BEHAVIOURAL ECONOMICS AND ENVIRONMENTAL POLICY DESIGN

Project Description

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1. Background

Behavioural economics is an area of research that tests the orthodox ("neoclassical") assumptions that individuals are rational, possess all freely available information, and self-interested. This area of research has identified a number of consistent behavioural patterns which systematically violate these assumptions.

Behavioural economics research has played an important role in informing policymakers in areas such as personal health, labour markets, consumer markets and personal finance. A number of OECD governments have put in place mechanisms which encourage the consideration of insights from behavioural economics in their deliberations concerning policy design. In the United Kingdom the "Behavioural Insights Team" sits directly in the Cabinet Office, and has been active across a variety of policy fields.[1] In the European Commission the Directorate-General for Health and Consumers has launched a framework programme to support behavioural economics work in the Commission as a whole.[2] In the United States the Office of Management and Budget has drawn inspiration from behavioural economics in the areas of health-care provision and financial regulation.

The methods utilized in behavioural economics research include laboratory and "field" experiments (or trials), sometimes involving many thousands of people. The advantages of using methods of this kind are that they are easily interpretable by non-experts (e.g. they do not require sophisticated theoretical modelling), and yet provide a scientifically valid way of testing the effectiveness, costs, and public acceptability of innovative policy instruments.

In the environmental sphere, behavioural economics can inform public policy in two ways: (a) improving benefit-cost analysis (BCA) through methodological adjustments to nonmarket valuation techniques, and (b) informing the development of policy mechanisms to influence environmental behaviour (see Shogren and Taylor 2008 and Brown and Hagen 2010 for recent reviews.)

The majority of research on behavioural economics in environmental policy currently focuses on non-market valuation. Perhaps the most important insights relate to the interpretation (and thus treatment) of protest responses, as well as the economic valuation of risk and investments in projects with cost or benefits in the distant future. In addition, behavioural economics has cast light on issues such as "hypothetical bias," as well as reasons for the common finding that WTA does not equal WTP. These have

[1] https://update.cabinetoffice.gov.uk/resource-library/behavioural-insight-team-annual-update

[2] See http://ec.europa.eu/consumers/conferences/behavioural_economics2/programme_en.htm for details of a conference on consumer policy.

important implications for valuation of non-market goods, and thus the optimal setting of policy stringency.

However, in this project we focus on the implications for policy design. In order to ensure that policy mechanisms have their intended impact, more applied policy research is required on how responses and attitudes toward specific environmental policies differ from those predicted by standard theory. This is an issue which policy-makers are starting to recognise as potentially being of some importance.[3]

2. Possible Areas of Applied Policy Research

Behavioural anomalies can be explained by factors such as bounded rationality, loss aversion, status-quo bias, cognitive framing, context-dependency, mental accounting and others. However, the full repercussions of different biases remain unclear, and have unknown implications for policy design and implementation. Insights from behavioural economics are likely to benefit particularly *environmental policy* because many of the "choices" that have significant environmental implications are the outcome of a complex set of motivations. Indeed, such environment-related decisions often require careful consideration between external (e.g. financial), internal (e.g. intrinsic motivations) and social (e.g. norms) factors.

This project aims to summarize some of the main policy implications for a sub-set of specific policy areas in which many of these apparent anomalies are important. Possible areas include: energy and water use; resource conservation and agriculture, and waste generation and recycling. With a view toward implementing laboratory or (preferably) field experiments in several countries, specific areas of applied policy research might include the following:

1. *Can "referencing" and peer pressure complement "hard" environmental policy instruments?* For example, can providing information on energy use in the household's community affect consumption choices? There is evidence that some types of low-cost appeals can measurably affect environmental behaviour, such as energy conservation (e.g. letters to households showing their energy use relative to their neighbourhood average). In the first instance it would be interesting to know whether these appeals have different impacts than information on trends on one's own consumption, and how important the impacts are likely to be. Even if the absolute impact of such information provision is not large, it is possible that when implemented in conjunction with a "harder" policy measure, such as the roll-out of smart meters, they may have a much larger impact than the "hard" policy on its own. Testing this would necessitate collaboration with electricity providers who are rolling out smart meters in the next few years. Another potential application would be in real-time metering of water consumption or fuel consumption in cars.

