Pro-Environmental Behaviour Change for Nature: Empirical and Theoretical Evidence from a Field Experiment in Aotearoa New Zealand

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Abstract

Individual behaviour change is a crucial component of our response to current environmental challenges and over recent years, a growing body of literature has focussed on the drivers and levers of pro-environmental behaviours. However, scholars have noted there is a considerable shortage of behavioural science research that focuses on behaviours that directly impact nature and biodiversity. This is concerning, given the enormous value populations place on nature, the fundamental role nature plays in society and because nature is declining rapidly.

In this thesis, we focus on understanding volunteering for nature restoration groups, which we show is an under-researched behaviour in the literature. It also has relatively high potential to deliver positive impacts for biodiversity and nature. We start by developing a simple generalisable theoretical model that suggests three main factors may be inhibiting the uptake of volunteering for nature – uncertainty, inaccuracy and high behavioural adjustment costs. We use this model to inform the design and hypotheses for a large field experiment in Aotearoa New Zealand where we aim to answer the following questions:

How can we increase volunteering for nature restoration groups? What are the effects of volunteering for the first-time on future volunteering behaviour? How does volunteering affect other important outcomes of interest, like environmental identity, locus of control beliefs and wellbeing?

Our field experiment has two stages. In stage one, we randomly assign first-time volunteers (those who are not already engaged in nature volunteering) to treatment groups to assess the impact of a nudge, a supermarket voucher incentive and a nudge and incentive combined on volunteering behaviour. We find that a $50 NZD supermarket incentive increases attendance rates at volunteering events and commitment rates to attend volunteering events. On the other hand, an environmentally and socially motivated nudge in isolation has no effect on volunteering behaviour. However, combining the nudge with the voucher incentive enhances the efficacy of either treatment alone, demonstrating that significant positive synergies exist between nudges and incentives in this context.

In stage two, we show volunteering for the first-time is plausibly randomly assigned, conditional on availability and being offered an incentive. We use this feature to estimate the causal impact of volunteering for the first time on future volunteering behaviour and other outcomes of interest. We find that volunteering for the first time crowds in future volunteering behaviour, generates positive spillovers to other pro-environmental behaviours and strengthens environmental self-identity and locus of control beliefs, which are important pre-cursors to pro-environmental behaviour. Our results show two mechanisms are likely driving these effects. Firstly, volunteering for the first-time provides important information about the benefits of volunteering that are used in future decision-making. Secondly, it strengthens environmental attitudes and identity, which in-turn affect preferences for pro-environmental behaviour. Taken together, our results show that using a financial incentive to help people experiment with volunteering can lead to large positive spillovers and crowding-in effects for future pro-environmental behaviour.

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Chapter 1: Introduction and Background

1.1 Introduction

The world is crossing or set to cross multiple planetary boundaries as we continue to push the limits of nature and the natural environment (Rockström et al., 2009; Steffen et al., 2015). Shifting individual behaviour is a critical tool for preventing further transgressions and for addressing environmental issues like climate change and biodiversity decline. Indeed, there has been significant work investigating the drivers and levers of pro-environmental behaviours (PEBs) (for recent examples, see Bonan et al., 2021; Carlsson et al., 2021; Zemo & Termansen, 2022). Unsurprisingly, in response to the growing urgency around climate change, we have seen a proliferation in behavioural science research applied to the climate domain (Carrico, 2021; Kulin & Johansson Sevä, 2021; Sloggy et al., 2021; Zemo & Termansen, 2022).

However, as other authors note, there has been substantially less research on behaviour change efforts to positively impact nature and biodiversity (Nielsen et al., 2021; Rare and The Behavioural Insights Team, 2019). Moreover, behavioural studies with links to biodiversity outcomes tend to focus on easily accessible and measurable behaviours, which often do not deliver substantial impacts on actual outcomes (Nielsen et al., 2021). These gaps exists despite the fundamental role that nature plays across all aspects of society and the enormous value populations place on nature and biodiversity (Ballet et al., 2018; Costanza et al., 2017; Kreye et al., 2018).

One behavior that directly contributes to environmental restoration is volunteering for nature restoration community groups (Ganzevoort & van den Born, 2020; Ryan et al., 2001; United Nations Volunteers (UNV) programme, 2021). Volunteering is a public good and research shows that it enhances welfare for society through protecting nature, strengthening social cohesion, increasing wellbeing and educating people (see for example, Meier & Stutzer, 2008). However, the behaviour of volunteering for nature restoration efforts is relatively uncommon and is often unknown to the general population (Ministry for the Environment (MFE), 2021).[1] It is also an under-studied behaviour, relative to other pro-environmental behaviours that are easier to measure (like water consumption and recycling - Abbott et al., 2013; Brent et al., 2017; Nielsen et al., 2021).

In recent calls to action, Nielsen et al. (2021) sets out the need for more research on behaviour change to positively affect biodiversity and wider environmental outcomes. The authors urgently call for more research on a) understanding which behaviours matter the most for outcomes in nature, b) understanding

[1] Members of our wider research team have also found that lack of information is a major barrier to nature restoration volunteering.

which interventions are most effective at shifting priority behaviours and c) how intervention effectiveness varies for different actors (Nielsen et al., 2021). A recent global report echoed the call for more research on behavioural interventions to shift conservation behaviours (Rare and The Behavioural Insights Team, 2019). And across the wider behaviour change literature, there is an increasing focus on selecting behaviours based on end outcomes, rather than what is easiest to measure and will deliver the largest effect size (Al-Ubaydli, List, LoRe, et al., 2017; Grilli & Curtis, 2021). As Al-Ubaydli et al. (2017) aptly puts it:

"Doctors want patients to get better, not to take pills."

Likewise, as researchers, we should ultimately be focused on improving environmental outcomes through behaviour change, rather than being vehemently focused on the behaviour change itself (Al-Ubaydli, List, LoRe, et al., 2017; Nielsen et al., 2021).

In this thesis, we address these recent calls to action and gaps in the literature through our research on volunteering for nature restoration groups in Aotearoa New Zealand.

In the remainder of this chapter, we summarise some of the pertinent areas of the literature and provide relevant background on volunteering for nature restoration groups. We start by outlining the aims, structure and contributions of this thesis. We then discuss the decision to focus on nature restoration volunteering as our target pro-environmental behaviour (PEB) and then briefly review the literature on field experiments and pro-environmental behaviour (PEB) research. We then review the literature on volunteering, documenting a comprehensive range of benefits one could obtain from volunteering (which is relevant for our theoretical model in Chapter 2). Finally, we conclude with a brief discussion of the environmental context in Aoteaora New Zealand.

1.2 Thesis overview

1.2.1 Aim

The overall aim for this thesis (and the research contained within it) is to further the understanding of pro-environmental behaviour change to positively impact biodiversity and wider environmental outcomes. We focus on volunteering for nature restoration groups, which is a behaviour we specifically target for its high potential impact on actual environmental outcomes (Ryan et al., 2001). While the empirical research is set in Aotearoa New Zealand, we aim to generate insights that are transferrable to other behaviours and contexts.

1.2.2 Short summary

A substantial component of this thesis is reporting on the design, results and insights from a two-stage field experiment on volunteering for nature restoration groups in Kirikiriroa Hamilton, New Zealand. In stage one, we evaluate the effects of three interventions on increasing volunteering behaviour amongst “first-time” volunteers (individuals who are not currently engaged in nature volunteering). In stage two, we identify the causal impacts of volunteering for nature (for the first time) on future volunteering behaviour and other outcomes of interest (pro-environmental attitudes, wellbeing, environmental identity and wider pro-environmental behaviours).

There is also a theoretical component to this thesis, which helps inform the experimental design and hypotheses. We develop a simple generalisable model of behaviour change which postulates that three main factors (uncertainty, inaccuracy and high adjustment costs) are hindering the uptake of socially desirable behaviours that are also welfare-enhancing for the individual. We use this theoretical model to help design and ground our field experiment, and then use the results from our experiment to validate the theoretical model.

1.2.3 Structure

The structure of the thesis is as follows:

In Chapter 1 (current chapter), we set out the aims, motivation and background for the rest of the thesis.

In Chapter 2, we present our simple theoretical model of individual behaviour and show how uncertainty, inaccuracy and high adjustment costs may hinder the uptake of desirable behaviours. The model is based on concepts from various literatures, so a substantial section of this chapter is devoted to reviewing relevant literature and linking that to our novel theoretical model. The theory is used to make predictions and form hypotheses for the field experiment.

In Chapter 3, we present the full experimental design for stages one and two of our field experiment. This includes details about participant recruitment, treatment design and the volunteering events we hold in partnership with the Fairfield Project (our field partners).

In Chapter 4, we document the hypotheses, methods, data and results for stage one of the field experiment.

In Chapter 5, we report on the hypotheses, methods, data and results for stage two of the field experiment.

In Chapter 6, we briefly summarise and conclude the thesis, rounding out with a short discussion of the key contributions and insights for future research and policymaking.

1.2.4 Thesis contributions

As we alluded to at the start of this chapter, this thesis makes several important contributions to different areas of literature. This thesis makes four key contributions.

Firstly, we present a novel field experiment designed to increase volunteering for nature restoration groups in Aotearoa New Zealand. Both the location and behaviour are novel contexts in the literature.

Moreover, we ensure the design is grounded in theory and addresses other key recommendations in the recent literature on field experiments (Brent et al., 2017; Harrison, 2013). Moreover, we focus only on individuals not engaging in the pro-social behaviour at all (shifting volunteering behaviour at the extensive margin). Most studies focus on the intensive margin (for example, more recycling or less energy usage).

Secondly, we synthesise several strands of the literature to produce a simple novel theoretical model explaining why some people may not volunteer for nature and engage more widely in other behaviours that are socially and privately optimal. We use this theoretical model to design our field experiment and formulate hypotheses and we then find support for the model in our empirical results.

Thirdly, in stage one of the field experiment, we evaluate the effects of different interventions on encouraging first-time volunteering. This includes testing the effects of a voucher incentive on volunteering behaviour, which adds to the literature on using financial incentives to encourage pro-social and pro-environmental behaviour and the literature on spillover effects (Gneezy et al., 2011; Ling & Xu, 2021). We also add to the literature on the efficacy of nudging for shifting pro-environmental behaviour and test whether there are synergies between nudges and financial incentives.

Finally, in stage two of the field experiment, we estimate the causal effects of a first-time experience volunteering on future behaviour. Psychological wisdom says "past behaviour predicts future behaviour", but generally there is little causal evidence because it is hard to randomise or effectively randomise past behaviour. We also examine the causal effects of volunteering on several other important outcomes where evidence (and particularly, causal evidence) is limited. For example, the effects of volunteering on locus of control beliefs. We also assess whether volunteering for the first-time generates spillover effects to other pro-environmental behaviour. We do this in-part by using a semi-incentivised measure of donation behaviour, which is, as far as we can tell, a relatively novel approach to measuring donation behaviour and is a good middle ground between fully-incentivised measures and self-reported measures (which often suffer from significant biases).

1.3 Behaviour selection: Volunteering for nature restoration groups

At the start of this chapter, we documented a clear call to action from the literature for more research on behaviour change for biodiversity and nature. We also showed that volunteering for restoration groups is an under-studied behaviour in the literature, relative to behaviours that are commonly studied in the field (Brent et al., 2017). However, being under-studied is not the only reason we focus on volunteering in this thesis. Unlike many experimental studies, we chose volunteering for nature restoration groups through an explicit behaviour selection process designed to prioritise behaviours that would deliver the greatest environmental impact. This addresses a key criticism of past experimental studies in the literature (Al-Ubaydli, List, LoRe, et al., 2017; Grilli & Curtis, 2021; Nielsen et al., 2021).

The selection process was carried our by fellow researchers in the New Zealand's Biological Heritage National Science Challenge | Ngā Koiora Tuku Iho Strategic Objective 2 (SO2) research team.[2]

In consultation with environmental scientists, the wider SO2 team created a comprehensive list of individual pro-environmental behaviours (PEBs) that could positively impact freshwater biodiversity outcomes. Examples of behaviours include planting trees, installing copper-free pipes, picking up litter, reporting pollution and installing a rainwater tank. Researchers in the wider SO2 team then utilised the well-established Capability, Opportunity, Motivation and Behaviour (COM-B) model to identify target PEBs for behaviour change efforts (Michie et al., 2011).

The COM-B modelling involved scoring PEBs on three criteria:

- How impactful they are, in terms of freshwater biodiversity outcomes
- How many people are already doing the behaviour (if there is already wide uptake, there is less reason for intervention)
- How many people would be willing to do the behaviour (if very few are willing, interventions are likely to be relatively ineffective).

The SO2 team conducted a survey of New Zealand environmental scientists and ecologists to evaluate the first criterion (impact on environmental outcomes). They measured the second two criteria (current levels of uptake and willingness to uptake) through a large-scale nationally representative survey of residents in Aotearoa New Zealand. Researchers then calculated a weighted score based on all three criteria for each PEB. This method revealed that volunteering for a restoration group was one of the most impactful PEBs and a key target for further research (Figure 1). This is why we chose to focus on volunteering for nature restoration groups in our field experiment and this explicit selection process focusing on end outcomes stands in contrast with many experimental studies in the literature (Al-Ubaydli, List, LoRe, et al., 2017; Grilli & Curtis, 2021).

[2] The SO2 team, which this thesis sits within, focus on *"Empowering New Zealanders to demand and enact environmental stewardship and kaitiakitanga (guardianship)"* (Biological Heritage, 2022). Our wider SO2 research group is focussing on environmental stewardship and kaitiakitanga in the context of freshwater biodiversity in urban areas, which was identified as a significantly under-researched area in the literature. The work identifying gaps in the environmental stewardship literature is currently undergoing peer review (Mcleod et al., *forthcoming at PLOS ONE*).

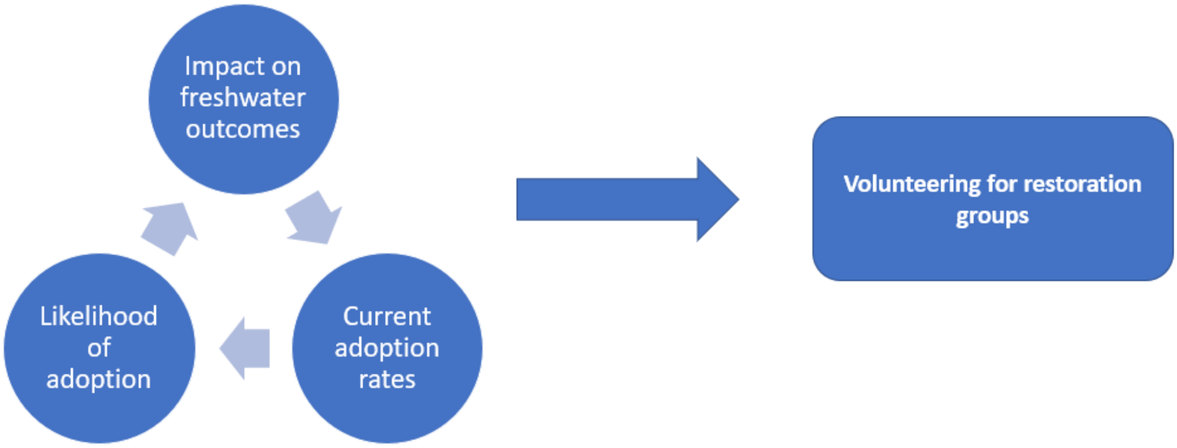

Figure 1. Selection process for target behaviour (volunteering for nature restoration groups)

1.4 Pro-environmental behaviour (PEB) change literature review

In this short section, we briefly review the literature on PEB change, covering common types of interventions and the topic of environmental identity, which is growing in importance and popularity within the field.

1.4.1 Common types of interventions

It is useful to consider the types of experiments and interventions that focus on PEBs to help inform the design of our field experiment interventions. Grilli & Curtis (2021) present a nice review of PEB intervention studies across a range of contexts. They categorise interventions into five categories, in line with Wallen & Daut (2018) who studied behaviour change relating to biodiversity conservation (which is particularly relevant to this thesis):

1) Education and awareness (EAA) = Information materials (newsletters, ads etc.).
2) Outreach and relationship building (ORB) = Focus on providing services and building relationships with community. Often, workshops, focus groups or public events. More costly but more effective.
3) Social Influence (SI) = Communicates others performance to influence behaviour. Could also be around commitment devices (i.e. publicly commit to an action).
4) Nudges and behavioural insights (NBI) = Changing choice architecture. Asserts that EAA are not NBI, but are often confused as such.
5) Incentives = Material (monetary or non-monetary) compensation/punishment for a behaviour.

Through Grilli & Curtis' (2021) review, it was clear that most behavioural intervention studies focus on energy consumption, water consumption and waste - a finding consistent across the literature (Brent et al., 2017). One obvious reason for this is that consumption and waste production are easily observable behaviours, so have been seen favourably by those conducting field experiments. Only one study in

Grilli & Curtis (2021) looked at nature conservation and there were very limited field experiments in New Zealand. Recent work by our wider SO2 team also showed that studies of environmental stewardship rarely considered urban populations and freshwater biodiversity conservation.

1.4.2 Environmental identity and PEBs

Environmental identity is widely recognised as a major driver of PEBs (and different aspects of identity have been strongly linked behaviour generally). Economists, psychologists and sociologists have long argued the importance of identity for behaviour and decision making (Akerlof & Kranton, 2000; Callero, 1985; T. J. Jr. Davis, 1995; Sen, 1985). McCloskey (1999, p. 52) famously wrote; "It's identity, stupid. Not cost and benefit". Indeed, interventions targeting environmental identity and intrinsic motivation are more likely to be effective and lead to positive spillovers in PEBs (Truelove et al., 2014). Truelove et al. (2014) suggests that consistency and identity effects are the driving forces behind positive pro-environmental spillovers. Consistency effects arise because individuals act in way as to remain consistent with their own self-identity and social identity. Bénabou & Tirole (2011) suggest that individuals invest in their self and social identities by engaging in actions that signal morality to others (social signalling) or themselves (self-signalling). As Truelove et al. (2014) discusses, if performing one behaviour increases an individuals' association with a group or identity, they are more likely to engage in behaviours prescribed by social norms for that group or identity in the future.

Silvi & Padilla (2021) present a model of pro-environmental behaviour that depends on social norms, intrinsic motivation and external conditions. They argue that intrinsic motivation is the driving force (intrinsic motivation is inherently linked to concepts of identity). These authors discuss some of the popular behavioural models from psychology and sociology, including the Norm Activation Model (Schwartz, 1977). This model describes how a personal norm becomes activated in behaviour. Extensive literature shows the theoretical and empirical superiority of the Norm Activation Model in relation to other psycho-social models of behaviour (see Silvi & Padilla, 2021). The authors also discuss the Attitude Behaviour Context (ABC) model (Guagnano et al., 1995). The ABC model postulates that behaviour uptake depends on individuals' attitudes towards the pro-environmental behaviour and external conditions.

Crompton & Kasser (2009) also argue that identity campaigning (targeting identity) is an important approach for improving PEBs. They focus on addressing aspects of human identity that contribute to proclivities towards environmentally unfriendly behaviour. They argue that holding materialistic values and defining non-human nature as an "out-group" leads to a lower connectedness to nature. And, as many studies show, there are strong positive associations between connectedness to nature and environmental identity (Balundė et al., 2019; Rosa & Collado, 2019). The connectedness to nature argument is particularly concerning for urban populations, where researchers argue connection to nature has been eroded (Rosa & Collado, 2019; Soga & Gaston, 2016).

Overall, there is ample support for the notion that people with stronger environmental identity, environmental self-identity, connectedness to nature and environmental attitudes engage in more PEBs (Balundė et al., 2019; Crompton & Kasser, 2009; Mayer & Frantz, 2004; A. C. Sparks et al., 2021; Whitmarsh & O'Neill, 2010). However, most studies are correlational (or the direction of causation is unclear) and there is little empirical work on how environmental identity is established and strengthened. Such causal inference research is well suited to the field of economics, yet there are very few papers in environmental economics that consider environmental identity in any capacity (some exceptions include recent papers by Bonan et al., 2021; Gleue et al., 2022; Panzone et al., 2021; Zemo & Termansen, 2022).

We will add to this literature in stage two of our field experiment by assessing the causal impacts of volunteering on environmental self-identity, connectedness to nature and general environmental attitudes.

1.5 Volunteering background

In this section, we review the literature on volunteering (with an emphasis on nature restoration volunteering). Broadly speaking, volunteering is *"any activity in which time is given freely to benefit another person, group or cause"* (Wilson, 2000). In our context, volunteering for nature restoration groups involves giving time to nature restoration groups who are actively engaged in nature restoration (for example, preserving a national park or protecting a local waterway).

Volunteers and volunteering play important roles in society, providing services that markets do not, helping others during difficult times, strengthening social cohesion and enabling different types of economic activity (United Nations Volunteers programme, 2021; Wilson, 2000). Volunteering contributes significantly to public goods, like environmental restoration, the provision of sport, strengthening civic pride and creating a more connected and fair society (United Nations Volunteers (UNV) programme, 2021).

