like' having extra space, but that if extra space is there, patrolling will be elicited.

Matthews' objection to our argument that interruption may devalue some activities also highlights an area needing research. We maintain that not all activities resemble the obtaining of food (etc.) in the unimportance of the unit-size offered, and that for some activities, for example, sleep, interruption has a devaluing effect so that the size of the unit offered per fee will affect demand. We therefore hypothesize that pigs' demand for social contact would prove less elastic than found by Matthews & Ladewig (1994) if allowed, say 5 or 10 min of contact at a time, rather than 15 s. This is an empirical question, and only data, not argument, will yield an answer.

In our final paragraph, we advocate that scientists should generate both valid demand curves and valid rankings of behavioural priorities that reflect those of the animal (Sherwin & Nicol are themselves guilty of selective quotation here). This was to emphasize our point that consumer demand techniques, although fashionable, are not the only ways of assessing animal priorities. For example, measuring the maximum price an animal will pay to reach a resource is not a consumer demand technique, and this frees the experimenter from a requirement of consumer demand methodology: that costs are imposed in terms of a constrained budget (e.g. time). Costs such as the requirement to overcome an aversive stimulus are not paid out of a constrained budget: they are best modelled as negative commodities that are titrated against positive commodities by the animal. Such costs can be used validly only with 'non-economic' techniques (to answer points made in both replies).

Sherwin & Nicol express confusion over which method we most favour. Our response is similar to the multidimensional approach they recommend. However, it is not enough to select randomly several methods of measurement for comparison, as the issues raised by all the authors in this exchange suggest that some methods are more

suitable for some scenarios than for others. For example, requiring animals to work for fixed units of reward, and then increasing the price per unit (cf. Matthews & Ladewig 1994) is ideal if pilot studies confirm that the opportunities on offer are not devalued by being offered in small fixed units. Restricting time budgets, to assess income elasticity, can disproportionately penalize activities that already require a lot of time, but may be an ideal means of comparing the importance of activities that, unconstrained, take similar amounts of time to perform. Finally, looking at the maximum price an animal will pay is a profitable method to use where experiments can be left to run over an extended period, for example several weeks: this will minimize the noise emphasized by Matthews resulting from short-term variations in motivational state.

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