2. *Can governments facilitate bottom-up collective action in the management of natural resources?* For example, can facilitating stakeholder groups among farmers facilitate the attainment of ambient water quality standards and other environmental objectives? An informative experiment could analyze the effectiveness of a mandatory tax/subsidy/permit scheme for nutrient pollution in combination with a "softer" intervention, such as the provision of incentives for the voluntary formation of catchment (watershed) stakeholder organizations where farmers and other "polluters" interact and work together to collectively achieve an objective. Other potential applications would be in the management of biodiversity, water salinity, and other non-point

[3] For example, the European Commission financed a review of the literature in a number of key areas. See http://ec.europa.eu/environment/enveco/pdf/RealWorldConsumerBehaviour.pdf

source (diffused) environmental problems that require broad support from farmers, forest-managers, or fishermen. There are many case studies of successful watershed organizations and partnerships with government. For example OECD's 2006 *Environmental Performance Review* of Korea, describes a successful self-enforcing catchment organization created by residents near the Daepho River, in response to government pressure.

3. *Risk communication and cognitive biases.* It has long been known that people have difficulty assessing low-probability but high-impact risks in a consistent manner. This issue could be examined in the context of the development of residential communities in zones at risk of flooding, which has become a problem in many OECD countries. In many cases the perceived risk is not sufficiently important (i.e. once- in-100 year floods) to have an appreciable impact on housing location decisions. Many people are not able to reason about such levels of risk in a coherent manner in terms of trade-offs with other aspects of their housing location choice. Assessing the effect of alternative forms of risk communication on housing location decisions could be undertaken.

4. *How does the effect of the mere fact of pricing the environment differ from the level of the price itself?* In the area of telecommunications it has been found that people behave in an anomalous manner when comparing situations of "zero" prices with exceedingly low prices. In the environmental context, it would be interesting to assess conditions under which a "price" (however low) provides information about the value of the environmental good that is distinct from the level of the price itself? For example, does the fact of introducing a price on a resource (e.g. water) even at very low rates, have a distinct effect on consumption relative to the effect an equal increase in a pre-existing price. An alternative case might relate to the use of voucher schemes (or rebates) offering discounts on goods that generate positive externalities (e.g. flow-restricting showerheads, compact-fluorescent light bulbs, and other Energy Star appliances). They are a common and well-studied method for distributing health-related goods, but less studied in environmental policy.

5. *How important are "reference points" or status quo bias?* There is considerable literature on market failures in markets for tradable permits. However, behavioural issues have not usually been addressed when discussing permit allocation rules. Given that "grandfathering" is the most common form of allocation mechanism for tradable permit schemes it might be interesting to examine the role of differences in initial allocations on subsequent trading patterns. Do market participants seek to "hold on" to permits allocated, even when apparent opportunity costs exceed the market price?

6. *Can "commitment devices" increase the effectiveness of environmental policy instruments?* In financial decisions it has often been found that people sometimes lack "self-control". There may be analogues in the environmental sphere. For example, what are economically efficient designs for deposit-refund schemes for household hazardous waste? Environmental economists have observed that these systems may be particularly attractive for dealing with hazardous waste, such as car batteries and e-waste. A simple experiment could consist of comparing a newly implemented deposit-refund system for some good (e.g. batteries, cell phones) to a simple surtax (of varying value) which could not be refunded. This would allow for testing of issues such as commitment and discounting. Other areas might include length of commitment to "green energy" tariff structures, or decisions by farmers with respect to payments for environmental services.