Who volunteers? Past research suggests that volunteers, including nature restoration volunteers, tend to be older, well-educated and are more likely to be female (Ganzevoort & van den Born, 2020; Volunteering New Zealand, 2023). Fujiwara et al. (2018) also find that rural residents are significantly more likely to volunteer than urban residents, a finding echoed by work in New Zealand and elsewhere (Davies et al., 2018; Ministry for the Environment, 2021; Paarlberg et al., 2021)**.** Our initial conversations with local nature restoration groups revealed similar results – current volunteers tend to be older (or retired) and it has been anecdotally difficult to encourage new groups of people to start volunteering.

Volunteering for nature restoration is one area that fewer people tend to be involved in (Volunteering New Zealand, 2023) but is pivotal to our societal efforts against environmental degradation, biodiversity loss and climate change. As Ganzevoort & van den Born (2020) argue, under our current system, the protection of nature is inextricably linked to the efforts of volunteers. Ryan et al. (2001) also contend that many of the improvements in environmental quality in the past would not have happened without volunteers, and these volunteers are needed now more than ever.

On top of the wider societal benefits, volunteering imparts several other substantial benefits on the volunteers themselves and this is a widely recognised relationship (Wilson, 2000). From an individual perspective, we review the reasons why one might choose to volunteer more generally. We focus on general volunteering because there is a wider literature in this space to draw upon and the behavioural literature on nature conservation volunteering is still relatively small (Nielsen et al., 2021). It is useful to document these private benefits because it provides context to the theoretical model we will introduce in Chapter 2 (as our model depends on the expected private net benefits from volunteering).

1.5.1 Private benefits of volunteering overview

There are a range of studies that investigate the private benefits of volunteering, which include wellbeing benefits, gaining altruism utility, social capital benefits and human capital benefits (for examples, see Fujiwara et al., 2018; Van Willigen, 2000; Yeung et al., 2017). Below, we briefly review the literature around these private benefits and conclude with a short discussion of some of the wider societal benefits of volunteering.

1.5.2 Wellbeing benefits

Many studies have reported that volunteers have higher wellbeing, better mental health, greater self-worth and are generally happier than those not volunteering (a good recent example is Fujiwara et al., 2018). However, most of these studies are correlational, given the difficulty of randomising people into volunteering conditions.

Over recent years, more effort has been devoted to estimating the causal impact of volunteering on wellbeing and happiness outcomes. Binder & Freytag (2013) deploy matching (based on propensity score) estimators on the British Household Panel Survey (BHPS) to estimate the impact of volunteering on subjective wellbeing. They find that regular volunteers have greater subjective wellbeing and that these effects are stronger in lower income quartiles. However, this causal interpretation relies on assumptions around the direction of causality and the absence of omitted variable bias (from unobservable covariates that are not include in the matching equation).

Meier & Stutzer (2008) use the re-unification of Germany as a natural experiment to evaluate the effects of volunteering on life satisfaction. They use data from the German Socioeconomic Panel and argue that they can treat the re-unification as an exogenous event impacting the ability to volunteer for members formally in the GDR (German Democratic Republic). After the breakdown of the GDR, a significant portion of volunteering infrastructure collapsed and people in East Germany were "*forced*" to stop volunteering. After the reunification, life satisfaction fell in East Germany. The authors use a difference-in-differences design to assess differences in the average decline in life satisfaction for those who did and didn't volunteer before and after the collapse in East Germany. They found that people who stopped volunteering had significantly lower wellbeing and interpreted this as the causal effects of volunteering on wellbeing.

There are other articles that show associations between volunteering and subjective wellbeing, happiness or life satisfaction (for example, Fujiwara et al., 2018). Other studies show strong positive associations between volunteering and physical and mental health outcomes (McDougle et al., 2014; Piliavin & Siegl, 2007; Yeung et al., 2017). However, as Dolan et al. (2021) notes, most evidence to date is correlational or the direction of causation is unclear (because it may be that happier people are more likely to volunteer, rather than volunteering increasing happiness – this concern applies to Binder & Freytag, 2013). Dolan et al. (2021) adds to the limited literature on the impacts of volunteering by estimating the causal impacts of a Covid-19 micro-volunteering program on subjective wellbeing (SWB). In March 2020, the UK's National Health Service (NHS) issued a mass call for volunteers to help shield high-risk individuals in their homes. Three quarters of a million people registered within a few days and as a result, there was significant oversubscription to the volunteering program. An app was designed for the volunteering programme which allocated tasks to volunteers at random. This allows Dolan et al. (2021) to compare SWB outcomes between highly similar individuals that signed up to the program but either volunteered or did not volunteer at random (as some miss out due to oversubscription). The authors find that volunteering increases overall life satisfaction, feelings of worthwhileness, feelings of social connectedness and feelings of belonging to the community. These effects were sizeable, in line with Binder & Freytag's (2013) estimates and persistent over the three months of data collection. Interestingly, the marginal impacts of volunteering on SWB diminished with the number of tasks carried out, pointing to an inverse U-shaped relationship between volunteering and wellbeing outcomes. This suggests that that the first task (or first few tasks) contribute the most to wellbeing improvements.

1.5.3 Altruism and warm glow utility

Sticking with the theme of the private benefits of volunteering, it is well-documented that many individuals gain utility from pure or impure altruism and that these are important determinants of prosocial behaviour. Meier (2006) surveys and reviews theories in economics around pro-social preferences and how these are incorporated into utility models. With prosocial preferences (or pure altruism), the individual receives utility from the knowledge that they are helping others (for example, see the model by Bénabou & Tirole, 2006).

On the other hand, individuals may gain utility simply from the act of volunteering itself, rather than the outcome of helping others. This is known as warm glow utility or impure altruism (Andreoni, 1990).

Warm glow and pure altruism utility are generally regarded as intrinsic motivators for pro-social behaviour, along with the intrinsic work enjoyment individuals' receive from carrying out specific tasks and interacting with others (Deci, 1975; Meier & Stutzer, 2008). There is broad consensus that these intrinsic motivations are important predictors of prosocial behaviour, including in the environmental domain (Abbott et al., 2013; Brent et al., 2017; Silvi & Padilla, 2021; Steinhorst & Klöckner, 2018). Indeed, there is a large literature on the potential dangers of crowding out intrinsic motivation for prosocial behaviours through the use of particular types of incentives (usually, monetary incentives – for reviews in the environmental domain, see Brent et al., 2017; and Truelove et al., 2014).

1.5.4 Social motivations

One of the common reasons people engage in prosocial behaviour is to maintain a certain social identity or to appear "pro-social" to their peers (Ariely et al., 2009; Bénabou & Tirole, 2006; Bowles & Polania-Reyes, 2012). People also have strong desires to adhere to general social norms (not just adhering to group-specific behaviours – see Schwartz, 1977).[3] Brent et al. (2017) discusses the difficulties of measuring pro-social preferences in the field because people may actually hold pro-social preferences or simply be behaving in a way to make it look like they hold pro-social preferences. People who only hold preferences to *appear* pro-social will not behave pro-socially without observation. For example, in a door to door charitable giving experiment, DellaVigna et al. (2012) found that social pressure was a highly significant motivator for donations (and was more important than pure altruism). In their experiment, many residents, when given the opportunity, sorted out in advance (made sure they were not home) so they did not have to open the door and respond to an in-person request and experience the social pressure to donate.

1.5.5 Human capital

Research shows people also volunteer as an investment into their human capital (Meier & Stutzer, 2008; Menchik & Weisbrod, 1987). That is to say, some people volunteer to increase their expected future payoff in the labour market, through gaining valuable volunteering experience (which sends a positive signal to employers) and developing connections and networks. Using field experiments in Belgium, Baert & Vujić (2018) find that CVs with volunteering experience are more likely to receive positive reactions from employers than identical CVs without such experience. Hackl et al. (2007) shows econometrically that volunteering produces a wage premium and supports the notion that volunteering

[3] Schwartz (1977) presents the widely used Norm Activation Model (NAM). It describes how norms become activated in behaviour. Extensive literature shows the theoretical and empirical superiority of the Norm Activation Model in relation to other psycho-social models of behaviour (Silvi & Padilla, 2021). For behaviour to occur (the norm to be "activated"), the individual must consciously acknowledge the norm (internalising the norm) and be aware that their actions may impact others welfare (awareness of consequences) and ascribe some responsibility for these acts and their consequences to themselves.

is an investment into human capital. Loosemore & Bridgeman (2017) studies a large UK construction companies corporate volunteering program and finds that individual participation varies by several factors. While altruism is one of these factors, personal satisfaction, networking opportunities, social interaction and skills development are all highly relevant too (further demonstrating the importance of other motivators beyond altruism. Rodell et al. (2016) reinforces this in their review of corporate volunteering schemes.

1.5.6 Wider benefits of volunteering

Societal benefits of volunteering

In terms of wider benefits to society, Dolan et al. (2021) argues that our current estimates of the economic value of volunteering are underestimates because they do not account for wellbeing gains, increases in social cohesion and other harder to measure outcomes arising from volunteering. Generally, estimates of value look at the types and quantities of volunteer work carried out and use market prices to calculate a total value for these volunteer hours. In New Zealand, for example, Volunteering New Zealand (2023) use national statistics to estimate that the value of volunteering to the New Zealand economy is $4 billion NZD.

However, if volunteering enhances connectedness to community (as Lee & Brudney, 2009 suggests, for example), improves mental and physical wellbeing and improves productivity, there are clear justifications from a social good perspective to support people to volunteer above and beyond the economic values typically reported. The social case for volunteering is even stronger when it comes to environmental restoration. Volunteers are, first and foremost, contributing to a significant public good (environmental quality) which is not valued in the market. Secondly, if volunteers are doing work in nature, there is the possibility for further mental and physical health benefits (see White et al., 2019) above and beyond other types of volunteering. Thirdly, spending time in nature could also strengthen environmental values and identity, which may increase support for pro-environmental policy and crowd-in future pro-environmental behaviour (Balundė et al., 2019; Rosa & Collado, 2019).

Benefits of volunteering to businesses

Increasingly, businesses are providing for employee volunteering opportunities as part of their corporate social responsibility (CSR) strategies and to foster better work environments (Cassar & Meier, 2021; Loosemore & Bridgeman, 2017). Rodell et al. (2016) reviews the literature on employee volunteering programmes and finds that the most common type of scheme is to provide employees with paid time off to volunteer. However, uptake of these volunteering days is heterogenous and depends on demographics like age (older employees are more likely to engage) and education (more educated employees are more likely to engage). These are similar to general trends in volunteering numbers (Ganzevoort & van den Born, 2020; Volunteering New Zealand, 2023).

Jones et al. (2014) shows that from a CSR perspective, employee volunteering programmes increase the attractiveness of the company to job-seekers because the expected pride of working at the company increases, the value fit appears better and the scheme is a signal that the company treats their employees well. Caligiuri et al. (2013) finds that employee volunteering programmes generate higher staff engagement when the volunteering programmes are perceived to be having a significant positive impact on the volunteering organisation or group. Likewise, Kim et al. (2010) shows that employees at firms with employee volunteering schemes have stronger employee-company identification and connectedness, which increases commitment to the firm and reduces turnover.

1.6 Case study context: Aotearoa New Zealand

Motivating factors

Biodiversity and freshwater quality are rapidly declining globally. In Aotearoa New Zealand, 2,741 native species are at risk, 799 are threatened, and innumerable habitat areas are in detrimental condition (Convention on Biological Diversity (CBD), 2021). Many modelled freshwater bodies exceed national safety standards for concentrations of bacteria (E. coli), sediment and a range of nutrients (MFE & Stats NZ, 2020). Moreover, untreated aquifers and groundwater resources consistently fail to meet New Zealand drinking water safety standards (68% of sites monitored in 2018). These levels of degradation have detrimental effects on the environment, human health, cultural values and social wellbeing.

Urban environments make up more than 15 percent of catchment areas across New Zealand and the state of freshwater bodies is often worst these environments. MFE & Stats NZ (2020) show that 94 percent of river stretches in urban catchments are not safe for swimming. Modelling for 2013-17 indicates that over 99% of urban river stretches exceed default guideline values (GDVs) for nutrients and sediment. Furthermore, 77% of lakes downstream of urban landcover are in poor or very poor ecological health. In their systematic review of freshwater conditions in Aotearoa New Zealand, MFE & Stats NZ (2020) conclude that:

"Our overall understanding of freshwater pollution is limited in some areas – especially the urban environment. The types and sources of pollution in our cities and towns are complex, and their cumulative effects are not well understood."

Evidently, there is considerably more research required to understand urban freshwater systems and improve urban freshwater outcomes. Exacerbating this is the fact that most of the New Zealand population live in urban environments (87%, according to the World Bank, 2022). Hence, the negative impacts of freshwater degradation on health, recreational and cultural values are likely to be the greatest in urban environments.

However, urban freshwater conservation is rarely a focus for community groups in Aotearoa New Zealand. In a recent survey of 240 community conservation groups, the Cawthron Institute found that only 2.5% of groups (N = 6) were “urban” groups and only one of those groups stated that waterways were a priority for them (Sinner et al., 2022). The relatively low number of urban freshwater conservation groups means urban individuals have less opportunity to collectively gather and engage in freshwater conservation. MFE (2021) show that only 3% of the population engages with community groups seeking to improve freshwater outcomes and that urban people take less action for freshwater quality than rural people do. These results aren’t because people do not think freshwater conservation is important or relevant to them. Indeed, MFE (2021) find that 85% of the New Zealand population believe improving freshwater quality is the responsibility of all New Zealanders. They also show that 69% of respondents (representative of the New Zealand population) were unaware of groups they could volunteer with to improve freshwater quality. Hence, there is considerable opportunity to enhance urban residents’ knowledge of and connections to conservation groups working to improve freshwater outcomes.

Overall, urban freshwater conservation has received relatively less attention in academic and community spheres. The paucity of attention translates into less individual action for improving urban freshwater quality. Hence, we believe there is a significant opportunity and need to increase individual urban freshwater conservation efforts, which is a focus of our research.

Volunteering in New Zealand

Using Labour Market data, Volunteering New Zealand show that approximately 21.5% of New Zealanders undertake some form of formal voluntary work and 11.8% of people undertake informal voluntary work (Volunteering New Zealand, 2023). Formal voluntary work is generally defined as volunteering coordinated by an organisation and informal volunteering is not coordinated through an organisation (Volunteering New Zealand, 2022). Similar to elsewhere, volunteers in New Zealand are more likely to be women, employed and working in a professional occupation (Volunteering New Zealand, 2023). They are also significant differences in volunteering rates by ethnicity - those with European and Māori ethnicities are more likely to volunteer than other ethnicities.

Of particular importance to this thesis, Volunteering New Zealand (2023) also show that environmental volunteering represents a small proportion of overall volunteering, but this has been rising over time. Out of those who are actively volunteering, the proportion who do some volunteering for the environment was 11.1% in 2021, an increase from 6.9% in 2016. Moreover, results from the 2021 General Social Survey show that out of all volunteering hours (formal and informal), only 3.7% went to volunteering for an environmental restoration, conservation or animal protection organisation (Stats NZ, 2021a).

More important background information on environmental volunteering in Aotearoa New Zealand comes from research done by the wider SO2 team. After identifying nature restoration volunteering as an impactful behaviour that few people are currently doing (in urban populations), SO2 researchers conducted further surveys using the COM-B methodology (Michie et al., 2011) to understand the barriers and drivers for the nature restoration volunteering. They found that the main barriers were being unaware of local groups and when activities were occurring (lack of information), not knowing others who also volunteer (a social barrier), being too busy and volunteering events occurring at inconvenient times.

Finally, through early discussions with nature restoration groups (including our field experiment partner, the Fairfield Project[4]) and other stakeholders in the nature volunteering space, we observed useful qualitative feedback on the general state of volunteering for nature restoration groups in New Zealand. Firstly, nature restoration groups are in need of more volunteers and volunteer hours. Groups are also struggling to attract new volunteers and younger volunteers. There is also a sense that many people are not aware of their local restoration groups and the opportunities that exist to volunteer (which fits with the SO2 findings and with previous research - Ministry for the Environment, 2021).

We use this pertinent background information and context to inform our field experiment theory and design for increasing nature restoration volunteering in Aotearoa New Zealand.

[4] http://www.thefairfieldproject.co.nz/

Chapter 2: Theoretical Model

In this first substantive chapter, we develop a simple novel theoretical model that may help explain why few people engage in volunteering for nature restoration groups, despite the wide range of private benefits reported in Chapter 1. This model also provides important testable predictions which we will evaluate in our field experiment.

2.1 Introduction and background

Encouraging pro-social behaviour, and in particular, pro-environmental behaviour is an increasingly important focus area for many researchers and policymakers (Adena & Harke, 2022; Bénabou & Tirole, 2006; Farrow et al., 2017; Grilli & Curtis, 2021; Kappes et al., 2018; Lange, 2022). However, the motivation and emphasis tend to be on the overall societal benefits from behaving pro-socially or pro-environmentally, rather than the private benefits to the individuals themselves. For example, we want people to conserve water so that we have more water in the future, greater system resilience and a flourishing natural environment. But conserving water also delivers private benefits too, like cost savings which increases discretionary income. Despite being well-known, these private benefits tend to be less of a focus for researchers and policymakers.[5] They are often considered a bi-product of a socially or environmentally motivated interventions (see the Jevon's Paradox and discussion on rebound effects from efficiency improvements - Alcott, 2005; Dorner, 2019).[6]

One of the reasons there may be less focus on the private benefits is that the common pro-social behaviours studied in the literature tend to have fewer private benefits (aside from gaining pure or impure altruism utility - Andreoni, 1990; Schwartz, 1977). One prominent example is the focus on donations to charities or environmental organisations (Adena & Harke, 2022; Andreoni et al., 2017; DellaVigna et al., 2012; Exley, 2016; Feine et al., 2023). There are far more papers on donations (financial and in-kind) than there are on giving time (volunteering). Yet, many studies have shown volunteering provides benefits to mental health, physical health, wellbeing, social capital and human capital for the individual (see Chapter 1). The focus on donations is largely because it is easier to measure and track donations than it is to track volunteering, where studies often resort to self-reported measures of volunteering (Binder & Freytag, 2013; Meier & Stutzer, 2008; Thoits & Hewitt, 2001). Other common behaviours in the literature include giving blood, giving money to others, voting and purchasing carbon offsets – these complete the set of pro-social behaviours considered in a recent NBER

[5] There are exceptions to this, like Steinhorst & Klöckner (2018) who look at the difference between a private financially motivated nudge and a social environmentally motivated nudge (both perform equally well over the short run).

[6] Though, private benefits are a key feature of nudges, which are based on the idea of helping make individuals better off by avoiding or mitigating against behavioural biases (Thaler & Sunstein, 2009).

paper (Andor et al., 2022). These behaviours, like charitable donations, occur over a relatively short timeframe (the actual behaviour take minutes) and have fewer private benefits than volunteering.

Volunteering for a restoration group benefits the individual, who receives enjoyment from volunteering in nature and may also see improvements in mental and physical health from being outdoors and the act of volunteering (see Chapter 1). Volunteering in nature also has positive externalities, including protecting the natural environment, improving overall societal mental and physical health (which reduces healthcare costs for society) and enhancing social cohesion.

However, few people currently volunteer for nature restoration groups (see Chapter 1). Only 3% of New Zealanders are volunteering to improve local freshwater outcomes and 69% are not even aware of these activities (Ministry for the Environment, 2021).[7] Moreover, volunteering for nature is one of the less common volunteering activities in New Zealand (see Chapter 1 and Volunteering New Zealand, 2023). Less than 4% of total volunteering hours are devoted to environmental or animal welfare groups in New Zealand (Stats NZ, 2021a). On the one hand, this could simply reflect people's true underlying preferences. However, given the extensive range of private benefits to volunteering and particularly, volunteering in nature, and the strong pro-environmental attitudes in the majority of the population (Ministry for the Environment, 2021), there is reason to believe other factors may be at play.

In this chapter, we present a simple theoretical model that focuses on the private net benefits of engaging in socially desirable behaviours (these could be pro-social or pro-environmental behaviours). We are focusing on behaviours that would benefit individuals and provide positive externalities to society.[8] We use the primary example of volunteering for a nature restoration group, as that is the focus of this thesis. Our simple theoretical model postulates that there are three main underlying barriers to volunteering for restoration groups that may also be common to a range of other socially desirable behaviours (like taking public transport). When individuals are making decisions about whether to engage in these behaviours, they may estimate the net benefits of the behaviour with a) high uncertainty, b) inaccuracy (underestimation[9]) and c) high adjustment costs. These key characteristics may mean people are under-investing their time into behaviours that are both socially and personally welfare-enhancing. This would

[7] Of course, volunteering to improve freshwater outcomes is not the only type of nature volunteering. However, as far as we are aware, there are no published data on nature volunteering in New Zealand more broadly. The statistics we present are indicative of the generally low volunteering numbers for nature restoration groups.
[8] When we talk about benefits to an individual, we are referring to a subset of individuals who will receive some enjoyment from the activity.
[9] Of course, people may also over-estimate the expected net benefits from volunteering. However, our theoretical model is focussing on a sub-group of people who are likely not engaging or trying a behaviour despite it being beneficial for them to do so. As such, under-estimation is the key concern (because if they over-estimate, they are more likely to have tried the behaviour anyway). Moreover, we provide some empirical evidence that supports a wider prevalence of under-estimation than over-estimation in our stage two experimental results.

give clear justification for policy interventions to increase the uptake of these behaviours from a social good and paternalistic perspective (like green nudges - Carlsson et al., 2021; Schubert, 2017).