7. *Do different policy measures have an effect on the way in which people consider the environmental good to be conserved?* For example, across OECD countries there is wide

variation in the mechanisms used to control the ecological impacts of congestion in national parks and to raise revenue for the maintenance of biodiversity and ensuring equitable access to countries' natural heritage. Some countries emphasize price-based policies—with user fees comprising the main instrument for limiting congestion. Others limit congestion by issuing user permits, sometimes through lotteries. And finally, some countries may limit access by restricting nearby parking and other means of access. In many cases a combination of these strategies is adopted. It is possible that people have preferences for different access policies unrelated to their efficiency, effectiveness or equity impacts. Moreover, policy choice can in turn affect users' attitudes and values for conservation areas – i.e. the policies can affect how the "good" is considered by users. This is likely to be particularly true for relatively pristine areas.

8. *Can pricing "crowd out" social norms, increasing costs of implementation?* For example, does monetary compensation mitigate or exacerbate the NIMBY phenomenon? Decisions over siting important infrastructure projects (landfills, hazardous waste sites, wastewater treatment facilities, industrial sites) are often impeded by local opposition (also called not-in-my-back-yard, or NIMBY, phenomenon). This contributes to the administratively long lead times for such projects. However, there are significant environmental benefits associated with more streamlined procedures. An experiment could be designed in which we would test the effect of monetary compensation (and alternatives) on the resolution of siting conflicts.

9. *Do preferences for fairness affect the acceptability (and costs of enforcement) for different policy types?* For example, do households' concerns for fairness affect their preferences for policy alternatives to increase water efficiency? The alternatives for limiting water use could include (a) increasing residential water tariffs uniformly by a given amount (e.g. by 10%) and (b) restricting water use for certain activities, such as watering gardens, washing cars, running decorative fountains, swimming pools, etc. This experiment would be suitable in countries where new residential water conservation policies are currently being proposed. Another potential application would be management of urban congestion.

10. *How important is the "framing" of information?* For example, do alternative designs of carbon content labels affect their effectiveness as a non-price incentive? Several OECD countries have started introducing carbon footprint labelling schemes (voluntary or mandatory). Given that such labels can be designed in many different ways (incl. metric and reference used, the balance of public and private benefits, etc.), this seems like a suitable moment to examine how the manner in which information is provided affects consumers' decisions. For example, little work has been done on the relative importance of information which relates to the "private" (i.e. financial) and "public" (environmental) dimensions of the good in question. Taking the example of household appliances it would be possible to "unbundle" the two effects through an appropriately designed experiment.

11. *Can "green first" defaults be an effective means to affect consumers' choices?* Mandating certain defaults (rather than leaving the service provider to set their own defaults) has been applied in other domains of public policy. For example, in some countries pharmacies are obliged to offer a generic drug before they offer a more expensive alternative. In the environmental domain, possible applications include defaults for information on public transport (online mapping services), green cab (municipal taxi), tap water (restaurants), stop&start systems (cars), thermostat settings (buildings), "green energy" tariffs, or parking facilities which favour bicycles relative to cars. In some cases the feasibility of mandating certain defaults (e.g. green energy tariffs) will depend upon regulatory and competition frameworks, and this should be taken into account.

This list indicates that the implications of behavioural economics for environmental policy design extend well beyond "nudges", but rather have implications for all policy types from taxes and regulations to information- and process-based measures. In many cases the issues overlap, and thus careful design is required in order to generate results with policy implications.

3. Work Programme

The proposed work has been initiated with a review of the insights gained from the behavioural economics literature relevant for environmental policy-making in OECD countries. The objective of this review is to identify the most important hypotheses for environmental policy design, considering both the relevance of different policy instruments and the potential for unconventional behavioural responses to these instruments. A report will be presented to the OECD Working Party on Integrating Environmental and Economic Policies at its meeting in June 2012.

To complement this, the OECD Secretariat is now developing a "catalogue" of existing policy experiences in which the outcomes can be better understood through the lens of behavioural economics. The focus will be on environmental policy examples but insights from the implementation of policies in other fields will be examined where this is relevant.

On the basis of these two elements the OECD Secretariat is developing possible methodologies and experiments to evaluate a set of policy-relevant hypotheses. This would include laboratory and/or field experiments. It would be particularly interesting to run a set of field experiments across multiple countries. Possible collaborating research teams would be identified with the necessary experience to carry out these experiments in three or four OECD countries.[4] The objective would be to carry out original research which would provide concrete policy guidance for a sample of the issues identified above.

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[4] There are many economics laboratories around the world, for some examples see http://en.wikipedia.org/wiki/Experimental_economics#Experimental_economics_organizations.

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Annex: An experiment to test the effect of defaults on thermostat settings of OECD employees

A summary by Zack Brown, OECD

Introduction

As mentioned above, there are many documented cases in which the default option in a choice task can strongly influence the behaviour of economic agents. By default option, we mean an alternative that is automatically selected when an agent makes no active choice. Although standard economic theory implies that such defaults should not determine choices when there is no cost associated with switching from one option to another, in practice numerous scientific demonstrations have been conducted in which default settings do evidently influence agents' choices: The most famous examples include the demonstration that employee selection of retirement plan is strongly determined by the default option (Madrian and Shea 2001), and an experiment showing that an opt-in default for organ donor programs can double participation rates relative to an opt-out default (Johnson and Goldstein 2003). Of course, a number of cultural factors and sociological norms can mediate the impact of default options, posing important questions about the external validity of such experiments. But there is now little question that defaults matter for policy.

However there are still few published examples of scientifically rigorous studies which examine the role of default options in environmental policy. This is in spite of the fact that there are numerous instances of default options for a variety of environmentally related choice tasks, such as opt-in/opt-out defaults for electronic/paperless bank statements, default options for carbon offsets associated with air travel, as well as an array of additional examples in the context of consumer energy and water use.

To highlight the relative ease and clarity of utilizing experimental methods to evaluate default options in the context of energy use, the project team conducted a simple experiment among employees at the OECD to see how default thermostat setting affected occupants' chosen temperature settings over a 6 week period. Analysis of data from the experiment indicates that building managers would achieve lower energy use with no noticeable decrease in occupants' comfort by decreasing the winter default settings on office thermostats from 20°C to 19°C. However, the data indicate that decreasing the default setting by more than 2° would cause occupants to actively intervene and increase their temperature setting to what prevailed with the 20° default, thereby yielding no decrease in energy use.

Description of the experiment

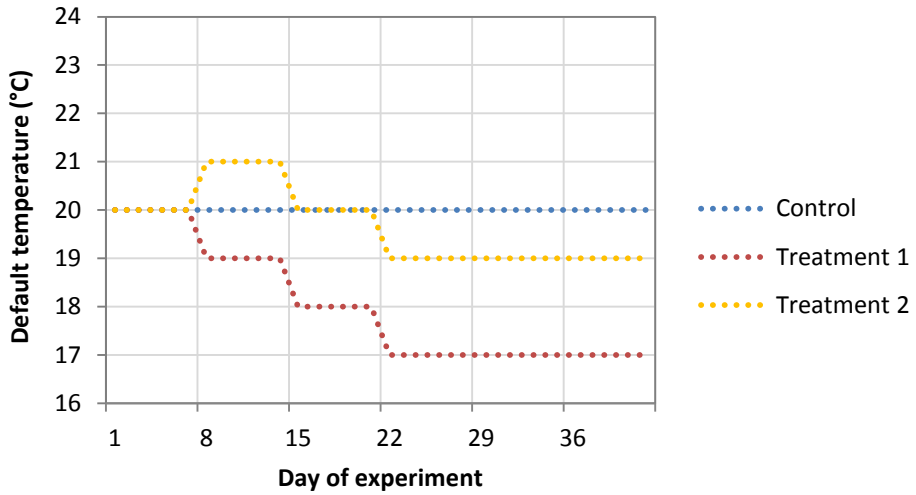
The basic research question for the experiment was: How much does changing the default setting on office thermostats affect the chosen thermostat settings in offices? Each office in the experiment included a thermostat that was accessible to the occupants and for which the default setting of 20°C prior to the experiment. This meant that, unless occupants actively changed their thermostat settings, the heating, ventilation, and air condition (HVAC) system would attempt to drive the office temperature to 20°C.