In this chapter, we investigate these three characteristics further in the context of volunteering for nature restoration groups, but these barriers may also apply to other socially desirable behaviours. Our novel theoretical model explains how these three characteristics interact to produce sub-optimally low levels of uptake of a desirable behaviour and show how behaviour change interventions could work within this context. Our model builds on an extensive range of literature and makes a significant contribution by bringing these various strands of the literature together. These strands of the literature fall both within and outside of economics, including the literature on habit formation, experiential utility, the economics of information and Bayesian updating.

Our model is primarily graphical but is also formalised by a series of simple mathematical equations. We opt not to develop a complex mathematical model and focus on explaining the insights and mechanics verbally, given this is relatively new territory. Our focus is on developing a simple graphical theoretical model based on a wide set of literature that seeks to explain current behaviour, provide guidance on opportunities for positive behaviour change and make predictions about how interventions will affect the uptake of desirable behaviours. Our emphasis is on a certain class of socially desirable behaviours where the *status quo* inhibits the uptake of these behaviours, despite there being net benefits for the individual and society.

We generate insights and hypotheses from our model that we test for and examine in our field experiment documented in Chapters 3 to 6. One of the key insights from the model is that under certain conditions, we may be able to crowd-in pro-social behaviour using well-designed and targeted interventions. This includes financial incentives, which have been shown to have crowding out effects in other contexts (Frey & Jegen, 2001).

In the following section, we will review the relevant areas of the literature that significantly informed the theoretical model. We will then present an overview of our theoretical model, followed by a brief example of how a financial incentive would affect behaviour. We will finish the chapter with a simple mathematical formalisation of the model.

2.2 Supporting literature

In this section, we will discuss several strands of the literature that support our theoretical model, with particular attention to volunteering for nature restoration groups as our example behaviour. The areas of the literature we discuss lend support to one or more of the three characteristics in our model – uncertainty when estimating the benefits of volunteering, inaccuracy when estimating the benefits of volunteering and high behavioural adjustment costs.

We start by briefly reviewing the literature on information and decision-making and then consider the types of information that are available for experience goods. This leads to a discussion of Bayesian modelling, where beliefs update following sequential experiences. These sections relate mostly to the uncertainty side of our theory. We then look at the literatures on time allocation and habits, which provides insights into risk preferences and adjustment costs. Penultimately, we examine the literature on satisficing, rational inattention and experimentation, which support the overall principles of our theory. Finally, we look at some literature in health economics where researchers have discussed the under-estimation of benefits and costs, which provides useful background for the inaccuracy characteristic in our model.

2.2.1 Information economics

Information and behaviour

Unlike agents in standard neoclassical expected utility models, consumers are limited in their ability to acquire, store and process information and this generates uncertainty when choices are made. Uncertainty forms one of the key barriers in our theoretical model and is derived from imperfect foresight and incomplete information (Alchian, 1950). Indeed, the notion that consumers have limited computational power and information storage capacity forms the basis of "bounded rationality", a cornerstone principle of behavioural economics (Klaes & Sent, 2005; Simon, 1957). Behavioural economics revolves around departures from expected utility theory and has guided recent policy developments (Congiu & Moscati, 2022; Rare and The Behavioural Insights Team, 2019; Thaler & Sunstein, 2009). For example, nudges[10] have risen to popularity as a way of helping individuals avoid biases (or correct for biases) without altering actual incentives or introducing coercion. Green nudges have also become popular as a way to help address negative environmental externalities arising from individuals' decisions (Carlsson et al., 2021; Schubert, 2017). Nudges are not always welfare-enhancing for the individual being "nudged", but they are welfare enhancing from a societal point of view (Schubert, 2017).

One common set of interventions designed to change behaviour revolve around information provision (for a recent example, see Haaland et al., 2023). Many see these interventions as nudges because they do not alter the choices available or inherently change incentives or preferences. However, Grilli & Curtis (2021) argue that informational and educational interventions are a separate class of treatments altogether. Many information interventions are designed on the premise that missing information is the (or a) cause of inaction. However, in many instances, there are other barriers (often relating to the

[10] Nudges are changes in the choice architecture that do not affect the choices available to an individual, their incentives or introduce coercion (Thaler & Sunstein, 2009).

practicality of engaging in a behaviour) that remain and information treatments alone are relatively ineffective (Al-Ubaydli, List, LoRe, et al., 2017; Grilli & Curtis, 2021). Indeed, in recent models of pro-social and pro-environmental behaviour, information is just one of the factors influencing pro-environmental behaviour (Ajzen, 2011; Bénabou & Tirole, 2011; Hines et al., 1987; Silvi & Padilla, 2021). Al-Ubaydli et al. (2017) use insights from the medical literature to suggest that simple informational treatments and primes should be coupled with more complex interventions (in their context, technological innovation) to maximise the combined effectiveness of the interventions. Overall, there are mixed findings for the efficacy of information interventions and the effectiveness is ultimately highly context specific (Mertens et al., 2022).

Information and uncertainty

Information is important because it reduces the uncertainty associated with the expected outcome of a decision (Alchian, 1950; Nelson, 1970). For example, imagine a consumer who is deciding whether to purchase a computer (which we'll refer to as *the computer*). Without any further information (except the computer's make and price), the consumer will have an expected utility function with high variance. As the consumer acquires additional information about the attributes of the computer (for example, information about the processor, memory card, display and users' experiences), the variance of the expected utility function falls (and the mean could also change if the information shifts the original belief about the computer's quality). As a result, the consumer's confidence in the computer's quality increases. We illustrate this more generally in Figure 2, where information reduces the variance of the expected utility function. Our theoretical model has this feature, as the expected net benefits function narrows as information is gained and uncertainty is reduced. As we mentioned earlier, in the presence of uncertainty, the certainty-equivalent utility would be lower than the true value if a consumer is risk or loss averse (Kahneman & Tversky, 1979; Pratt, 1964).

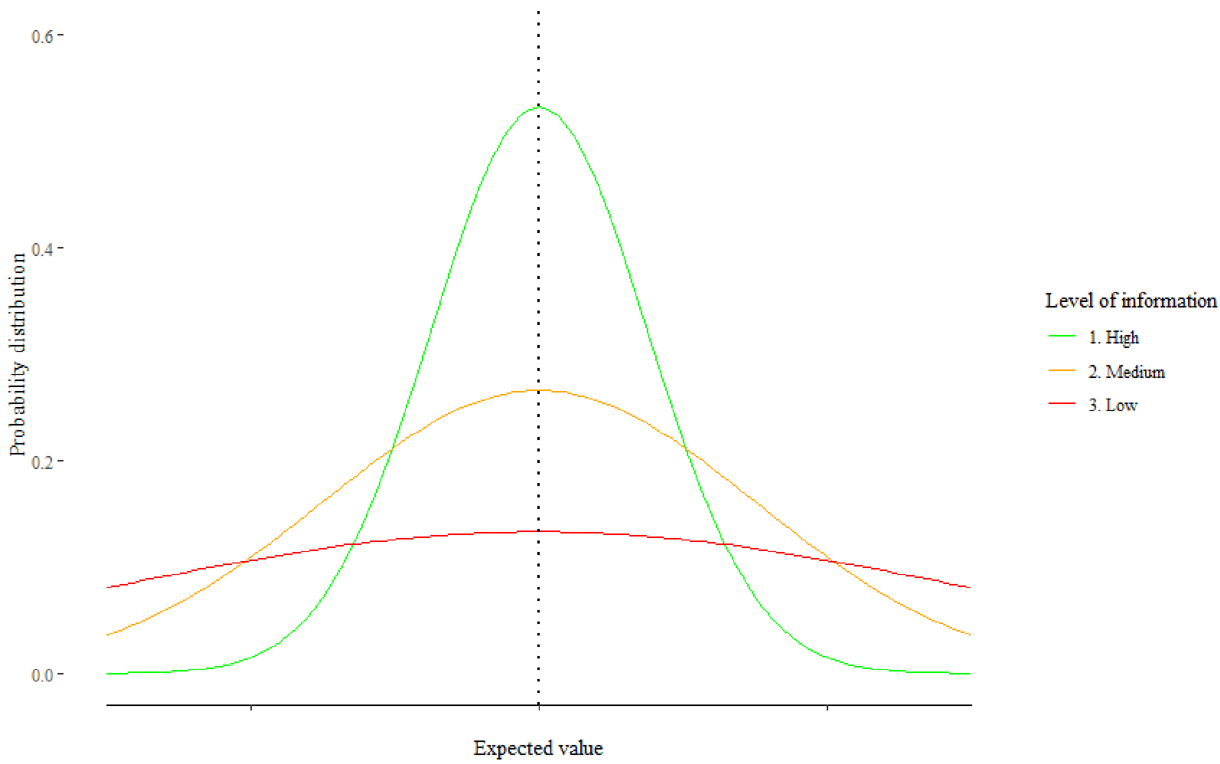

Figure 2. Schematic representation of the expected utility of consuming a good or service when information availability about that good or service is high, medium and low.

2.2.2 Experience goods

Experience goods and information availability

Given that uncertainty and inaccuracy play pivotal roles in our theoretical model and depend on information availability, it is worth considering where and what information is available for pro-social behaviours. For standard market goods that have observable physical properties, there is an abundance of information that consumers can consult (manuals, specifications, expert reviews).

For experience goods, or goods with a significant experience component, there is more uncertainty around the expected benefits from consuming the good (Nelson, 1970). This is because experiences are inherently individual-specific, and they often do not have physical attributes that can be described in a non-partisan manner. That is, there is very little objective information about experience goods in comparison to "standard goods" and much of the information needs to be revealed through experiences (Nelson, 1970; Stigler & Becker, 1977).

In the market, two avenues have emerged to address this information deficit – free trials and social learning (or reviews).

Free trials and first-hand experiential information

Free trials are commonly used for experience goods (or goods with a significant experience component) and allow individuals to test out a good before they purchase it. For example, free trials are commonplace for gyms, software companies and streaming services (Arora et al., 2017; Dey et al., 2013). Many goods have an experience component because an aspect of the product is unobservable until the product is purchased and consumed (Czajkowski et al., 2015). Most food products have a weak experience component because individuals are not certain they will like the taste of a product until they actually consume it. In that respect, taste tests (in supermarkets, for example) are a popular marketing strategy to provide information to consumers (Bawa & Shoemaker, 2004).

Trials and taste tests are a premier form of information because there is no uncertainty associated with translating information from one individual to another (which we deem, "translation uncertainty"). Indeed, recent work suggests individuals place higher value and more emphasis on experiential information they gather themselves, rather than receive or hear from others (Conlon et al., 2022; Malmendier & Nagel, 2016). In a recent NBER working paper, Conlon et al. (2022) used a series of lab experiments to show that people are less receptive to information received by others than by information gathered directly through experiences themselves. They find individuals underweight other peoples' experiences more than they should do, ruling out other explanations like distrust, difficulties in probabilistic reasoning, overconfidence and competitiveness. Others have shown lower levels of trust in experiential information from others (Haaland et al. 2023).

Social learning and second-hand experiential information

Of course, some products are not suited to the free trial or taste test paradigm. Nontheless, the advent of social media and the proliferation of technology has significantly enhanced the frequency and importance of social learning (SL). Social learning theory emphasises the importance of learning from others' behaviour and consumption decisions (Feldman et al., 2018). Nowadays, people frequent review websites or review services to learn from others' experiences and purchasing decisions. This has generated a whole new branch of literature on social learning in the context of consumer behaviour and industrial organisation (see Feldman et al., 2018). Prospective consumers can use such platforms to enhance their understanding of the experience and more accurately evaluate whether an experience is worthwhile (assuming the information is credible and credibility will be a function of the number of reviews).

So, despite the lack of objective information (because experiences are not objective), there are a range of ways in which consumers can obtain credible information about most experience goods (directly or indirectly). Indeed, experience goods have become central to the modern economy (Pine & Gilmore, 2013).

Volunteering and public experience goods

Pure public goods are goods that are non-excludable and non-rivalrous.[11] Environmental resources are widely considered to be public goods or have high public good character. For example, people can not be excluded from enjoying clean air and one person enjoying clean air has no effect on others' use of clean air. One of the pre-eminent ways of increasing the supply of public goods is through voluntary contributions. Indeed, there is a whole branch of game theory literature devoted to the understanding of voluntary contributions to public goods (Brent et al., 2017). For example, people can donate to charities or restoration groups working to enhance local biodiversity. Alternatively, people can volunteer their time to organisations and provide directly to the public good. For example, by volunteering for a planting day with a local restoration group or volunteering at the local foodbank.

As we mention in Chapter 1, volunteering (particularly, volunteering in nature) provides a substantive range of private benefits. However, those benefits are individual-specific given the importance of the "experience" volunteering – we could consider volunteering to be a *public* experience good. And, as per the previous section, the attributes of experience goods and the expected utility from consuming experience goods are initially uncertain. For volunteering, individuals do not know *ex ante* the level of warm glow utility they will receive (if any), and it is difficult to predict how much they will enjoy the act of volunteering (Andreoni, 1990). This can only be observed *ex post*. So, while a consumer may have a good idea about their level of altruism, they are likely uncertain around many of the remaining benefits of volunteering.

What about the two mechanisms (reviews and trials) that arose to reconcile the information deficit around experience goods? Well, it is not clear that either are relevant or apply to public experience goods. We are not aware of any platforms that provide reviews and credible information on volunteering experiences.[12] We are also not sure how beneficial that information would be because how do you credibly describe the *warm glow* you have experienced to another person? "Volunteering feels good". What does that mean? How would a person interpret that? How do they know the same will apply to them?

Free trials are another issue. With private experience goods, you pay for the experience with money and time. As a result, you can create a "free trial" scheme where consumers are simply not charged the product. In contrast, consumers pay with their time for public experience goods like volunteering. More generally, people use their leisure time to "pay" for activities and behaviours they want to engage in.

[11] Non-excludable means you can not prevent anyone from consuming or using the good and non-rivalrous means one persons' use of the good does not prevent others from using the good or diminish the quality for others.

[12] That is not to say these types of websites do not exist. Just that they are not widespread and we are unaware of any such websites in our geographical context.

How do we provide people with experiences volunteering at no time cost? That is a more difficult question to answer and as a result, we may not see enough people “testing” or “trialling” volunteering – this also relates to the literature on sub-optimal experimentation, which we discuss in later sections.

Some businesses are addressing this by providing corporate volunteering days where their employees can volunteer during work hours (Boštjančič et al., 2018). This is paid time off and in theory, is time they would not have otherwise had (to volunteer). However, these volunteer schemes are largely deployed in white collar businesses where people are on salaries (Afsar et al., 2018; Boštjančič et al., 2018). It is not clear whether employees’ workloads (and thus, time burden) is actually reduced by providing an optional volunteering day. If they are still expected to deliver the same outputs, then the experience is likely not coming at no time cost.

2.2.3 Bayesian updating

In our model, uncertainty and inaccuracy represent two of the three important barriers to nature restoration volunteering and other behaviours. These concepts are closely linked to the literature on Bayesian updating and Bayesian preferences (Ackerberg, 2003; Czajkowski et al., 2015), where consumers are uncertain or unaware of their “type” before having experiences consuming a particular good. For example, “am I a seafood person?” – that is a particularly difficult question to answer if you have never tried seafood before or had any direct experience with seafood.

The underlying premise of Bayesian updating is that initially, individuals are uncertain of their true preference type. As they experience consumption events, they become more and more certain about their preferences (Czajkowski et al., 2015). This literature recognises that there is always some degree of uncertainty around preferences, but that degree of uncertainty can vary substantially between goods and individuals, depending on prior consumption events. Preferences could also change after unexpected shocks, like an environmental disaster (for example, Sloggy et al., 2021). Experiences (more broadly) serve to reveal information about one’s true preferences (reduce preference uncertainty) or shift underlying preferences by affecting ones morals or personal beliefs (Davis, 2003; Sen, 1985).

Kularatne et al. (2021) showed tourists valued nature more after experiencing it, demonstrating that they were unaware of some features prior to their first experience. People also develop stronger environmental attitudes and identity through experiences in nature, which then affects preferences for pro-environmental behaviours (Rosa & Collado, 2019).

There has been an abundance of empirical work assessing this evolution of preferences and preference uncertainty over time for private consumption goods (for discussion, see Czajkowski et al., 2015). This is made possible through the observation of private consumption decisions over time, which we tend to have good market data for. For public goods, on the other hand, tracking changes in preferences following “consumption events” is more difficult because there are no formal markets for these goods.

Czajkowski et al. (2015) made notable contributions to this space by using a Bayesian model to track changes in preferences for public goods following sequential experiences with those public goods. We briefly summarise their work below.

Bayesian model of public experience goods

In line with random utility modelling (RUM) in the experience good literature (Ackerberg, 2003), Czajkowski et al. (2015) start by defining a standard Lancastarian model of utility (Lancaster, 1966). Following Ackerberg, (2003) the authors include a term δ_{ij}^{t+1} which is denoted as experience utility and represents the utility gained from factors unobservable to the decision maker at the time of the consumption decision (hence, the superscript $t + 1$, showing that the consumption decision is made first). Czajkowski et al. (2015) argue that this term could represent intrinsic feelings about the consumption of a good. In our context, this could be the intrinsic enjoyment an individual receives when volunteering in nature. In general, δ_{ij}^{t+1} could represent consumer i's true type (in our context, whether they are a person who enjoys volunteering in nature or not).

In the simplest case, δ_{ij}^{t+1} could be time invariant if the consumer learns their true type with absolute certainty after their first consumption event. However, in reality, δ_{ij}^{t+1} is usually better modelled with a time-invariant fixed effect and an idiosyncratic component. This captures the process where consumers learn about their true type or true preferences through a series of noisy signals in the real world.

Czajkowski et al. (2015) define a consumer's priors about their type and show how consumption events (experiences) will affect those beliefs. Their models shows that additional experience has an ambiguous effect on the mean posterior beliefs about one's type (as it depends on the prior beliefs about one's type and the strength of the experiences). If one's prior beliefs are underestimates of one's true type, then on average, more consumption events should increase the mean belief of one's type. However, the variance of beliefs over type fall with more consumption events (uncertainty falls with more experience). Czajkowski et al. (2015) then extend their theory to a discrete choice experiment modelling scenario and find good support for their model (particularly, the hypothesis that experiences reduce uncertainty).

There are other strands of literature that consider the effects of experiences and learning on WTP and preferences. One area shows that experiences can help people learn their preferences in relation to unfamiliar goods (for example, see Bateman et al., 2008; Brown et al., 2008). This branch studies how preferences for unfamiliar products converge with experiences to stable values that are consistent with the axioms of consumer preference (denoted as preference refinement by Brown et al., 2008).

We incorporate the insights from Czajkowski et al. (2015) into our theoretical framework, which allows individuals to reveal their "true type" (if they had inaccurate prior beliefs) and reduce uncertainty about the expected benefits through experiences engaging in the behaviour. However, in contrast to Czajkowski et al. (2015), we focus closely on behaviours or goods that have had few or no past consumption events, so likely have the greatest uncertainty.

2.2.4 Literature on spending time and spending money

How do people allocate their time? Is time really money? Probably not. There is strong evidence that decision makers do not treat time and money in the same way ((Handy & Katz, 2008; Olivola & Wang, 2016). This is an important topic, because people spend their time rather than (or as well as) their money to engage in pro-social behaviours (for example, volunteering). If individuals are risk averse with their time, then the certainty-equivalent benefits from pro-social behaviour will be markedly lower, as our theoretical model suggests. Therefore, it is important to consider the literature on how consumers treat time and money.

Spending time and risk aversion

Olivola & Wang (2016) run a series of patience auctions money and time bids. They find that people are more impatient when they bid with time, and they discount rewards exponentially (as opposed to hyperbolically with money bids).

Kemel & Paraschiv (2013) estimate risk preferences in a transportation scenario where consumers are concerned about time and monetary payoffs. They compare decisions under risk (where there are known probabilities of payoffs) and ambiguity (where the probability distributions for payoffs are unknown) and find that consumers generally prefer riskier options to ambiguous options. Ambiguity is often generated by a lack of information, and as we have detailed, limited information is a major issue for behaviours with high experience character that are not being widely carried out.

Indeed, past work by Leclerc et al. (1995) shows that people tend to be more risk-averse in the domain of time, rather than money.

In contrast to Kemel & Paraschiv (2013) and Leclerc et al. (1995), Okada et al. (2004) suggests people are more willing to spend time over money in risky circumstances. However, Okada et al. (2004) does not discuss the role of ambiguity (as in Kemel & Paraschiv (2013), does not use a real effort task and is based on a lab experiment with students, who likely have more free time than the working population.

Overall, the literature suggests that individuals may be more risk averse when making decisions with their time rather than money.

Leisure time and risk aversion

Zauberman & Lynch Jr. (2005) show that people have the common misperception that they will have more leisure time in the future and thus are more likely to delay decisions around engaging in new activities. This relates to the classic procrastination problem and the notion of time scarcity – people tend to be very busy and have limited leisure time allocation (Bellezza et al., 2017).