A randomized controlled experimental design was used in the experiment as follows: Ninety-three offices were selected for inclusion in the experiment, corresponding to all occupied offices on the first floor (above ground floor) of the OECD's Marshall Building. To collect baseline data, thermostat settings, measured temperature, and office occupancy (whether or not someone was present) were recorded for a 1 week period, during OECD's working hours. Thermostat settings and temperature were recorded hourly, and occupancy was recorded twice daily, once in the morning and again in the afternoon. While baseline data were being collected, two distinct, three-week schedules were formulated for changes to thermostat

defaults. These two schedules (shown in Figure 1) comprised the two "treatment" arms of the experiment. The first treatment simply lowered the default setting by 1°C per week over a three week period, ending at 17°C. The second treatment first increased the default by 1°C (to 21°C) in the first week of the treatment, before lowering the setting by 1°C per week for two subsequent weeks, ending at 19°C. A control group was also specified, in which the default setting remained at 20°C throughout the experiment. Offices were then randomly assigned to one of the two treatment arms or to the control, the default schedules were implemented by building management, and data on thermostat settings, temperature, and occupancy were collected throughout the three treatment weeks and for two subsequent weeks.

It is worth mentioning, for those not familiar with experimental methods, what the advantages are from this study design, in particular the inclusion of a control group and the random assignment of units to treatments or control. The inclusion of a control group in conjunction with data collection before, during, and after the treatments allows us to control for choice differences over time that may have nothing do with defaults, such as changes in the weather: If for example the decrease in default setting coincided with a fall in outdoor temperatures (which is exactly what happened in the experiment), then without a control group it would be impossible to know whether differences in choices before and after treatment were due to changes in the default setting or due to changes in the weather. Additionally, by randomly assigning offices to either treatment or control, we can be sure that differences in average observed temperature settings between the two groups is due directly to the treatments and not due to selection bias. For example, if we allowed occupants to choose which group to join, we might expect those who joined the group with the lowest default setting to *a priori* prefer lower temperatures in their offices. Such selection would lead to an overestimation of the effect of defaults.

Figure 1. Scheduled changes in defaults for treatment and control groups

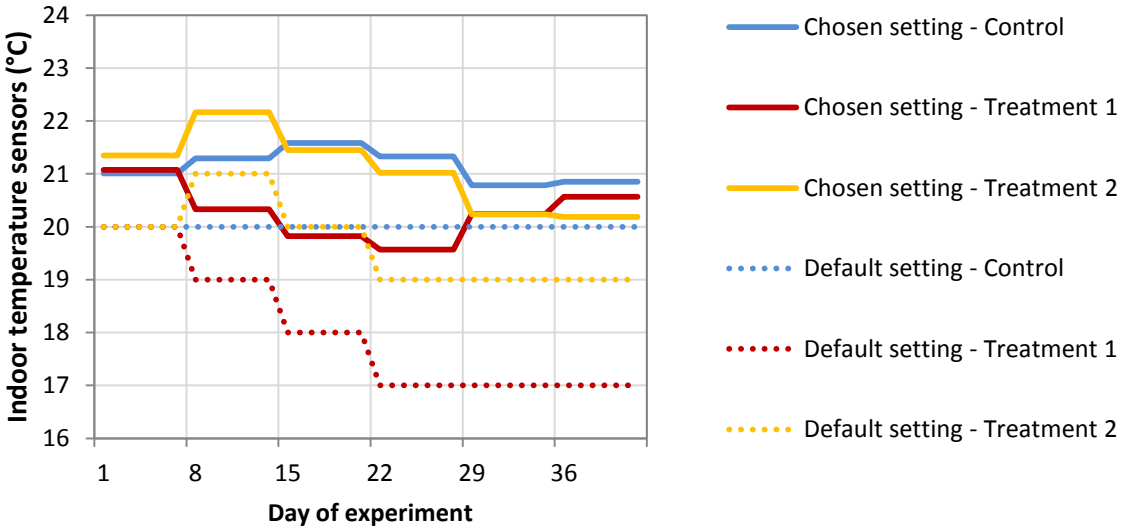


Results

The primary outcomes of interest are the temperature settings chosen by office occupants (either actively or passively). Figure 2 plots these data over time for each of the treatment and control groups and among only those offices which were actually occupied. The scheduled changes in default settings are also shown for reference. There are three important points contained in this figure, two relating to the validity of the experiment and the last comprising our main result: First, those in the control group increased their thermostat setting greatly during February; this was likely a response to the unusually cold weather during that period (see Figure 3) and certainly was not related to the default setting, since that remained at a

constant 20°C throughout. Second, all treatment and control groups exhibited the same average settings during baseline data collection, suggesting that the random assignment of offices to each of the groups worked, and that there was no systematic difference between the groups *a priori*. Most importantly, Figure 2 shows that both Treatment 1 and Treatment 2 average settings paralleled the scheduled changes in default settings (Figure 1) for the majority of the experiment, but not for the last two weeks. This suggests that most of the default changes were not negated by individual responses. Only in the last stage of Treatment 2, where the default setting was decreased to 17°, did individuals seem to finally respond to the external changes by adjusting their settings higher. Perhaps most interesting is that the Treatment 2 group—which ended the experiment with a default 2° higher than Treatment 1—exhibited an average setting that was 0.5°C *lower* than Treatment 1. This suggests that the exceedingly low default setting for Treatment 1 activated a conscious response from individuals to warm up their offices.

Figure 2. Changes in thermostat settings over time for treatment and control groups.



While Figure 2 provides a suggestive picture of how default settings affected choices, scientifically valid assessment of impacts require an estimate of statistical significance to rule out that the pattern shown above occurred by chance. Estimating statistical significance requires not only calculating the mean thermostat settings for each treatment group in each week (as shown above), but also calculating the standard errors of the means, which are based on the amount of variation in the observed choices. If there is a lot of random variation in chosen thermostat settings, then the more difficult it becomes to verify that an observed pattern is statistically significant.

The standard measure of impact in social science experiments is the average treatment effect (ATE), which, given our randomized controlled experimental design, consists of comparing the weekly mean thermostat settings between treatment and control groups. Formally, the ATE for a given treatment in a given week is the difference in mean temperature settings between the treatment and control group for that week, subtracting the same difference for the baseline week. The estimated ATE_{wt} for week w and treatment t is:

$$ATE_{wt} = (S_{wt} - S_{wC}) - (S_{0t} - S_{0C})$$

where S_{wt} is the average temperature setting for week w and treatment t (with $t = C$ denoting the control). This formulation of the treatment effect in this manner controls for preexisting differences between the treatment and control groups that were present prior to the intervention: If the difference in baseline choices between treatment and control is close to zero (as is the case in the present experiment, see Figure 2), then ATE_{wt} is approximately equal to $(S_{wt} - S_{wC})$, which is just the difference in the average chosen thermostat settings for week w between treatment and control. These estimates, their standard errors and their statistical significance are show in Table 1, along with the experimentally controlled default settings.

As can be seen from the table, the decreasing default setting in Treatment 1 caused a statistically significant reduction in the chosen thermostat setting for weeks 2 through 4 of the experiment, but this effect dissipated in weeks 5 and 6. Meanwhile, the chosen settings of those exposed to Treatment 2 became significantly lower than those in the control group towards the end of the experiment, from Week 4 onward.