An interesting postulation that appears absent from the literature is that given leisure time is so scarce in modern society, individuals may be more risk averse with their leisure time. Past research shows that poorer households have greater risk aversion because there is "more at stake" - they do not want to lose the little income they have (Dohmen et al., 2011; Guiso & Paiella, 2008). Likewise, it appears reasonable to suggest that people will be more risk averse with their leisure time, given how little they have.

Charitable giving – time and money

Handy & Katz (2008) discuss the distinction between time and money in the context of charitable giving and pro-social behaviour. They argue that giving time and money clearly differ and they discuss how consumers make decisions around their time allocation (between work, leisure and volunteering). Taking these findings into account, Bauer et al. (2013) presents a summary of the private consumption decision for philanthropic (pro-social) behaviour. In their model, each individual is endowed with T units of time, which is allocated to work time t^w, leisure time t^l and charitable time (volunteering time) t^v. Such that, $T = t^w + t^l + t^v$. Utility is described as a function of private consumption C, leisure time and voluntary contributions to a public good $G(t^v, D)$ where D is monetary contributions to the public good. Hence, the consumers utility maximisation problem is:

$$\max U\left(C, t^l, G(t^v, D)\right) \tag{1}$$

$$s.t.\ C + D = wt^l + Y$$

where w is the wage rate, Y is non-market income and there are no savings.

Bauer et al. (2013) assumes that the hours of work are fully flexible, but that time allocation more follows a sequential decision process (as in Clotfelter, 1985). An individual first decides how much time they will work and then determines their allocation of remaining time between leisure and volunteering.

In contrast to the public goods model, which predicts that time and financial donations are perfect substitutes in their contributions to the public good, Bauer et al.'s (2013) philanthropy model provides no such prediction. The philanthropy model allows people to donate time and money for different reasons, recognising that there are a range of private benefits to physical volunteering that one cannot attain with financial contributions. The model also allows the level of utility to vary by type of group (environmental organisation, for example). Bauer et al. (2013) goes on to show that there are widely differing correlates between financial and time contributions to public goods across Europe.

Summary – time allocation literature

Overall, the extant literature suggests that there are fundamental differences between spending time and money. Of note, people tend to be more risk and loss averse when it comes to spending time. Moreover, experimental evidence suggests that people are more risk averse when decisions involve payoffs to charitable organisations (Exley, 2016; Kappes et al., 2018). This reinforces the concern that individuals may behave in a risk averse manner when assessing whether to participate in a pro-social behaviour with uncertain private returns and supports the intuition in our theoretical model..

2.2.5 Habits literature and adjustment costs

It is also useful to look at the habits literature because it relates closely to adjustment costs and switching costs. The habits literature provides a case where preferences are endogenous to some extent because researchers recognise that preferences are not time-seperable, as was previously assumed (for more detail, see Dunn & Singleton, 1986; Dynan, 2000). In Fuhrer's (2000) description of habit formation, utility depends on consumption relative to some reference level (lagged consumption). Fuhrer (2000) shows that consumers need to consume more and more of a good over time, and this generates the persistence in behaviour. Habits are notoriously difficult to break (Verplanken & Orbell, 2022). So, if target pro-social behaviours need to displace habits, the psychological adjustment costs will be higher (as per the discussion in our theoretical framework).

Habits and leisure time

One interesting question that is important for our theory is whether habits are stronger for decisions around allocating time?

The literature on identity argues that our identity is made up of past experiences that inform who we are (Bénabou & Tirole, 2011; J. Davis, 2003; Kahneman & Riis, 2005; Locke, 1690). For that reason, Gini (1998) argued that the way we spend our time has a considerable impact on our identity (specifically referring to our time at work). Moreover, how we allocate our leisure time also has a significant impact on our identity, but leisure time allocation decisions have received little attention in economics (Izquierdo Sanchez et al., 2016). The question remains as to whether habits are stronger in leisure time allocation than consumption decisions. Recently, researchers have found evidence for habits in leisure activities, but there is still more work required to determine whether these habits are stronger or weaker than other habits (Boto-García, 2022; Harris & Kessler, 2019; Potter, 2022).

Dynan (2000) discusses the likelihood that habits are weaker when decisions are less complex and less interwoven with other aspects of people’s lives. In the case of leisure time decisions, habits may indeed be stronger, which would mean adjustment costs or switching costs to transition to other behaviours may be larger (Burnham et al., 2003; Muellbauer, 1988). This would present an additional barrier to behaviour change and one that is well-recognised within our theoretical framework.

2.2.6 Satisficing and experimentation

In behavioural economics and psychology, there is a common assumption that persistent behaviour is attributable to habit formation (Volpp & Loewenstein, 2020). However, Volpp & Loewenstein (2020) argue that there may be other mechanisms at play more closely linked to information acquisition and underlying preferences. For example, it could be that an intervention helps one learn about their "true" preferences (Czajkowski et al., 2015; Loewenstein & Angner, 2003). It may also be that an intervention helps individuals experiment, which reveals new information that they would otherwise not have gained and this rationally shifts behaviour (Larcom et al., 2017). We focus on this experimentation literature below, as it relates closely to our theoretical model (whereby an experience with a behaviour can provide information and shift future behaviour) and the Bayesian updating literature.

We know that experimentation is helpful where there is information uncertainty because it can help reveal the payoffs of different choices (Simon, 1955; Wilde, 1981). Likewise, you can have too much experimentation because searching and experimenting with alternatives is costly (Branco et al., 2016; Simon, 1955). This partly led to Simon's (1955) theory of satisficing, recognising that consumers do not optimise, they instead satisfice by finding an option that is good enough and then stopping the search. This has informed a sub-field of the literature on satisficing and rational inattention (Caplin et al., 2011; Caplin & Dean, 2015; de Oliveira et al., 2017; Gabaix et al., 2006; Sallee, 2014).

However, if people experiment less because of uncertainty and risk aversion, inaccurate estimates of the benefits or because of high adjustment costs, outcomes may be sub-optimal for the individual and society. Below, we discuss two recent papers (one empirical and the other theoretical) on experimentation and optimal outcomes. Larcom et al. (2017) show empirically that people are potentially "under-experimenting" and that exogenously imposed experimentation can benefits individuals and society. Pourbabaee (2022) presents a theoretical model showing why people may under-experiment in certain behaviours or potential outcomes.

Larcom et al. (2017) – Experimentation with the London Underground

Larcom et al. (2017) exploit a natural experiment related to the London Underground transportation system to investigate whether agents make optimal choices and provide some insights into consumer experimentation. In 2014, worker strikes resulted in some London Underground stations being temporarily closed for two days. On these days, some commuters had to experiment with new routes. Larcom et al. (2017) followed these commuters to check whether they changed back to their original routes after the strike. If the transit users had already optimised and were taking the best route before the strikes, we would expect them to revert back to their original routes. However, Larcom et al. (2017) found that many commuters stuck with their new route, indicating through revealed preferences that these commuters had not found their optimal route prior to the closure. Larcom et al. (2017) shows that these effects are stronger when there is higher information uncertainty (in their context, measured by how distorted the London Underground map was for the user's area).

Pourbabaee (2022) - Experimentation in the Continuous Time Bandit Problem

Pourbabaee (2022) presents a theoretical model of experimentation where an individual decides between two options in a two-armed bandit setup (Bolton & Harris, 1999). The individual holds certain beliefs about the benefits of one arm and ambiguous beliefs about the benefits of the other (Pourbabaee, 2022). This is analogous to the behaviours relevant in our theoretical model. The behaviours in our theoretical model have ambiguous (uncertain) returns and are an alternative to the current behaviours individuals are engaged in (which would have near certain returns). Pourbabaee (2022) shows that the individual's choice depends on their ambiguity aversion (or risk aversion) and the relative expected payoffs from each arm. In line with our theoretical model, if the expected benefits are inaccurately low and uncertain, the individual will rarely choose the ambiguous arm even if would deliver a larger payoff.

Satisficing – a final note

If people satisfice (Simon, 1955) with their leisure time, they are likely to experiment with more accessible and common behaviours. In the literature on experimentation, researchers tend to assume that all options are equally accessible and have the same search cost (Gabaix et al., 2006; Pourbabaee, 2022; Wilde, 1981). However, in reality, more common behaviours will be easier to experiment with and there may be a bias towards behaviours that have been more common historically. This could mean people experiment far less with uncommon pro-social behaviours (like volunteering for restoration groups) even though they may be welfare-enhancing to the individual.

2.2.7 Under-estimating benefits literature

We finish this review section on the inaccuracy postulations from our theoretical model. In general, the inaccuracy aspect of our model is not discussed widely in the literature and appears to be a relatively novel hypothesis. Our model states that individuals may systematically under-estimate the benefits associated with pro-social behaviours, which leads to an under-investment in those behaviours. In the literature we reviewed earlier, the potential for inaccurate beliefs and sub-optimal decision-making is widely supported (Ackerberg, 2003; Czajkowski et al., 2015; Larcom et al., 2017). However, few papers make statements or assertions about which direction bias is in and why under different circumstances.

In the limited literature on biased perceptions of benefits and costs, most work focuses on decision-making around physical and mental health (Spitzer & Shaikh, 2022). This emerging branch of the literature does not focus on under-estimating the benefits of intervention, like our model suggests. Rather, it emphasises that individuals often under-estimate their risk of illness or injury, which leads to sub-optimal decisions (Arni et al., 2021; Sakurai et al., 2013; Spitzer & Shaikh, 2022). This directly implies that individuals would under-estimate the benefits of related interventions that reduce the risk of illness.

In our review, we also came across one paper that makes a remarkably similar contention (to our model) about inaccuracy outside of the health context. Welsch and Kühling (2010) theoretically and empirically evaluate the hypothesis that individuals under-invest in environmentally friendly consumption. They hypothesise that the intrinsic nature of the utility received from environmentally friendly consumption may be under-estimated by individuals. They suggest that this puts environmentally-friendly consumption at a disadvantage relative to other products (Welsch & Kühling, 2010). This contention is similar to our hypothesis that the benefits of engaging in pro-social behaviour may be under-estimated because they are difficult to imagine *ex ante* (due to the intrinsic nature of altruism utility). Welsch and Kühling (2010) find empirical support for their theoretical model and results that are consistent with individuals under-estimating the utility from environmentally friendly consumption.

Our model complements the work of Welsch and Kühling (2010) by focussing on wider pro-social behaviours and integrating the effects of uncertainty and adjustment costs. Moreover, Welsch and Kühling's (2010) measure of pro-environmental consumption includes three common behaviours – recycling, reducing water use and purchasing environmentally friendly household products. In our model, we have a particular focus on behaviours that are less well-known and not widely carried out (like nature restoration volunteering). This fact could mean under-estimation issues are even more pronounced in the behaviours relevant to our model.

2.3 Model

Our theoretical model is summarised in Figure 3. Our model is primarily a visual, graphical model that is highly flexible to different conditions, behaviours and environments. We do present some basic mathematical modelling in the final section of this chapter, but the focus is on the graphical framework.

The underlying premise of our model is that there are equilibriums for certain pro-social behaviours that have lower than expected rates of uptake, given the private benefits from engaging in the behaviour. In the context of nature volunteering, the premise is that the social equilibrium volunteering rates are lower than we would expect, given the private benefits. The idea that individuals under-invest in desirable behaviours is not new – this is a fundamental principle of the nudge literature (Congiu & Moscati, 2022; Thaler & Sunstein, 2009).

We posit that there are three key reasons people "under-consume" these socially desirable behaviours:

1) Uncertainty – individuals are uncertain as to the private benefits they will accrue from volunteering. That is to say, their expected utility (or net benefit) function is imprecise. When coupled with risk aversion, the certainty-equivalent expected utility will be less than true utility.

2) Inaccuracy – individuals underestimate the expected private benefits from volunteering, in part because those benefits are hard to concretely describe and there is a lack of information about the benefits. That is to say, their expected utility (or benefit) function is inaccurately low.

3) High adjustment costs – Switching to a new behaviour, good or service brings adjustment costs and there is evidence these are greater for leisure behaviours. These costs could be transaction (search) costs or psychological costs associated with changing your mind.

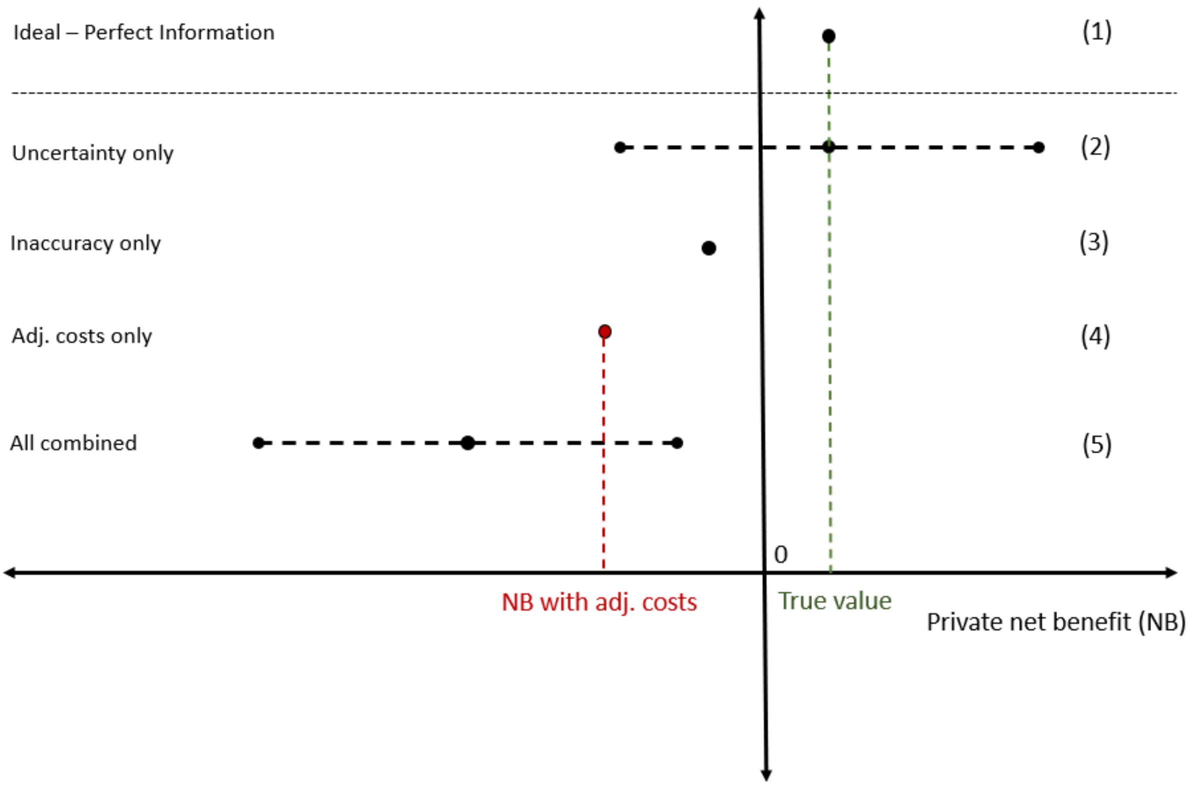

Figure 3. Theoretical model of individual net benefits function from volunteering for a sub-set of the population under different scenarios.

Some behaviours will exhibit all three of the aforementioned characteristics and some behaviours will exhibit none. As policymakers and researchers, we are most concerned with behaviours where people exhibit at least one of the issues above and that results in an under-investment in the socially and personally desirable behaviour.

The case for policy intervention is similar to those described in the nudge literature – we want to help individuals correct errors in their decision-making (stemming from various sources) and account for positive or negative externalities (Carlsson et al., 2021; Thaler & Sunstein, 2009). Our model and its general premise also display synergies with research on the energy efficiency gap, which has also received attention from the nudge and behavioural change literature (Allcott & Greenstone, 2012; Jaffe & Stavins, 1994). The energy efficiency gap states that individuals under-invest in energy efficiency, relative to the private benefits and costs of such investments. Similar to the first two characteristics in our model, this literature argues that the under-investment in energy efficiency usually stems from informational market failures (imperfect information - Allcott & Greenstone, 2012; Jaffe & Stavins, 1994). Inattention to the benefits of energy efficiency is also one of the proposed mechanisms behind the gap (Allcott & Greenstone, 2012). This is somewhat analogous to the inaccuracy component of our model, which talks about individuals having an inattention towards some of the benefits from engaging in the desirable behaviour.

To see how uncertainty, inaccuracy and high adjustment costs act as barriers to socially desirable behaviour, we depict the net benefits function of volunteering for a representative individual of a *subset* of the wider population (Figure 3). This is a subset of individuals that would derive positive net benefits from volunteering but are not currently engaged in volunteering. We are not considering individuals who are already volunteering or individuals who have no intrinsic motivation as they do not apply to our model. We model how the net benefits function would look under different scenarios in Figure 3.

Scenario 1: Perfect information

An individual with perfect information would have a private net benefits function as shown by line 1 in Figure 3. Under perfect information, we can see that the net benefits of volunteering are positive, and the individual should be volunteering.

The uncertainty (or variance) of the net benefits function is represented by the bars around the central dot. This uncertainty represents the uncertainties around the actual experience of volunteering and the types and magnitudes of the benefits from such an experience. Under perfect information, there is no uncertainty, and the individual is certain that volunteering is worthwhile (hence, there are no error bars in line 1).

Scenario 2: Uncertainty

However, in the face of high uncertainty, the individual’s net benefits estimate becomes imprecise (line 2). Even though on average they believe that volunteering will be beneficial, they are not confident in that assessment. In the presence of risk or loss aversion, it is unlikely they will engage in volunteering over other activities they are more familiar with because the expected certainty equivalent utility (or net benefits) is likely less than zero. Particularly, if individuals have built up habits, which we discuss more under scenario 4.

High uncertainty could arise if:

- The individual is not currently engaging in the behaviour, so they have little experiential knowledge of the behaviour.
    - This is exacerbated if the types of benefits from the behaviour have a large individual-specific component (high experience good character).

- Most people are not widely engaging in the behaviour, so there is little collective experiential knowledge of the behaviour and less knowledge of the wider social impacts – this affects uncertainty around altruism utility.
- There is little accessible credible information on the private benefits of the behaviour.

Scenario 3: Inaccuracy

We then turn to inaccuracy in line 3. Due to the nature of some of the benefits of the behaviour, some individuals may under-estimate them or be unaware of benefits altogether. As a result, while the net benefits function has perfect precision (no uncertainty), the point estimate of the net benefits is negative and below the true value (Figure 3). As a result, the individual would not engage in the behaviour even though it would make them better off.

High inaccuracy could arise if:

- There is little accessible credible information on the private benefits of the behaviour and the wider social benefits of the behaviour (social benefits are of relevance for altruists).
- The private benefits have a large individual-specific component (high experience good character) and the individual is not currently engaged in the behaviour.

In response, individuals underestimate the benefits, underweight the benefits or do not account for them at all in their decision-making.

Scenario 4: High adjustment costs

Finally, we turn to high adjustment costs in line 4. Adjustment costs are a common barrier to behaviour change across the board (for examples, see Harris & Kessler, 2019; Polites & Karahanna, 2012). They usually represent the upfront cost of trying a new behaviour, good or service, and this includes the costs associated with finding the behaviour (search costs) and the psychological costs of changing (in general, people like the status quo and have inertia towards change - Samuelson & Zeckhauser, 1988). They are analogous to switching costs, which are reviewed in Burnham et al. (2003).

In line 4, we show the net benefits function for a single period where information is perfect but there are high adjustment costs.[13] The net benefits function is precise but significantly below the true value and below zero, so the individual will not engage in the behaviour. If they did start engaging in the behaviour, the adjustment costs in future periods would essentially fall to zero and the net benefits would be the true value. These adjustment costs are a major factor in explaining why people often

[13] For simplicity, we are only looking at one period. Many individuals would ration some of the upfront adjustment costs over the entire expected benefits in the future. However, we know many individuals exhibit hyperbolic discounting, so the upfront cost would still have a disproportionally large impact on the individual cost-benefit analysis (Laibson, 1996).

choose sub-optimal outcomes for themselves (because it takes time to search through alternatives, so people stop their search when the benefits are "good enough" – see the satisficing literature in the previous section and notably, Simon, 1955).

Adjustment costs are also likely to be larger if the behaviours individuals switch out are habitual (and habits become stronger with age and other factors - Verplanken & Orbell, 2022). Moreover, adjustment costs may be higher when making decisions with time, as what we do with our free time forms a significant part of our social and personal identities (J. B. Davis, 2011; Sen, 1985). These points are discussed further in the literature review in the previous section.

Scenario 5: Combined

These three factors could combine in several ways and for brevity, we have illustrated only the scenario where all three factors are occurring at once. We show this in line 5 where the net benefits function has a wide variance (high uncertainty) and its centre is significantly below the true value. As with the previous three scenarios, the individual will not engage in the behaviour despite the fact that the true net benefits are positive.

2.4 Financial incentives – a model exercise

One of the benefits of our model and framework is that we can use it to flexibly think about the design of behaviour change efforts for different individuals and how we might create sustained change. For example, we could design interventions that change the net benefits function temporarily (to encourage uptake over a period of time) or change the net benefits function more permanently, to encourage long-term change. One intervention that may do both is a temporary financial incentive, which has effects on the initial net benefits function and may also alter the long-run net benefits function.