Table 1: Treatment effect on chosen temperature settings, by treatment arm and week

		Week of experiment				
		2	*3*	*4*	*5*	*6*
Treatment 1	Default setting (°C)	19	18	17	17	17
	Treatment effect (°C)	-1.125***	-1.883***	-1.966***	-0.515	-0.408
	Standard error	(0.371)	(0.454)	(0.464)	(0.451)	(0.498)
Treatment 2	Default setting (°C)	21	20	19	19	19
	Treatment effect (°C)	0.381	-0.569*	-0.770**	-1.001***	-1.030**
	Standard error	(0.294)	(0.338)	(0.318)	(0.343)	(0.421)

*See text for formal definition of the average treatment effect. The asterisks denote statistical significance of the treatment, with ***,**, and * denoting 1%, 5%, and 10% statistical significance, respectively. Robust standard errors estimates are calculated via clustering by each of the 87 offices for which a full set of measurements were obtained.*

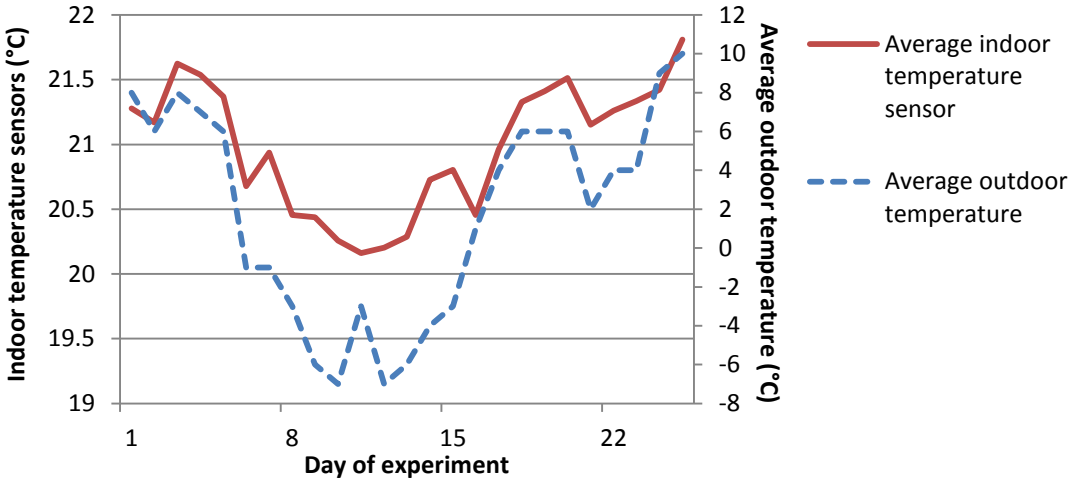
Implications

The key policy message from this small experiment is limited in scope, but very easy to explain: *Small reductions in the defaults of office thermostats can lead to lower temperature settings by occupants in the winter heating season, which when scaled up to the whole building translates into lower energy use. However, if the reduction in default temperature is too large, then respondents respond very actively, increase their temperature settings, and behave as if the change in default had never occurred. In quantitative terms, our results indicate that a reduction of the default temperature from 20°C to 19°C would decrease energy use, but a reduction to 17°C would have no effect.*

Of course, explaining *why* small changes in defaults can have a bigger impact than large changes is important for a broader understanding of how defaults can affect policy. There are a number of competing hypotheses which can explain our results here, but testing these hypotheses would require additional experimentation. The first hypothesis goes as follows: Occupants did not perceive small changes in their office temperature, but did perceive large changes, at which point they acted to improve their comfort by increasing their thermostat setting. Alternatively, small (and large) changes in office temperature were perceived, but acting to change the thermostat setting required cognitive effort, the "cost" of which did not outweigh occupants' perceived gains from action. Lastly, changes in defaults may have reshaped occupants' temperature preferences and literally expanded their comfort zones, but when the default

extended too far beyond what occupants were familiar with, then this comfort zone could no longer be expanded.

Figure 3. Outdoor and indoor temperature trends during the experiment



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