We depict this scenario in Figure 4. Imagine we start with a net benefits function in line 2 that is well below zero because of uncertainty, inaccuracy and adjustment cost effects (this is the same as line 5 from Figure 3). This could be because the individual has never tried the behaviour and few others are engaging in the behaviour.

We could design an intervention that provides some information about the desired behaviour and offers a temporary financial incentive for engaging in the behaviour. This would do two things to the net benefits function:

1. Reduce the uncertainty of the function because individuals have more information about the behaviour.[14]

[14] The information could also shift the net benefits function if it partially corrects any inaccurate prior beliefs.

2. Shift the net benefits function to the right by exactly the incentive value (this increases the expected net benefits by the incentive value at every point on the function).

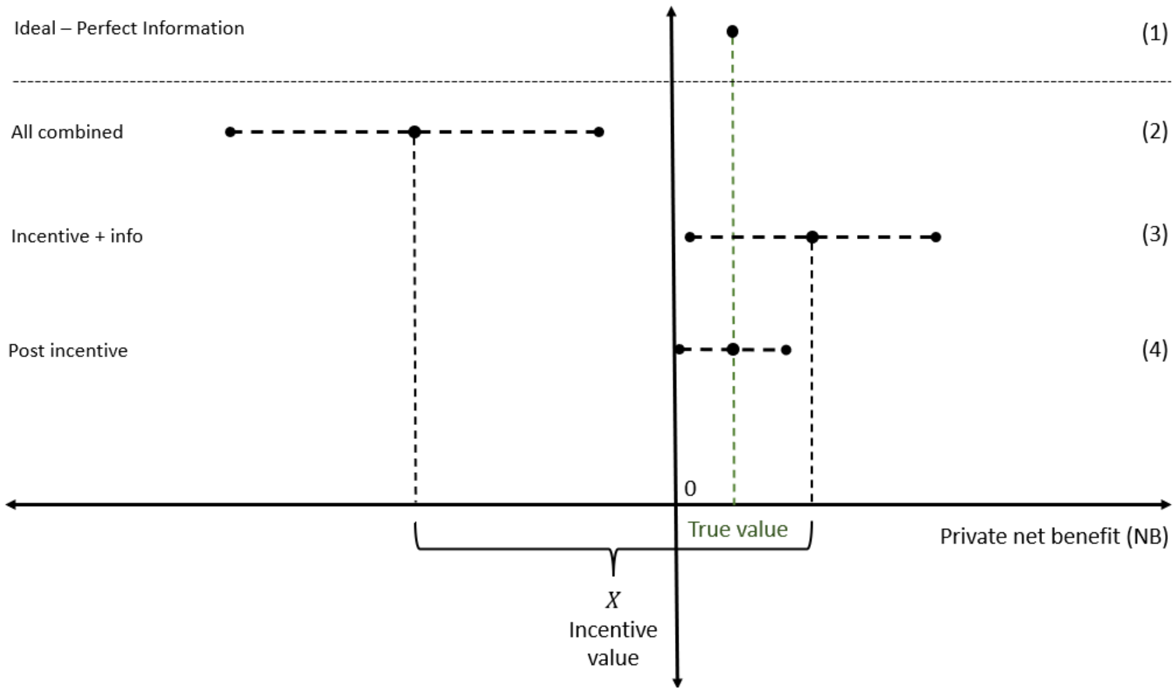

Figure 4. Theoretical model of net benefits from engaging in a behaviour before, during and after an incentive is provided. The all combined function (line 2) is the net benefits for someone experiencing uncertainty, inaccuracy and high adjustment costs.

The goal of the incentive and information is to shift and alter the net benefits function such that some people who were not engaging in the behaviour are sufficiently motivated to start engaging in the behaviour. In Figure 4, this could look like providing an incentive of value X which shifts the entire net benefits function for our individual into strictly positive territory. The incentive works to "make sure" the individual knows that they will benefit from engaging in the behaviour. Even if the experience itself is not as good as expected, they still receive the incentive which will result in a net benefit.

After individuals have started engaging in the behaviour, the incentive is removed which would shift the net benefits function back to the left. However, because the incentive encouraged the individual to start engaging in the behaviour, we observe the following:

1. Uncertainty declines significantly because the individual has now had first-hand experience engaging in the behaviour. As a result, then net benefits function becomes narrower and has a smaller variance.

2. The inaccurately low initial estimates of the benefits are corrected through experiencing the behaviour, so the net benefits function shifts out to the right and is in alignment with the true value (less adjustment costs).[15]

3. Finally, the initial adjustment costs dissipate because the incentive has prompted the individual to face those costs in the previous period and start engaging in the behaviour. As a result, the net benefits function shifts to the right and is centred around the true value of the net benefits.

These three effects are shown in line 4 of Figure 4. In our diagram, the new net benefits function is in strictly positive territory and the individual should continue engaging in the behaviour absent the incentive. Hence, the incentive has crowded in future behaviour by encouraging individuals to "try something new" or "experiment" (Larcom et al., 2017; Pourbabaee, 2022).

2.5 Crowding in model predictions

Our model shows we may be able achieve a "crowding in" effect using a financial incentive, which stands in contrast to the conventional wisdom that financial incentives crowd out intrinsic motivation (Frey & Jegen, 2001; U. Gneezy & Rustichini, 2000).[16] Indeed, it is now widely recognised that incentives have both crowding in and crowding out potential (Brent et al., 2017; U. Gneezy et al., 2011; Rode et al., 2015). However, there is still mixed evidence on where and when financial incentives crowd in intrinsic motivation and work to promote long-term behaviour change. As Gneezy et al. (2011) discusses, the impacts of an incentive are highly context-specific and depend on how the incentive is framed.

Because of these mixed results, experimental practitioners often stay away from direct financial incentives to avoid the risk of crowding out intrinsic motivation (Al-Ubaydli, List, LoRe, et al., 2017; Frey & Jegen, 2001). Moreover, financial incentives are difficult to find public support for, particularly for encouraging pro-social and pro-environmental behaviours where researchers have raised significant concerns around crowding out effects (Bowles & Polania-Reyes, 2012; Brent et al., 2017; Ling & Xu, 2021; Rode et al., 2015). However, in our case, if a privately desirable behaviour (positive underlying net benefits) is surrounded by uncertainty and infrequently performed, using a financial incentive to promote initial behaviour uptake could crowd in future behaviour.

[15] This assumes that the experiences during the incentive period are 100% effective at correcting inaccurate beliefs, as per the previous section.

[16] In saying that, many papers have shown financial incentives have heterogenous crowding in and crowding out effects (d'Adda, 2011; Gneezy et al., 2011; Kerr et al., 2019).

In the volunteering context, if we provide an incentive to volunteer for the first time, we predict that this would crowd in future volunteering from individuals who are well-represented by our model (those with positive net benefits from volunteering, who are not currently volunteering).

Indeed, a recent paper reports on findings that align well with our model. Gravert & Olsson Collentine (2021) show that an economic incentive to use public transit (free fares for a period of time) crowded in future public transit behaviour. The authors show that this effect is driven by low users who start using public transit during the incentive period. Our model also aligns well with findings elsewhere in the literatures on Bayesian updating, the economics of information and forced experimentation, which are discussed in the literature review section.

2.6 Model summary and wider applications

Overall, our model shows that three main factors (uncertainty, inaccuracy and adjustment costs) may help explain the lower-than-expected private investment in desirable behaviours that would enhance welfare for both individuals and society (like volunteering). Our model has been built on the insights from a wide range of literature, including the economics of information, Bayesian updating, satisficing and habits literatures. Moreover, the notion that there is an under-investment in desirable behaviours is central to much of the nudge and behavioural change theory (Congiu & Moscati, 2022).

Through a model exercise, we show that interventions can be designed to overcome these three barriers and that helping individuals experiment with new behaviours could crowd-in future socially desirable behaviour (in line with the literature on experimentation and satisficing - Larcom et al., 2017). Our model is applied to the case of volunteering, but may be relevant to other socially desirable behaviours that exhibit one or more of the following attributes:

1) *Research shows that the behaviour has substantial private benefits, yet few people are engaging in the behaviour (the behaviour is uncommon).*

This is an early indicator that a behaviour may be being under-invested in by individuals because they are failing to recognise certain benefits, are uncertain about the benefits or face high adjustment costs.

2) *The private benefits of the behaviour are hard to describe and/or are strongly individual-specific. Often, these benefits have high experience character.*

When benefits are individual-specific and have experience character, they are more difficult to describe and inform people about. This tends to lead to uncertainty and inaccuracy. This uncertainty and inaccuracy will be larger if the individual has not had direct experiences with the behaviour.

3) *The behaviour requires a significant time investment or would displace other habitual behaviours (leading to high adjustment costs).*

As literature shows, changing habits is particularly challenging because of high adjustment costs (Gravert & Olsson Collentine, 2021). Furthermore, leisure time is scarce so a significant time investment may induce greater risk aversion and higher adjustment costs.

4) *Information about the behaviour is relatively scarce, of low quality or difficult to access.*

If there is little information available, or that information is low quality, there will be higher levels of uncertainty and potentially inaccuracy around estimating the benefits of engaging in a behaviour.

In the final section, we formalise the model in a relatively simple manner to show the basic intuition mathematically.

2.7 Formalising model

In this section, we briefly formalise our theoretical model. We perform a relatively simple formalisation to capture the overarching concepts of the theory. Future researchers may want to formalise the model further in such a way that structural parameters can be estimated empirically. However, this was out of the scope for this thesis. For this formalisation, we focus on showing the value of the incentive required to encourage individuals to start engaging in a behaviour that will make them better off in the long-run.

Our model has two periods denoted by $t = 1$ and $t = 2$. In the first period ($t = 1$), an incentive is offered to encourage initial behaviour uptake. In the second period ($t = 2$), the incentive is removed. This is similar to the incentives section earlier. The key decision rule for individual i at time t is that the behaviour will be undertaken if the net benefits are greater than zero ($NB_i^t > 0$).

2.7.1 Costs

When evaluating net benefits (NB), the individual faces a series of costs and benefits. The costs for individual i at time t are defined as

$$\boldsymbol{C_i^t = C_{opp,i}^t + C_{adj,i}^t} \tag{2}$$

where $C_{opp,\boldsymbol{i}}^t$ are the opportunity and transaction costs and $C_{adj,\boldsymbol{i}}^t$ are the adjustment costs for moving to a new behaviour.

2.7.2 Benefits and inaccuracy

The expected benefits for individual i at time t, $\hat{B}_i^t$, are defined as

$$\hat{B}_i^t = B_i * \lambda_i^t \tag{3}$$

where B_i are the true benefits that the individual would receive from participating (which is assumed to be time invariant) and λ_i^t is an under-estimation parameter[17] that is equal to the proportion of benefits that are known at time t for individual i. This captures the inaccuracy aspect of our model.[18]

As a proportion, $\lambda_i^t \in [0,1]$ where $\lambda_i^t = 1$ defines the case where the individual is aware of all benefits they would potentially receive and $\lambda_i^t = 0$ represents the case where all benefits are unknown and thus estimated to be zero. We note that even if $\lambda_i^t = 1$ and the individual knows of all possible benefits, the size of these benefits is estimated with uncertainty $\sigma_{\hat{B}}^2$.

2.7.3 Uncertainty and risk aversion

The expected net benefits function has uncertainty which is captured by the variance of Equation 3 which is equal to $\sigma_{\hat{B}}^2 = \sigma_B^2 * \lambda^2$. Introducing uncertainty in the form of $\sigma_{\hat{B}}^2$ allows us to incorporate risk aversion and the influence of uncertainty on decision making.

We define a general risk aversion term R_i^t that is a function of both relative risk aversion θ_i^t and the level of uncertainty $\sigma_{\hat{B}}^2$.

$$R_i^t = R\left(\theta_i^t, \sigma_{\hat{B}}^2\right) \tag{4}$$

where $R_i^t \in [0,1]$ and $R_i^t = 1$ represents risk neutrality (or the case where uncertainty is zero) and $0 \leq R_i^t < 1$ denotes risk aversion. For the purposes of this model, we only worry about risk aversion and not risk-loving behaviour as risk-loving individuals are more likely to have already tried the uncertain but potentially beneficial behaviour at hand. Our treatment of uncertainty and risk aversion is highly simplified here and future researchers may want to expand with more complex modelling and functional forms in the future.

2.7.4 Expected net benefits

Returning to the decision rule, we define the expected net benefits as:

$$NB_i^t = R_i^t * \lambda_i^t * B_i - C_{opp,\boldsymbol{i}}^t - C_{adj,\boldsymbol{i}}^t \tag{5}$$

which states that individual i will engage in the behaviour if the certainty-equivalent (adjusted for risk aversion parameter R_i^t) expected benefits (B_i adjusted for inaccuracy scaling parameter λ_i^t) exceed the opportunity and adjustment costs.

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[17] As we mention earlier, we only focus on under-estimation of the benefits (or inattention to some of the benefits) as this is the case where we may see an under-investment in a desirable behaviour.
[18] This is similar to the idea of inattention towards benefits, as discussed in the energy efficiency literature (Allcott & Greenstone, 2012)

2.7.5 Changes over time

We assume that if an individual engages in the behaviour in the first period (meaning $NB_i^{t=1} > 0$) this will perfectly reveal all information such that uncertainty and inaccuracy are no longer issues for the individual. This is a simplifying assumption to make the formalisation more tractable.

Formally, if $NB_i^{t=1} > 0$ and the individual participates at $t = 1$, benefit uncertainty $\sigma_{\hat{B}}^2$ will fall to zero and the risk aversion parameter $R_i^{t=2}$ collapses to 1. Moreover, all benefits are known so $\lambda_i^{t=2}$ collapses to 1. Finally, we assume adjustment costs will fall to zero in the second period if the individual has already started engaging in the behaviour ($NB_i^{t=1} > 0$). These simplifying assumptions will help us show the size of the incentive required to encourage initial behaviour.

2.7.6 Incentives

We want to focus on individuals that would benefit from a socially desirable behaviour but are not currently engaged in the behaviour. The minimum condition for this to be the case is that the private benefits of the behaviour to individual i are equal to the opportunity and transaction costs of the behaviour ($B_i = C_{opp,\boldsymbol{i}}^t$).

We can re-write the net benefits function earlier as

$$NB_i^t = B_i - B_i * \left(1 - R_i^t * \lambda_i^t\right) - C_{opp,\boldsymbol{i}}^t - C_{adj,\boldsymbol{i}}^t \tag{6}$$

If we impose the condition $B_i = C_{opp,\boldsymbol{i}}^t$ the equation becomes

$$NB_i^t = -B_i * \left(1 - R_i^t * \lambda_i^t\right) - C_{adj,\boldsymbol{i}}^t \tag{7}$$

This equation shows the estimated net benefits for individual i at time t assuming that the true benefits are exactly equal to the opportunity and transaction costs. This equation tells us that the expected net benefits NB_i^t will be equal to zero if $R_i^t * \lambda_i^t = 1$ (no effect of risk aversion, uncertainty and inaccuracy) and there are no adjustment costs. However, as we outline in our model, this is unlikely to be the case and NB_i^t is likely to be below zero. To shift the expected net benefits to zero (which is the threshold for starting the behaviour), an incentive can be used that equals at least

$$Incentive_i = B_i * \left(1 - R_i^t * \lambda_i^t\right) + C_{adj,\boldsymbol{i}}^t \tag{8}$$

This equation shows that to encourage initial engagement in a behaviour, the incentive value is increasing in the size of the benefits from the behaviour (if $R_i^t * \lambda_i^t \neq 0$), increasing in risk aversion, increasing in inaccuracy and increasing in adjustment costs. This alludes to the fact that individual heterogeneity in risk preferences, information, habits and general preferences will give rise to heterogeneity in the incentive required to stimulate behaviour change. For example, if an individual has full knowledge of the benefits and is risk neutral, the incentive need only cover the adjustment costs of engaging in the behaviour. Under our earlier assumptions, if the incentive is sufficiently high to encourage initial behaviour uptake, the influence of risk aversion and uncertainty, inaccuracy and adjustment costs fall to zero. Hence, in the second period, if $NB_i^{t=2} = B_i - C_{opp,\boldsymbol{i}}^{t=2} > 0$ the individual will continue to engage in the behaviour.

The working above applies to the case where the individual's underlying benefits exactly equal opportunity costs. More generally, given our net benefits equation, the minimum incentive required to ensure $NB_i^t \geq 0$ is:

$$Incentive_i = C_{opp,\boldsymbol{i}}^t + C_{adj,\boldsymbol{i}}^t - R_i^t * \lambda_i^t * B_i \tag{9}$$

The required incentive is increasing in costs, decreasing in benefits, increasing with risk aversion and uncertainty (as R_i^t falls) and increasing in inaccuracy. These properties are well described in the earlier figures used in the theoretical model which shows a larger shift or incentive is required with additional uncertainty, inaccuracy and adjustment costs.

2.7.7 Heterogeneity

We can also see that if the benefits are sufficiently high, the minimum required incentive will be zero or negative, meaning the individual should not need an incentive to engage. These are the individuals that are likely to be already engaging in the behaviour. Alternatively, these are the individuals where simple information, opportunity or capability interventions may shift behaviour and no additional incentivisation is required. The key takeaway is that introducing a constant (or fixed) incentive for a group of individuals that are not engaging in a behaviour is likely to deliver significant response heterogeneity. This heterogeneity is explored empirically in Chapter 4.

Chapter 3: Field Experiment Design

3.1 Introduction

Our field experiment revolves around volunteering for nature restoration groups. Volunteering for nature restoration is a public good, increases welfare for society and plays a crucial role in addressing environmental issues like climate change and biodiversity loss (Ryan et al., 2001). In many instances, volunteering is also welfare-enhancing for the individual (although, as we show in our theoretical model, some people may not realise this – see Chapters 1 and 2). Our field experiment is thus designed with the overall aim of increasing participation in nature restoration volunteering.

We focus on two key areas, which form stage one and two of the field experiment. In stage one, we test the efficacy of different interventions for encouraging first-time nature volunteering. In stage two, we evaluate the effects of engaging in nature restoration volunteering for the first time on future behaviour and other outcomes of interest.

We have three overall research questions that inform our experimental design and are addressed in either stage one or two of the field experiment. We detail specific hypotheses around these research questions in later chapters (Chapter 4 and 5).

Our three key research questions are:

RQ1) Can we increase first-time volunteering through the use of nudge and a supermarket voucher incentive?

RQ2) Does intervening and providing people with their first experience volunteering lead to increases in future volunteering behaviour?

RQ3) Does a first experience volunteering lead to increases in other outcomes (like connectedness to nature and subjective wellbeing)?

In the following sub-section, we briefly outline the overall aims for stage one and two of the field experiment.

3.1.1 Aims

Our first stage focuses on evaluating ways to encourage people to try volunteering for the first time. We want to encourage people to take the first step and put their hand up for volunteering, in the face of uncertain (and potentially inaccurate) priors around the benefits of volunteering and high adjustment costs (see our theoretical model in Chapter 2). To do this, we design three treatments (a nudge, a voucher and a combined nudge and voucher) that aim to reduce barriers and uncertainty and incentivise participation in volunteering activities. We aim evaluate how effective these treatments are at encouraging first-time volunteering relative to a control group. This forms stage one of our field experiment.

Recognising that the experience of volunteering for the first time provides important information about the benefits of volunteering, stage two aims to evaluate the effects of volunteering for the first time on future behaviour and other outcomes of interest (like environmental identity). For example, once people have tried volunteering, they may be more likely to volunteer in the future as they now have significantly more information on the personal benefits of volunteering. An experience volunteering could also shift individuals' preferences and values (as experiences are the building blocks of identity and thus preferences - Davis, 2003). In essence, we aim to test a "crowding-in" intervention for increasing volunteering. That is, can we crowd-in long-term volunteering rates by helping people to experience volunteering for the first time? Our theoretical model predicts this may be the case if an experience helps reveal unknown information about the benefits of volunteering, reduces the uncertainty around the benefits of volunteering and overcomes the initial adjustment costs associated with changing behaviours.

In the next section, we briefly review the literature on field experiments in economics, highlighting recent recommendations on best practice for experiments and their designs. We then describe our field experiment design in full for increasing volunteering with nature restoration groups in Aotearoa New Zealand. This includes details on recruitment, field experiment partnerships and communications with participants.

3.2 Field experiments literature review

In this section, we briefly review the literature on field experiments in economics and their application to pro-environmental behaviour (PEB) research. This is useful to contextualise our field experiment design and contributions to the literature.

Field experiments are being increasingly used to understand the drivers behind PEBs and increase the uptake of PEBs. Field experiments are broadly seen as complements to laboratory (lab) experiments, which was the dominant experimental approach over the past three decades in economics (Brent et al., 2017). In the field, researchers can draw more reliable inferences about the population of interest, whereas lab experiments tend to have lower external validity because they are conducted with university students and are in an environment that promotes unnaturally high levels of compliance (Al-Ubaydli, List, & Suskind, 2017). However, lab experiments are important for making methodological and theoretical contributions in highly controlled environments (see Harrison, 2013, 2014). In contrast, field experiments afford researchers less control in the experimental conditions but greater control over who participates in the experiment (and thus, more control over the degree of external validity of the results - Al-Ubaydli & List, 2015). In saying that, there are various classifications of field experiments and there are increasingly opportunities to get the best of both the lab and the field. For example, using virtual reality (VR) headsets in the lab can allow participants to be exposed to "real world" or "field" stimuli. In line with Harrison & List (2004), Brent et al. (2017) defines three types of field experiments in environmental economics:

1) Natural field experiments (these are done in the field without the knowledge of the participants – these experiments raise ethical concerns but are incredibly useful in eliminating experimenter, response and selection biases).
2) Framed field experiments (these are done in the field with the expressed consent and informed knowledge of participants).
3) Artefactual field experiments (these are conducted in the lab, but carry some aspects of the fields - usually, they use non-standard subject pools (not university students). Artefactual experiments are an example of blending field and lab concepts into one experiment and are often referred to as "lab-in-the-field" experiments (Gneezy & Imas, 2017).

One of the most important steps in conducting good field experiments is developing a thorough understanding of the local context and carefully designing the interventions to identify the effects and parameters of interest (List, 2011). Over recent years, several review papers have provided guidance on running field experiments and we summarise a few of the key gaps and recommendations from these papers.

3.2.1 Scalability issues

Firstly, scalability issues are a major concern for field experiment practitioners. For example, John List recently published a book called "The Voltage Effect", which considers where and why "voltage drops" occur. That is, why powerful experimental results do not scale up. Al-Ubaydli et al. (2017) assert that the representativeness of the population and the situation are two key factors contributing to scalability issues. They describe the concept of adverse heterogeneity – where participants' attributes make them pre-disposed to showing a stronger relationship than we would otherwise expect in the population. For example, if an experiment requires informed consent, those with the most to gain are more likely to participate (and likewise, those who may be adversely affected are likely to opt-out). Moreover, researchers often (consciously or subconsciously) choose behaviours, situations or participants that will show more favourable results. A major driver of this is publication bias (the preference of journals to publish significant results), which inherently incentivises this behaviour (Al-Ubaydli, List, & Suskind, 2017).

Cooper et al. (2015) studied the sustainability (long-term viability, which is closely connected to scalability) of programmes to prevent youth behavioural issues in Pennsylvania. The authors found that sustainable programmes had stronger relationships and connections with local communities and stakeholders. Furthermore, McCoy (2015) found that scalability and fidelity to the original research design is higher when the underlying mechanism is described in the initial research. These two papers point to the importance of working closely with the community from the start (to ensure "buy-in" and effective intervention designs) and grounding experiments in theory (a point we discuss more next).

3.2.2 Theoretical grounding

Another major issue is the absence of theoretical grounding in many field experiments (Harrison, 2013). Often, field experiments are driven by industry and focus on a specific local context. As such, theoretical advances and grounding tends to be less of a focus. However, Wolpin (2013) and Harrison (2013) argue that theoretical grounding is crucial for improving the usefulness and accuracy of inferences from experiments. Brent et al. (2017) discusses this in the context of field experiments in environmental economics and argues that many results are difficult to replicate and extrapolate from without grounding in generalisable theory. Indeed, several papers have argued that economists should be doing more structural estimation (Al-Ubaydli, List, LoRe, et al., 2017; Harrison, 2014). DellaVigna (2018) shows field experiments lend themselves to structural estimation, but not many experiments employ such methods. This is despite the range of benefits from structural estimation, including the ability to estimate structural parameters, improving the experimental design and allowing for welfare analysis. DellaVigna (2018) also shows that structural estimation can be relatively simple if field experiments are carefully designed, and it doesn't take away from the key reduced form findings that policymakers and industry are usually more interested in.

3.2.3 Selection of behaviours

Thirdly, and particularly relevant, is that many papers reporting on field experiments do not include descriptions or reasoning for the selection of specific behaviours. Grilli & Curtis (2021) show that this is particularly an issue in experimental studies that focus on PEBs. This is concerning because if we want to prioritise cost-effective environmental action, we need to carefully consider which behaviours will contribute the most to our end goals (which, usually for studies of PEBs, is improving environmental outcomes). As Al-Ubaydli, List, LoRe, et al. (2017) eloquently put it:

> *"Doctors want patients to get better, not to take pills; equivalently, economists should want people to experience superior outcomes, rather than to modify their behavior as an end in and of itself. In this manner, a bit of backward induction at the design stage can go a long way."*

More backward induction at the design phase will not only improve the selection of target behaviours but may also improve the external validity of experimental results by grounding experiments in structural mechanisms and theory. And, of course, prudent behaviour selection will also minimise wasteful spending and enhance the contributions to the specific end goals at hand.

In the following sections, we present our full field experiment design. Using insights and recommendations from the literature, this design was crafted with our theoretical model front of mind and a strong understanding of the local context through initial discussions with stakeholders in the field (see Chapter 1 for more on the local context). Moreover, as outlined in Chapter 1, our target behaviour (volunteering for nature restoration groups) was chosen through an explicit selection process designed to maximise environmental impact.

3.3 Full design overview

3.3.1 Overall design

This section will briefly outline the overall field experiment design (stages one and two) and later sections will provide more detail on specific aspects of the experimental design (for example, recruitment, survey design and population description). We depict the overall design in Figure 5 below.

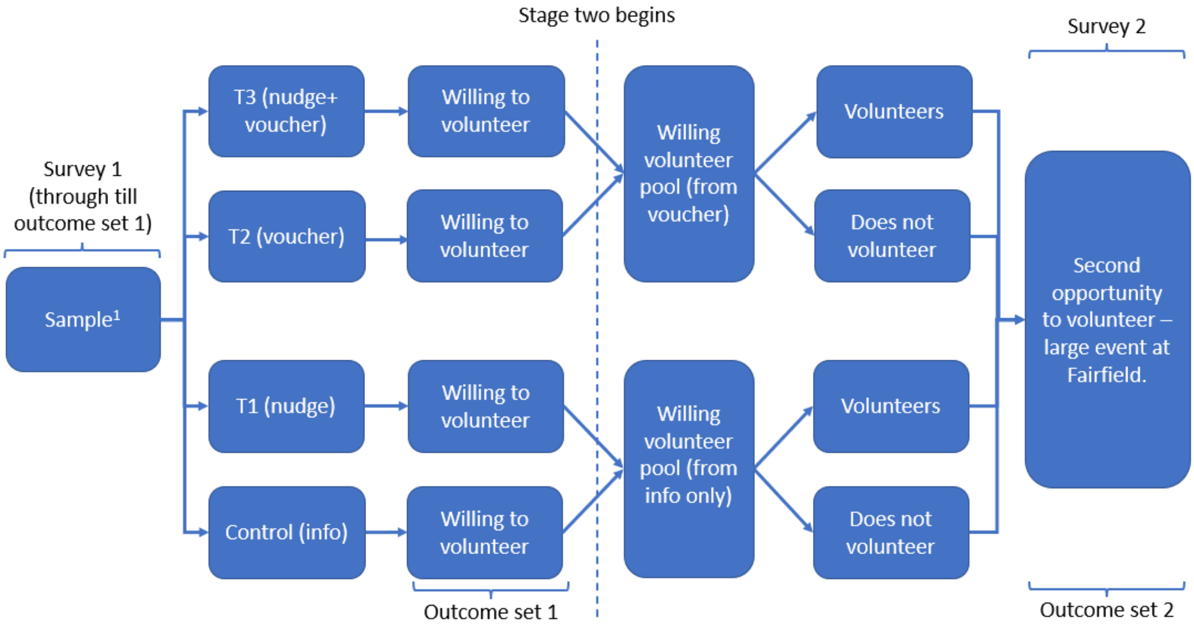

Figure 5. Overall field experiment design. 1 The initial sample of interest are first-time volunteers.

3.3.2 Stage one design

We start by recruiting a sample of first time-volunteers so we can test the effects of different interventions on nature volunteering behaviour and the effects of first-time experience volunteering. We do this using an online survey (survey one) administered through the Qualtrics platform targeted at first-time volunteers residing in or near Hamilton, New Zealand. Within survey one, we randomly assign individuals to one of four groups as per Figure 5:

1) Control (standard information about the volunteering event)

2) Treatment 1 (T1): Nudge (standard information + nudge aimed at environmental and social preferences)

3) Treatment 2 (T2): Voucher (standard information + $50 NZD supermarket voucher)

4) Treatment 3 (T3): Nudge + Voucher (combining the two treatments above).

Following random assignment, we assess differences in willingness to volunteer using three different variables (Figure 6).

Immediately following random assignment, we ask individuals whether they would be willing to sign-up for a nature volunteering event sometime over the next month and specify days they may be available. This is a stated preference variable that we call "pre-commitment" because individuals are committing to attend but have not yet committed to a specific date or time.

After survey completion, we reach out to all pre-committed individuals asking them to confirm whether they will be able to attend one of two volunteering events.[19] We call this variable "commitment" because we ask individuals to confirm their attendance at a specific event and inform us how many family members will be attending with them.

Finally, we observe whether individuals attend a volunteering event, denoting this variable as "attendance". We evaluate whether the treatments have any effects on all three measures of willingness to volunteer.

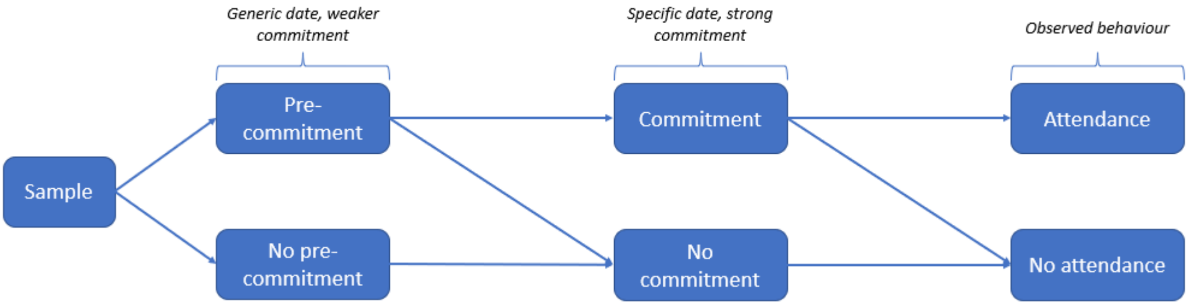

Figure 6. Outcome volunteering variables. Pre-commitment is general commitment to attend a volunteering event in the next four weeks. Commitment is commitment to attend a specific event and attendance is actual attendance at an event.

We also check whether the voucher incentive has any crowding out effects of intrinsic motivation by asking individuals if they would like to donate some of their survey prize money to an environmental not-for-profit (if selected as a winner). This is a different but related pro-environmental behaviour to

[19] There are four events in total, but individuals are only offered a choice between two events. This is to keep those receiving vouchers and not receiving vouchers separate throughout the experiment.

volunteering, which makes it well-suited to measuring spillover effects. This, along with the measures of volunteering, form the outcome set for stage one.

3.3.3 Stage two design

In stage two, we group the respondents who pre-committed to an event into our pool of willing volunteers. We separate those who were offered a voucher from those who were not to avoid situations where these two types of individuals mix at the same volunteering event. We do not further separate by the nudge treatment because the nudge is more subtle and the likelihood of treatment groups mixing in ways harmful to the experimental design are minimal.

We then give all willing volunteers the opportunity to attend one of two volunteering events and observe who attends. Attendance at an event is our treatment variable in stage two because we want to evaluate the effects of a first-time volunteering. While attendance is not randomly assigned, we show later that attendance is predominantly a function of two exogenous factors – availability (the number of days a person is available directly affects their ability to attend an event) and being offered a voucher incentive (in stage one). Once we control for these two factors, as well as demographics and environmental attitudes, the remaining variation in attendance is plausibly random and thus attendance can be treated as conditionally exogenous. We provide further details in Chapter 5.

Following our conditionally randomly assigned treatment (attending a nature volunteering event for the first time), we carry out our second main survey, inviting all those who completed survey one to complete survey two. We again ask whether individuals would like to pre-commit and commit to attending a volunteering event later in the month. We also observe who attends this follow-up event, which allows us to evaluate the effects of a first-time experience on pre-commitment, commitment and attendance at future volunteering occasions. In survey two, we also re-measure a range of other outcomes of interest (like environmental identity, wellbeing and perceptions of environmental organisations) so that we can evaluate the effects of volunteering for the first time on these outcomes. These supplementary outcomes, along with the future volunteering variables, form the outcome set for stage two (Figure 5).

3.4 Ethics and pre-registration

The full experimental design we present in this chapter was reviewed and received approval from the Waikato Management School (WMS) Human Research Ethics Committee, application number: WMS 22/134.

Moreover, we pre-registered our hypotheses and analyses before carrying out the experimental design. In the following chapters, we will report on specific hypotheses that were pre-registered and tested in our experiment. We pre-registered the experiment on AsPredicted, by the Wharton Credibility Lab. The pre-registration #119297 is titled: *"Volunteering for restoration groups - Field experiment"* and is publicly available at the following link: https://aspredicted.org/qi57d.pdf

3.5 Recruitment, surveys and tracking

3.5.1 Initial recruitment

We recruited our initial sample through our first online survey (survey one) administered on Qualtrics. A copy of this survey and our second main survey (survey two) are in Appendices A and B. We incentivised survey completion by offering the chance to win one of five $100 NZD Prezzy Cards and promoted the survey widely in Hamilton, New Zealand. Over a three week period (January 20th to February 10th, 2023), we promoted the survey on social media platforms, through pamphlet drops and by putting up posters in the community and at workplaces.

We used a combination of Facebook, LinkedIn, Instagram and Twitter paid advertising to promote the survey. Our social media posts and survey links were also shared by several local organisations, including Volunteering Waikato and GoEco. Figure 7 shows what these social media advertisements looked like on Facebook. The full blurb read:

> *"Do you live in Kirikiriroa Hamilton? If so, we want to hear from you!*
>
> *Please help researchers understand engagement with restoration groups by completing this short (10-minute) survey. We'd like to hear from everyone!*
>
> *You will also go in the draw to win* ***one of five $100 Prezzy Cards!****"*

We distributed 5,000 A5 pamphlets to properties around the following suburbs: Fairfield, Enderley, Chartwell, Queenwood, Claudelands and Hamilton East. We present the pamphlet design in Figure 8, which has a QR code for easy access to the online survey

We also put up and distributed 100 A3 posters around central Hamilton, Fairfield, Hamilton East and popular family locations. In total, we gave posters and pamphlets to 50 businesses or organisations, including local libraries, community centres, supermarkets and corporate offices. The poster design was identical to the pamphlet design in Figure 8.

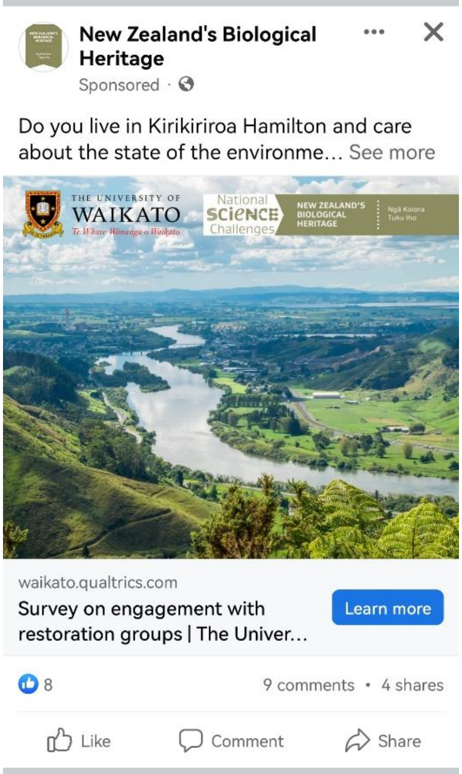

Figure 7. Example of Facebook advertisement for promoting survey one.

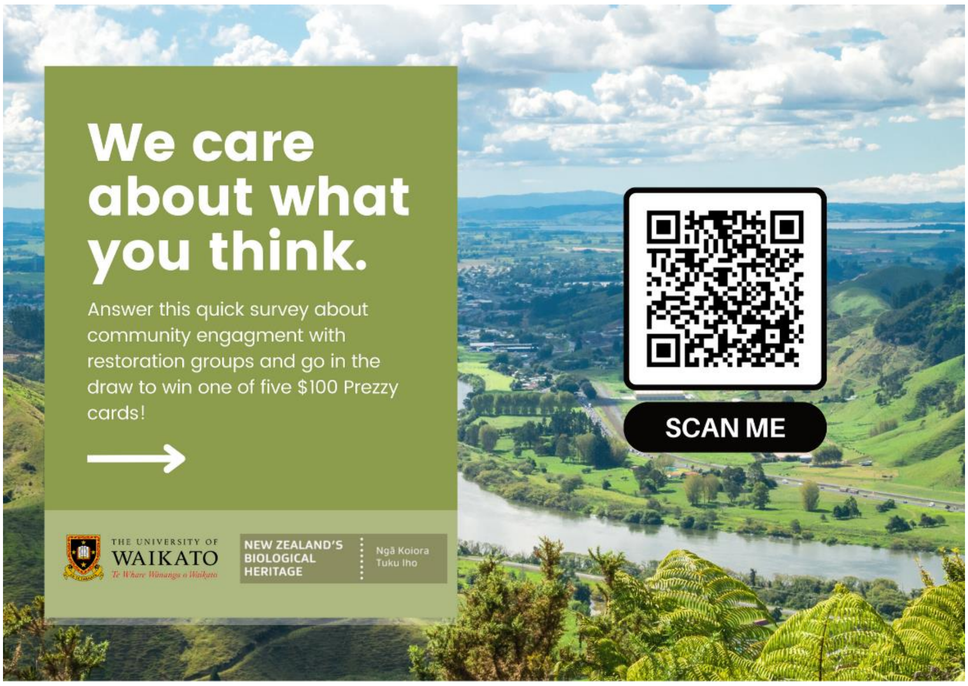

Figure 8. Pamphlet design for initial recruitment.

3.5.2 Exclusion criteria

The focus of our study is on increasing volunteering for nature restoration groups amongst adults who are not currently involved with nature volunteering. As such, our *target population* is adults in urban areas that are not currently volunteering for nature restoration groups ("first-time" volunteers) but have some willingness to become involved.

We did allow people who had volunteered for nature restoration groups in the past three years to complete the survey, but these individuals were not randomly assigned to treatment groups in stage one or two of the experiment. In addition, we ended the survey early for respondents who met any of the following conditions:

- Are under 18 years of age
- Does not live in or near Hamilton
- Knows other household members who have done the survey
- Are "very unwilling" to volunteer for a restoration group

We excluded those who are "very unwilling" to volunteer because they are unlikely to be shifted by any treatments and are not in our target population of interest. Moreover, we limited the experiment and survey eligibility to those who live near or in Hamilton, so that everyone in our survey could reasonably attend a volunteering event if they were willing and able. Finally, we only allow one person from each household to complete the survey to avoid situations where different household members are assigned to different treatment groups and the independence of groups becomes violated.

3.5.3 Communications during the experiment

We used texts and emails to communicate with respondents about the second survey and the volunteering events that they may have pre-committed to. We ensured messaging and communications were consistent between the stage one treatment groups to ensure we did not introduce any bias into the design.

Volunteering events and commitment

For all respondents who pre-committed to a volunteering event, we sent information about the two volunteering events that were available to them (different events for those who were and were not offered a voucher). We asked participants to tell us whether they would attend an event or not (commitment), and if they were attending, how many family members they would bring and which activities they were most interested in.

We sent reminders to complete our commitment surveys eight days, four days and two days ahead of the expected date of the first event.[20] If a participant had already responded after the first reminder (or

[20] We use "expected" because one event was moved from a Wednesday to the following Monday due to adverse weather conditions.

second), they did not receive unnecessary follow-up communications. An example email that was sent to respondents is in Appendix E and the full commitment survey is in Appendix C.

Post-experience survey

We sent a short survey after our volunteering events to ask participants how they found the experience. This served two purposes. Firstly, it provides useful feedback to our field experiment partners and other restoration groups on how they could enhance the volunteering experience. Secondly, it allows us to explicitly assess whether individuals under-estimated the benefits from volunteering, as our theoretical model predicts. A copy of this short survey can be found in Appendix D.

Survey two

The final survey (survey two) was sent out to all participants who completed at least 75% of survey one and met all of the inclusion criteria outlined above. This includes the sample of individuals who were already volunteering and thus were not part of the experiment itself. Survey two was also incentivised, with respondents having a chance to win one of another five $100 NZD Prezzy Cards.

Follow-up event

During survey two, all respondents were asked whether they wanted to pre-commit and commit to a follow-up public event on the 25th of March. In addition, we set up a workflow to automatically send an email reminder about this event immediately after a survey response was recorded. We also sent one final reminder about the event and survey two to all survey one respondents on the 16th of March. A copy of the email with the follow-up event details is in Appendix E.

3.6 Field experiment partnership for volunteering events

3.6.1 Partnership overview

For the field experiment, we partnered with the Fairfield Project to deliver the volunteering events and answer our key research questions. The Fairfield Project is an urban biodiversity and gully restoration group in Kirikiriroa | Hamilton, New Zealand.[21] They have a particular focus on environmental and sustainable education for people of all ages and background. As such, they carry out educational workshops and volunteering events for schools, businesses and the wider community.

The Fairfield project does so alongside their primary activity, the restoration and maintenance of the ecologically significant Kukutaaruhe Gully, for which they rely on the assistance of local volunteers. They serve a diverse community in Fairfield (a suburb of Hamilton) which includes managing several large community gardens and providing community members with opportunities to cultivate their own

[21] http://www.thefairfieldproject.co.nz/

crops. For readers not familiar with New Zealand, we show the location of Hamilton on a map of New Zealand in Figure 9.

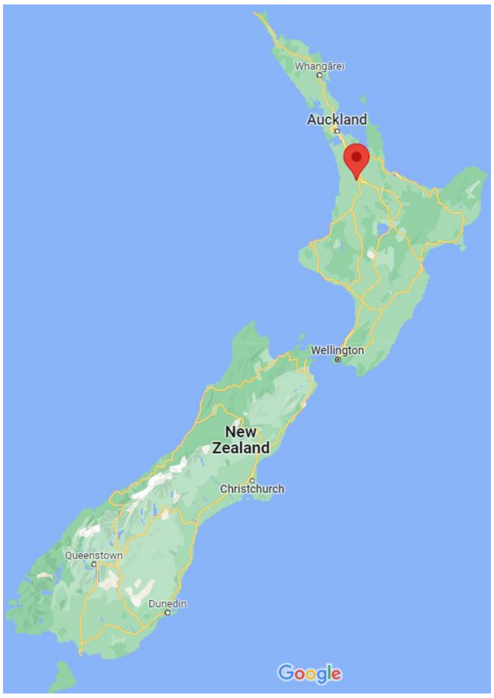

Figure 9. Map of New Zealand with Hamilton highlighted. (Map data © Google)

The Fairfield Project has a consistent base of volunteers but are always in need of more volunteers for various tasks. Like other community nature restoration groups we spoke to, the Fairfield Project find that volunteers tend to be older and that it is very difficult to attract and retain new volunteers. They also expressed the concern that many local residents were unaware of the work they were doing and the opportunities to get involved as a volunteer - a sentiment shared by other community groups and shown in recent research by the Ministry for the Environment (MFE) (2021).

As a result, the Fairfield Project have been incredibly supportive of the field experiment aims and have been our field partners, providing input to the research design and organising and hosting the volunteering events.

3.6.2 Volunteering events

Alongside our partner the Fairfield Project, we organised four volunteering events for people to attend as part of stage one and two of the experimental design. The events were held over a one-week period from the 18th to the 25th of February. There were two events to choose from for those who were offered a voucher and two different events to choose from for those not offered a voucher in stage one.

For the non-voucher groups, volunteering events were held on Saturday the 18th and Monday the 20th of February. For the voucher groups, volunteering events were held on Wednesday the 22nd and Saturday the 25th of February. The Monday event for the non-voucher group was supposed to be on

Wednesday the 15th of February but adverse weather conditions the day before the event meant we re-scheduled it to Monday the 20th of February.

The events all started at 10:00 AM and concluded at 12:00 PM, where a light lunch was provided for attendees and vouchers were handed out (at the relevant events). Fortunately, all four events had similar weather conditions – fine, with a mix of sun and cloud overcast. The temperature ranged from 17ºC to 24ºC throughout the events. Volunteers could choose between several volunteering activities on the day and had two opportunities to select activities during the event. The purpose of this was to cater to a broad range of interests and skills with the hope of increasing enjoyment for those involved.

The initial description of the events we provided in survey one is shown below.

> *"We are looking for volunteers for a series of events with a community restoration group on the eastern side of Hamilton.*
>
> *These short volunteering events will be in the mornings and last around 2 hours. Activities at the volunteering events may include planting, potting, monitoring and trapping and a range of other activities. No prior skills or experience are required for any of the activities. Lunch will be provided for all volunteers, and you may bring household members with you.*
>
> ***Would you be willing to participate in one of these volunteering events sometime in the next month?****"*

The final description with full details and information was given via email after individuals committed to attending the events. These details are shown in Appendix E.

3.6.3 Follow-up events

The main follow-up volunteering event was held a month after the last volunteering event on the 25th of March. This follow-up event was the Fairfield Project's Te Maara Kai O Kukutaaruhe Festival (Garden Festival) which involved a range of volunteering activities and educational workshops. The description of this event that we provided to survey two respondents is shown below:

> *"We are looking for volunteers for a family-friendly volunteering event in March on the Eastern side of Hamilton. There will be opportunities to engage in a range of volunteering activities and attend educational workshops. Also lunch will be provided for all attendees.*
>
> *The event will be in the morning and you will be able to come and go at times that suit your schedule.*
>
> ***I am willing to attend this event if I am available."***

There was also a working bee volunteering event held on the 17th of March and we observed whether participants attended this event.

3.6.4 Monitoring attendance

We monitored attendance at the original four events and the follow-up events using sign-in sheets from the Fairfield Project. As is standard at all Fairfield Project volunteering events, attendees need to listen to a health and safety briefing and then sign-in to the site. Our research team managed these sign-in sheets at the four volunteering events and the follow-up events, informing participants that the sheet would be used only for health and safety purposes and to track whether they attended an event as part of our research study. This approach to tracking participants is covered in our ethics approval.

Chapter 4: Stage One Methods and Results

4.1 Introduction

This chapter presents the empirical analysis for stage one of our field experiment. As a reminder, stage one is primarily concerned with evaluating the effects of a nudge, voucher and nudge and voucher combined on willingness to volunteer. In-depth details about the experimental design can be found in the previous chapter (Chapter 3).

Shifting individual behaviour is an important tool for addressing environmental issues like climate change and environmental degradation. Indeed, there has been extensive work investigating the drivers and levers of pro-environmental behaviours (PEBs) (for recent examples, see Bonan et al., 2021; Carlsson et al., 2021; Zemo & Termansen, 2022). However, there have been calls for researchers to allocate more resources to understanding PEBs that relate to nature and biodiversity, an area that has been less of a focus historically in the literature (Nielsen et al., 2021). Moreover, across the PEB and wider behaviour change literature, there has been a lack of focus on the behaviours that matter most for the end outcomes of interest (in our case, environmental outcomes - Al-Ubaydli, List, & Suskind, 2017; Grilli & Curtis, 2021; Nielsen et al., 2021). More often than not, studies focus on behaviours that are easy to measure and monitor, which has meant an abundance of PEB research relating to some behaviours (like water and energy consumption) and a shortage of research on others (Brent et al., 2017).

We add to the literature by presenting the analysis of stage one of our field experiment focussed on increasing volunteering for nature restoration groups in urban areas. Volunteering for nature restoration groups is an impactful behaviour (in terms of environmental outcomes) that few people are engaged in, has been under-studied in the literature and creates significant benefits for society and the volunteers themselves (see Chapter 1 for more on these points). Moreover, we focus on the urban population because few studies focus on behaviours for biodiversity conservation and even fewer study them in an urban context (Brent et al., 2017; Truelove et al., 2014). This is a significant gap because most people live in urban areas and urban populations tend to be less connected with nature on average (Rosa & Collado, 2019). Furthermore, research shows that New Zealander's living in urban areas carry out fewer PEBs relating to freshwater health than people living in rural areas (Ministry for the Environment, 2021).

Moreover, we add to the growing literature on the impacts of financial incentives on pro-environmental behaviour (Ling & Xu, 2021; Sloot & Scheibehenne, 2022). In general, there are mixed results, and there are many papers that study the potential crowding-in and crowding-out effets of financial incentive on intrinsic motivation (Frey & Jegen, 2001; Rode et al., 2015; Sloot & Scheibehenne, 2022). We also add to the literature on the synergies between nudges and financial incentives, where again there are mixed and conflicting results in the literature. Some studies suggest that nudges can detract from the effects of financial incentives and vice versa, while other suggest there are reinforcing synergies or no synergies (Drews et al., 2020; Fanghella et al., 2021). By evaluating the effects of a financial incentive alone, a nudge alone and a voucher and nudge combined on volunteering behaviour, we add to both of these literatures.

We start the chapter by presenting the main pre-registered hypotheses for stage one and a set of additional non-pre-registered hypotheses. We then discuss the data, data cleaning and variables for the subsequent analyses. Next, we present full details of our methods, which includes hypothesis testing and regression modelling to evaluate the stage one hypotheses. We then describe our methods to explore the heterogeneity in the effects of offering a voucher on volunteering behaviour. This is motivated by predictions from our theoretical model and previous work showing financial incentives often have heterogeneous effects (U. Gneezy et al., 2011; Gravert & Olsson Collentine, 2021; Ling & Xu, 2021). Following the methods, we present the results of our analyses and an assessment as to whether each of our hypotheses are supported by the data. We round the chapter out with the voucher treatment effect heterogeneity results and an overarching conclusion.

4.2. Hypotheses

4.2.1. Stage one main hypotheses

For stage one of our experiment, the overall research question (see Chapter 3) is, can we use a nudge or supermarket voucher or both to encourage first time volunteering? In relation to this question, we pre-registered the following hypotheses:

- ***H1.1.*** *All three treatments will increase the likelihood of volunteering (relative to the control).*
- ***H1.2.*** *The voucher conditions will be more effective at increasing the likelihood of volunteering than the information treatments (nudge and control).*
- ***H1.3.*** *The combined treatment will be better than the nudge or voucher treatment separately at increasing the likelihood of volunteering.*

The intuition behind these hypotheses come directly from our Chapter 2 and the literature on the efficacy of both financial incentives and nudges (Bénabou & Tirole, 2006; Carlsson et al., 2021). In our experimental design chapter (Chapter 3), we outlined how the nudge and voucher treatments should individually influence the expected net benefits of volunteering and thus volunteering behaviour.

We evaluate willingness to volunteer at three levels: precommitment, commitment and attendance (see Chapter 3 for more on this).

4.2.2. Stage one additional questions

We then also developed two further research questions relating to variables that were not originally covered in our pre-registration. These questions are:

Does treatment status in stage one affect:

- Observed donation behaviour (see Chapter 3) *and*
- Attrition (whether or not an individual decides to complete survey two)

We are interested in whether our treatment group assignment (specifically, voucher assignment) affects donation behaviour and the likelihood of participating in our second survey.

Understanding the immediate effects of being offered a financial incentive to engage in pro-social behaviour is pertinent, given the literature on the potential crowding out effects of financial incentives (Bowles & Polania-Reyes, 2012; Frey & Jegen, 2001).

Moreover, responding to survey two is an outcome in and of itself, because individuals have to complete a survey which is costly in terms of time. Thus, it is important to know whether being offered a voucher (or being in a different treatment group) affects the likelihood of participating in future surveys. For example, if the voucher crowds out intrinsic motivation for volunteering or other pro-environmental behaviours, we may expect to see fewer responses to a follow-up survey about said behaviours.

In addition, understanding whether treatment status in stage one affects attrition will be important when we analyse stage two of the field experiment (in the following chapter).

4.2.3. Stage one additional hypotheses

For both additional questions, we had no clear predictions on the direction of any effects because of the presence of competing theories.

Voucher impact on donations

We are interested in whether being offered a voucher to volunteer affects willingness to donate (on the extensive and intensive margins). We had no clear hypothesis ahead of time because being offered a voucher could have created “gift-exchange” effects where respondents felt the need to give back by donating some of their potential survey reward (Falk, 2007).

On the other hand, being offered a voucher may have crowded out willingness to donate for two reasons:

Individuals incorrectly link the voucher offer to the environmental organisations. They then believe that the environmental organisations have more resources than they actually do and feel there is less need for donations.

Being offered a voucher crowds out intrinsic motivation to donate to environmental organisations – we tried to minimise this during our framing of the voucher (see Chapter 3).

Thus, our additional hypothesis on donation behaviour is non-directional:

H1.A1. *Being offered a voucher will affect donation behaviour.*

Attrition

Again, we are interested in whether being offered a voucher affects willingness to engage in future surveys. For the same reasons as above, we do not have clear *a priori* hypotheses as to which way the effect may go.

On the one hand, there could be gift exchange effects or people might think they are going to be offered another voucher (which would lead to a higher response rate for the voucher group). On the other hand, being offered a voucher could crowd out intrinsic motivation for filling in an environmentally focussed survey.

Hence, our hypothesis around attrition is:

H1.A2. *Treatment status in stage one will affect the response rate to survey two.*

4.3. Theory

In stage one, treatment status is randomly assigned through survey one. Upon answering all demographics and environmental attitudes questions, respondents are asked whether they would like to pre-commit to a volunteering event. Based on treatment group status, individuals receive variations of the question about pre-committing to an event. We show the variations in the question below, along with some theoretical explanation for each treatment condition.

4.3.1. Control group

All respondents receive the same baseline information with details about the length and type of event (the baseline information is shown in Chapter 3). This baseline information forms the conditions for the control group.

In line with our theoretical model, the baseline information may still prompt a shift in volunteering behaviour (relative to those with no information) because the descriptive information may help reduce some of the uncertainty and inaccuracy around what nature volunteering is like. We can approximate whether the information alone has an impact on volunteering by simply looking at whether individuals in the control group volunteer at any of the events. This is an attractive feature of our design because our sample only includes those who are not already engaged in nature volunteering. Hence, any uptake of nature volunteering can be seen as additional relative to the status quo business as usual (BAU) scenario.

4.3.2. Nudge

For the nudge treatment group, the following statements were added to the baseline information:

> *"Participating in one of these events is a great way to give back to your community and the environment while having fun! It is also a good way to meet like-minded people. Studies show that volunteering increases overall wellbeing. You might also learn some new skills that you can apply at home or in your local neighbourhood to positively impact the environment."*

The statements are designed to make environmental and social benefits more salient when individuals make their decision to pre-commit to an event. Like the control group, the nudge also aims to highlight information that may reduce the uncertainty and inaccuracy of prior beliefs about volunteering. The nudge differs to the control by specifically referencing the social and environmental benefits of volunteering, which engages individuals pro-environmental and pro-social motivations.

4.3.3. Voucher group

For the voucher treatment group, individuals are informed that they will receive a one-off $50 NZD supermarket voucher for attending. This is equivalent to 1.3 hours of work based on the national average hourly earnings during the March 2023 quarter (Stats NZ, 2023). The following statement was added to the control brief:

> *"To recognise volunteers' time commitment and willingness to try something new, volunteers will receive* ***a one-off $50 supermarket voucher*** *at the event. Please note that we can only provide one voucher per household."*

The voucher is not meant to fully compensate individuals for their time. Rather, we see the voucher as a way of helping individuals experiment with a new and uncertain behaviour. We carefully frame the voucher in such a way to reduce the risk of crowding out of intrinsic motivation (U. Gneezy et al., 2011).

The intuition behind the voucher follows our theoretical model – if we can introduce an incentive that shifts the net benefits function into strictly positive territory, we can encourage initial behaviour uptake. The basic idea is that individuals who are uncertain can be confident they will benefit from the experience, even if they do not enjoy the volunteering (because they will be receiving a voucher regardless). We depict this in Figure 10. Line two represents the individual's initial inaccurate and uncertain beliefs with high adjustment costs. If an incentive of value X is offered alongside some basic information, the net benefits function shifts into strictly positive territory in line three. Now, even if the experience is at the lowest end of the individual's expectations (at point A), the net benefits are still positive. And of course, if the experience is at the upper end of the individual's expectations, the net benefits are strongly positive (point B in Figure 10).

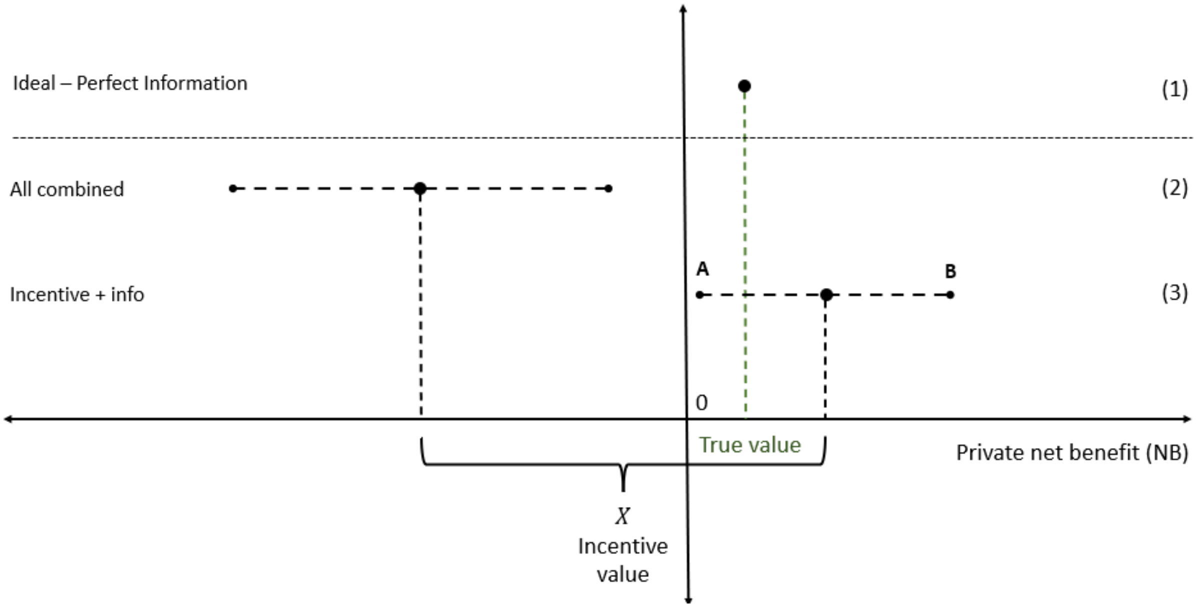

Figure 10. Theoretical model predictions of the effects of a voucher on the expected private net benefits from volunteering

4.3.4. Combined treatment

The combined treatment group receives the baseline information, nudge and offer of a supermarket voucher. The reason for having this group is to test the effect of combining the nudge with the voucher and vice versa. There is mixed evidence on whether nudges and related behavioural interventions enhance, diminish or have no impact on the efficacy of a financial incentive (Al-Ubaydli, List, LoRe, et al., 2017; Ling & Xu, 2021).

4.4. Data

In this section, we will briefly covers details of the data, data cleaning process and the variables for the analyses in this chapter.

4.4.1. Data overview

The data for stage one are from several sources, including survey one (this is the main source of data), the commitment surveys, attendance sheets from the volunteering events and survey two (to track attrition).

Survey one received high engagement, but there was a large number of incomplete surveys. In total, 1,497 surveys were started, and we ended up with a total usable sample of N = 757 (this includes those who are already volunteering). As per Chapter 3, we excluded individuals who were strongly opposed to volunteering, under 18 years of age, were not the first household member to complete our survey and did not live near Hamilton. We also dropped responses that were less than 75% complete. Of the 757 respondents, 130 were already volunteering for nature restoration groups and 627 were classified as "first-time" volunteers. This sample of first-time volunteers (N = 627) is the sample of interest for stage one.

4.4.2. Demographics variables

We collected data on a range of demographics and have reported these below:

- **Relative Income Class**: high, medium, low – self-reported perceptions
- **Age**: numeric
- **Ethnicity**: Māori and Pacifica dummy variable
- **Gender**: male dummy variable, base category is females and gender diverse
- **Any children**: dummy variable if individual has at least one child 17 yrs or under
- **Young children:** dummy variable if individual has at least on child 13 yrs or under
- **Education**: dummy variable for bachelor's or higher (university-educated)
- **Geographic location**: split into "Fairfield and surrounding suburbs", "Rest of Hamilton City" and "outside of Hamilton City". See Appendix F for details on how these groups were defined.
- **Employment status:** dummies for full-time, part-time, student and retired.
- **Other volunteering:** dummy for those who engage in other voluntary activities at least once a month.

4.4.3. Attitudinal, wellbeing and identity variables

We also collected data on the following variables (which are described more fully in Chapter 3):

- **Environmental identity (EID)**: Three-item index, seven-point Likert scale
- **Environmental locus of control (LOC):** Five-item index, seven-point Likert scale
- **Short-term Who-5 wellbeing**: Five-item score, six-point Who-5 scale
- **Willingness to volunteer (restoration):** seven -point Likert scale
- **Knowledge of restoration groups**: seven-point Likert scale
- **Perceptions of restoration groups**: seven-point Likert scale
- **Connectedness to nature:** seven-point Likert scale
- **Connectedness to community**: seven-point Likert scale
- **Pro-environmental behaviour (PEB):** six-item index, 7-point Likert scale
- **Donation behaviour:** dummy variable for donating and variable denoting amount[22]

[22] For the donation question, some respondents elected not to provide an answer (they ended the survey early). We coded these individuals as not willing to donate.

We do not include all variables in every model due to multicollinearity issues, which we discuss in later sections.

4.4.4. Availability variable

For those who pre-committed to attend a volunteering event over the following month, we also gathered information on their general availability to attend events. Immediately following pre-commitment, we asked individuals to select any dates over the upcoming four weeks (from an on-screen calendar) where they were likely to be available to attend a volunteering event between the hours of 10:00 AM and 12:00 PM. Please see Appendix A for the full survey question. This gave us a count variable for the number of days each pre-committed individual was available (taking values between 0 and 28).[23] We use this in stage one and stage two to proxy for general availability.

4.5. Methods

In this section, we show our methods for analysing stage one of the field experiment. We start with the methods for assessing whether randomisation was successful, then we show how we calculate our average treatment effects and conduct our main hypothesis testing. Next, we discuss our regression modelling to test certain hypotheses and finish by presenting the methods for our exploration of voucher treatment effect heterogeneity.

4.5.1. Formal assessment of randomisation

We will start by assessing balance between our four groups (the control and three treatment groups). For our pre-registered hypothesis testing, we rely on the assumption that treatment status is randomly assigned and is thus independent of potential outcomes (Rubin, 2005). That is, observables and unobservables are balanced on average between our treatment groups and there is no selection bias. As we have multiple treatment groups, to assess whether randomisation was successful, we estimate multinomial logistic (logit) regression models to predict treatment status (McCaffrey et al., 2013). We estimate a full model with all of our covariates (complete model) and a model with intercepts only (empty model).

To assess covariate balance, we will first examine the full regression output, noting cases where regressors are statistically significant. We will then compare the Akaike information criterion (AIC) between the complete and empty models, to see whether our covariates improve model fit. Finally, we

[23] Some respondents (N~10) accidentally skipped forwards and could not return to the calendar. However, in general, these individuals informed us of this and told us qualitatively which days they would be available. Hence, we manually coded availability for these individuals. If they told us they skipped forwards by mistake and did not indicate their availability, we assigned them the median value.
It is also worth noting that we capped the variable at 28 days (because we asked about the following four weeks).

will perform a likelihood ratio (LR) test to compare the models and see whether adding covariates improves predictive power (McFadden, 1987).

4.5.2. Calculating treatment effects

Assuming treatment status in stage one is effectively randomised, we can estimate average treatment effects (ATEs) by taking the difference between mean outcomes in the treated and control groups:

$$ATE = E[Y_i|Z_i = T] - E[Y_i|Z_i = C] \tag{10}$$

where Y_i is the outcome of interest (volunteering, donation behaviour or attrition), Z_i is treatment status, T represents the "treatment" group(s) and C represents the "control" group(s). We use T and C notation because the treatment and control groups may change based on the specific hypothesis being tested. For example, to examine the effects of only the nudge on outcome variable Y_i, the treatment would be the nudge group and the control would be the baseline control group. On the other hand, if we were assessing the effects of being offered a voucher on outcome variable Y_i, the treatment would be both voucher groups (voucher only and combined) and the control would be the other two groups (nudge and baseline control).

4.5.3. Chi-squared hypothesis testing

To test our three main stage one hypotheses (H1.1, H1.2, and H1.3) and our two additional hypotheses (H1.A1 and H1.A2) we use non-parametric chi-squared hypothesis tests (McHugh, 2013). These tests do not assume any particular distribution for the parameters of interest and do not impose homoskedasticity assumptions on the data (McHugh, 2013). This is also in-line with our pre-registration.

We will start by assessing overall differences between treatment groups using bi-directional chi-squared tests. This will inform us of whether there is any difference in our outcomes of interest between the four groups but will not evaluate where those differences come from.

We will then conduct specific chi-squared tests to evaluate the full set of hypotheses laid out in Section 4.2. These chi-squared tests will be one-sided for our main hypotheses (as these were pre-registered one-sided hypotheses) and take the following format:

$$H_0: E[Y_i|Z_i = T] \leq E[Y_i|Z_i = C] \tag{11}$$

$$H_1: E[Y_i|Z_i = T] > E[Y_i|Z_i = C]$$

where the null hypothesis (H_0) is that outcome Y_i in the specified treatment group (T) is on average less than or equal to the average outcome in the control group (C). Equivalently, we are testing the alternative hypothesis that the ATE is greater than zero.

$$H_0: ATE \leq 0 \tag{12}$$

$$H_1\colon ATE > 0$$

Conversely, for our additional hypotheses, we will run two-sided chi-squared tests where the null and alternative hypotheses take the following format (in terms of ATEs):

$$H_0\colon ATE = 0 \tag{13}$$

$$H_1\colon ATE \neq 0$$

where the alternative hypothesis is that the average ATE is not equal to zero.

4.5.4. Regression models

In line with our pre-registration, we also run a series of regression models to evaluate the impacts of our treatments on our main outcomes of interest (volunteering behaviour measured by pre-commitment, commitment and attendance).

We will use linear probability models (LPMs) to estimate the marginal effects of our treatments on commitment and attendance. We discuss the use of LPMs and non-linear alternatives in Appendix G. The results are very similar when using logistic regression and computing average marginal effects post-estimation so we proceed with LPMs.

The value of our regression models is that we can:

- control for a range of demographic and attitudinal characteristics,
- compare the treatment groups all in one model,
- evaluate which other characteristics predict willingness to volunteer, *and*
- look at the effects of our treatments on attendance after controlling for commitment.

The final point is particularly interesting because it tells us whether the treatments shift behaviour by encouraging people to a) commit, b) attend after committing or c) both. However, the chi-squared hypothesis testing is still our main approach for evaluating the stage one hypotheses.

The basic format for the regression models we will estimate is as follows:

$$Y_i = \beta_0 + \boldsymbol{Z_i}\boldsymbol{\delta} + \boldsymbol{X_i}\boldsymbol{\theta} + \varepsilon_i \tag{14}$$

where Y_i is the binary outcome of interest (pre-commitment, commitment or attendance), $\boldsymbol{Z_i}$ is a vector of treatment dummies, $\boldsymbol{X_i}$ is a vector of controls[24] and ε_i is the idiosyncratic error term. We use Huber-White robust standard errors when estimating these models (H. White, 1981).

[24] Which includes commitment for our attendance outcome variable in one of the specifications and also includes availability in some specifications.

4.5.5. Methods for exploring the heterogeneity in voucher treatment effects

Our theoretical model in Chapter 2 implies that offering a voucher will likely have heterogeneous effects on individuals' volunteering behaviour. This is because individuals have different expected net benefit functions based on their observable characteristics and this will impact whether the voucher is sufficient to shift behaviour. Indeed, our theoretical model predicts all three treatments will likely have heterogenous effects, but we focus on the voucher because it is the more substantial treatment (as opposed to the nudge, which is a minor change to the information provision) and comparing two combined groups (voucher vs non-voucher) will give us a larger sample size to explore heterogeneity. Moreover, the presence of heterogeneity in response to financial incentives is a common finding in the literature. For example, Ling & Xu (2021) show that offering an incentive to recycle crowds out behaviour to a greater extent for those with high pre-existing pro-environmental attitudes. Moreover, recent review papers show that incentives tend to have larger heterogeneity in their effects which depend on individual characteristics, the context and the incentive design (U. Gneezy et al., 2011; Sloot & Scheibehenne, 2022). As such, there is strong motivation from the literature and our theoretical model to explore heterogeneity further.

To explore heterogeneity in the effects of the voucher on volunteering behaviour, we use methods described by Nilsson et al. (2019) to estimate expected treatment effects for each individual (their expected individual treatment effect, or ITE). Like the authors, we use this method to explore the heterogeneity and potential determinants of such heterogeneity in the voucher treatment effects. For the purposes of this paper, an exploratory approach to investigating heterogeneity is sufficient and we hope the results provide useful suggestions for future researchers and policymakers. Future researchers may also want to conduct further robustness checks on these results and analyse the data with more complex machine learning methods (Athey & Wager, 2019).

The method follows the potential outcomes framework (Rubin, 2005) and is computationally equivalent to a single imputation approach (Nilsson et al., 2019). The premise is that each individual has two potential outcomes: Y_{1i}, which is the theoretical outcome when individual i is treated with a voucher ($Z = 1$) and Y_{0i}, which is the outcome when individual i is untreated ($Z = 0$). We only observe one of these theoretical outcomes for each individual based on which group they are randomly assigned to ($Z = 1 \text{ } or \text{ } 0$). The counterfactual, or unobserved outcome, is what we need to estimate the expected ITEs, which take the form of $Y_{1i} - Y_{0i}$ – outcome in the treated state less outcome in untreated state. The two potential outcomes and the ITEs will vary based on observable individual characteristics and this is the heterogeneity we explore with this method.

Statistical approach

Nilsson et al. (2019) propose modelling each potential outcome explicitly and examining the difference between these modelled potential outcomes. This approach yields consistent estimates when we assume that treatment status is independent of potential outcomes, $(Y_{0i}, Y_{1i} \perp Z_i)$ which is what we find in our randomised setting (Nilsson et al., 2019).

The regression equations for each of the two potential outcomes take the following form:

$$Y_{0i} = \alpha_0 + \sum_{k=1}^{K} B_{k0} X_{ki} + \varepsilon_{0i} \tag{15}$$

and

$$Y_{1i} = \alpha_1 + \sum_{k=1}^{K} B_{k1} X_{ki} + \varepsilon_{1i} \tag{16}$$

where X_{ki} represent the full set of k observable covariates for individual i, the α term represents the intercepts and the ε terms represent the idiosyncratic error terms. It is important to note that we estimate the first equation with the sample who are untreated ($Z = 0$) and the second equation with the sample that are treated ($Z = 1$).

To obtain the expected individual treatment effects (ITEs), we take the expected difference between the two equations above to yield:

$$E[ITE|X_{ki}] = E[Y_{1i} - Y_{0i}] = \Delta\alpha + \sum_{k=1}^{K} (\Delta B_k) X_{ki} \tag{17}$$

where the error terms disappear when we assume the error term has mean zero, conditional on observable covariates. We can see that heterogeneity in expected ITEs are driven by the differential influence of covariates X_{ki} in the treated and untreated states.

To obtain the ITEs in Equation 17, we estimate the two equations for the potential outcomes and then predict both potential outcomes for each individual $(\widehat{Y_{1\iota}}, \widehat{Y_{0\iota}})$. We then subtract $\widehat{Y_{0\iota}}$ from $\widehat{Y_{1\iota}}$ to obtain each individuals expected ITE.

Exploring heterogeneity in ITEs

Once we estimate expected ITEs for every individual in the sample, we explore the heterogeneity in the ITEs. In their original paper, Nilsson et al. (2019) suggest regressing the ITEs on covariates of interest, to see how different covariates affect the ITE estimates. However, this requires *apriori* knowledge as to which factors will likely drive heterogeneity in treatment effects. In our case, we did not pre-register any strong hypotheses around which variables would drive heterogeneity in treatment effects.

Moreover, we do not want to include all observable covariates in a regression predicting ITEs because that would perfectly identify the ITEs (as they are a function of the full set of observable covariates – see Equation 17).

Instead, we let the data tell us which variables are important and then explore heterogeneity driven by these variables. We do this by running simple regressions of the ITEs on each covariate individually, reporting the explanatory power (R^2) for each covariate from the simple regression case. We use this to rank covariates in terms of their importance for predicting ITEs and select the top five covariates to explore further. We select the top five variables because these on average explain almost ¾ of the total heterogeneity. The intuition and theory behind identifying the variables that matter most for the heterogeneity is similar to more advanced machine learning methods (Athey & Wager, 2019). While our approach of selecting the top five variables is relatively simple, it is appropriate for this exploratory analysis of heterogeneity.

4.6. Results and Discussion

4.6.1. Descriptive statistics - demographics

In Table 1, we report the demographic summary statistics for our overall sample in stage one. We also delineate between our "first-time" volunteers sample and those already volunteering.

Before we go any further, we note that our sample is not meant to be representative of the New Zealand population. Rather, our sample is aimed at being representative of those living near an urban centre with at least a minor interest in volunteering for a restoration group. This is reflected in our survey screening criteria (see Chapter 3).

Our overall sample is roughly representative of the New Zealand population on age and ethnicity. Our sample average age is 43 years old and the New Zealand median age is approximately 37 years old (Stats NZ, 2021b). However, the median age statistics include those under the age of 18 and our data does not – hence, we see a small difference in central measures of age. In terms of ethnicity, approximately 24.5% of New Zealand residents identify as either Māori or Pacific, aligning closely with our data (23% Māori or Pacific).

Our sample is also highly female-dominated, with 71% of survey one respondents being female. 1% of our sample reported being gender diverse. Our sample tends to be well-educated, with 58% of respondents having attained at least a bachelor's level education (this is higher than the overall population, where 30% had obtained at least a bachelor's level education in 2014 - Ministry of Social Development, 2016). Most respondents classify themselves as middle income and few classify themselves as high income. Half of respondents work full time, 16% work part-time, 11% are retired, 7% are students and the rest are in some other form of employment (or unemployed). Most respondents reside within Hamilton City and around a third of respondents have at least one dependent child. Most respondents never or infrequently volunteer elsewhere.

Those already volunteering are more likely to be male and gender diverse, less likely to be Māori or Pacific, more likely to live outside of Hamilton City and are less likely to have a dependent child.

Table 1. Demographics summary statistics

	Full sample (N = 757)		First time volunteers (N = 627		Already volunteering (N = 130)	
Variable	Mean	St. Dev.	Mean	St. Dev.	Mean	Std. Dev.
Age	43	16	43	16	45	16
Māori and Pacific Ethnicity	23%	-	24%	-	16%	-
Bachelors or higher	58%	-	56%	-	66%	-
Income (perceived)						
Low	24%	-	25%	-	18%	-
Middle	63%	-	63%	-	65%	-
High	12%	-	12%	-	16%	-
Gender						
Female	71%	-	74%	-	56%	-
Male	28%	-	26%	-	40%	-
Gender diverse	1%	-	1%	-	4%	-
Employment status						
Full time	50%	-	50%	-	50%	-
Student	7%	-	7%	-	6%	-
Retired	11%	-	11%	-	12%	-
Part time	16%	-	15%	-	17%	-
Other employment	18%	-	18%	-	16%	-
Geographic location						
Resides outside Hamilton City	16%	-	15%	-	24%	-
Resides near Fairfield	17%	-	17%	-	15%	-
Children						
Has a child	35%	-	36%	-	30%	-
Has a child under 14 yrs	29%	-	30%	-	23%	-
Other volunteering behaviour						
Never volunteers elsewhere	38%	-	41%	-	24%	-
Infrequently volunteers elsewhere	41%	-	39%	-	52%	-
Sometimes volunteers elsewhere	11%	-	11%	-	10%	-
Frequently volunteers elsewhere	10%	-	9%	-	15%	-

4.6.2. Descriptive statistics – attitudinal, wellbeing and identity variables

In Table 2, we report the summary statistics for the remaining variables common to both our "first-time volunteers" sample and our "already volunteering" sample. Donation behaviour and responding to survey two are considered outcome variables for stage one. The other variables are controls. Those already volunteering tend to score higher on average on all measures of pro-environmental attitudes, identity and behaviour. This is suggestive of a correlation between volunteering and pro-environmental attitudes and behaviours – a common finding in the literature Ganzevoort & van den Born, 2020). Stage two will extend this literature by estimating the casual of volunteering on these outcomes.

In Appendix H, we present graphs of the distributions of some of the variables in Table 2 for first-time volunteers and those already volunteering. Generally, there is more variability in perceptions and attitudes for our first-time volunteers than there are for the sample of existing volunteers.

Table 2. Summary statistics for attitudinal, identity and wellbeing variables.

	Full sample (N = 757)		First-time volunteers (N = 627)		Already volunteering (N = 130)	
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
EID scale	5.6	0.93	5.5	0.93	5.9	0.9
Locus of control scale	5.2	0.92	5.2	0.91	5.4	0.92
Who5 index	14	5.1	14	5	16	5
Willingness to volunteer (restoration)	5	1.3	4.8	1.3	5.9	1.1
Knowledge of restoration groups	3.1	1.6	2.9	1.5	4.2	1.7
Perceptions of restoration groups	5.4	1.2	5.3	1.2	5.8	1.1
Connection to nature	5.8	1.1	5.7	1.2	6.1	0.85
Connection to community	4.3	1.5	4.3	1.5	4.7	1.4
PEB Scale	4.9	0.9	4.9	0.9	5.1	0.9
Donation percentage	51%	-	50%	-	58%	-
Donation value	$24	$27	$22	$27	$29	$29
Responded to survey two	74%	-	71%	-	87%	-

Assessing the validity of our index variables

We assess the internal validity and consistency of our environmental attitudes, identity and wellbeing scales by calculating and evaluating Cronbach's alpha for each scale (Cronbach, 1951). The generally agreed upon heuristic is that a scale with a Cronbach's (or coefficient) alpha of 0.7 or above has good internal consistency and reliability (Cortina, 1993). However, a scale with an alpha between 0.6 and 0.7 is considered acceptable (van Griethuijsen et al., 2015).

In Table 3, we can see that our Who-5 wellbeing index, EID scale and environmental LOC scale have strong internal consistency. On the other hand, our PEB scale has a relatively lower internal consistency, which is likely due to the wider spread of behaviours (or topics) covered by the items in the PEB scale – this was also noted by some of the survey respondents.

However, in line with other researchers (Pisano & Lubell, 2017; Tam & Chan, 2017), we retain our PEB scale despite its low Cronbach's alpha because the scale is uni-dimensional, each item is face-valid and the average inter-correlation between items is 0.15, which is within the acceptable region delineated by Clark & Watson (1995). Moreover, removing items from the scale does not substantially improve the Cronbach's alpha, so we leave the scale as is.

Table 3. Cronbach's alpha scores for index variables

Scale name	No: items	Cronbach's α	Reference
Who5 Wellbeing Index	5	0.87	See Topp et al. (2015).
Environmental Identity (EID) Scale	3	0.90	See van der Werff et al. (2013)
Environmental Locus of Control (LOC) Scale	5	0.82	Adapted from Cleveland et al. (2012)
Pro-Environmental Behaviour (PEB) Scale	6	0.57	Adapted primarily from Blok et al. (2015)

Note: Please see Chapter 3 for details on how each scale was developed.

4.6.3. Assessing randomisation

In Table 4, we report demographic summary statistics for each treatment group and observe good balance overall.

Moreover, we assess whether randomisation was successful formally using a multinomial logit model to predict treatment status. We include all of our demographic control variables (16 coefficients in total) and the results are reported in Appendix I. Of the 48 estimated coefficients, only five are significant at the 10% level, which is what we would expect to see by chance alone. With any conservative corrections for multiple hypothesis testing, we find no significant coefficients. This indicates that our demographics have no true predictive power over treatment assignment.

We confirm this by running a second multinomial logit model with intercepts only (no regressors or explanatory variables). We find that the Akaike Information Criteria (AIC) is lower (indicating a better model fit) for the model with no variables (1,742.7) than the model with our full set of controls (1,779.8). This is also shown in Appendix I. Finally, an LR test comparing the complete and empty models reveals that the covariates jointly are non-significant in predicting treatment status (p-value of 0.136).

Evidently, we can conclude that our treatments were successfully randomly assigned and proceed to our chi-squared hypothesis testing and regression results.

Table 4. Demographic summary statistics by treatment group.

Variable	Control (N = 145)	Nudge (N = 154)	Voucher (N = 161)	Combined (N = 167)
Age	44	43	40	42
Māori and Pacific Ethnicity	22%	25%	27%	22%
Bachelor's or higher	57%	60%	58%	51%
Income (perceived)				
Low income	26%	16%	30%	29%
Middle income	62%	66%	63%	61%
High income	12%	18%	7%	10%
Gender				
Female	72%	70%	76%	75%
Male	27%	28%	24%	24%
Gender diverse	1%	2%	0%	1%
Employment status				
Full time	54%	52%	44%	50%
Student	3%	5%	13%	6%
Retired	10%	10%	9%	13%
Part time	15%	17%	17%	12%
Other employment	18%	16%	18%	19%
Geographic location				
Resides outside Hamilton City	14%	18%	12%	14%
Resides near Fairfield	19%	14%	17%	19%
Children				
Has a child	39%	32%	42%	33%
Has a child under 14 yrs	32%	25%	34%	28%
Other volunteering behaviour				
Never volunteers elsewhere	41%	45%	37%	40%
Infrequently volunteers elsewhere	37%	36%	43%	41%
Sometimes volunteers elsewhere	14%	9%	11%	12%
Frequently volunteers elsewhere	8%	10%	9%	7%

Note: We do not report standard deviations for brevity and because the standard deviation for proportions can be readily calculated using the values in the table.

4.6.4. Overall chi-squared tests

We start by presenting the overall bi-directional chi-squared test results in Table 5. These results show whether there are any differences between treatment groups in our outcomes variables and are used for our hypothesis testing, which is summarised towards the end of the chapter. We find that treatment assignment makes no difference to precommitment rates. However, there are significant differences between the treatment groups for commitment and attendance. We also find that there are differences between the groups at a 10% level in willingness to donate. Finally, there are no differences in completion rates for survey 2.

It is also notable that in both the control and nudge groups, we see "increases" in the probability of committing to and attending a volunteering event. We use the word "increases" because everyone in in these treatment groups have a baseline of not volunteering (a criterion for inclusion is that they are first-time volunteers). As such, if we take not volunteering as the average baseline, our results suggest that the control and nudge groups which promote information provision are effective for some individuals at encouraging nature volunteering.

Table 5. Overall chi-squared tests for differences in stage one outcomes.

	Mean for treatment groups					
Variable	Control	Nudge	Voucher	Combined	χ^2	p-value
Pre-commitment	49.0%	46.1%	50.3%	48.5%	0.578	0.901
Commitment	7.6%	9.7%	12.4%	19.8%	12.234	0.0066***
Attendance	4.8%	5.2%	8.7%	14.4%	12.146	0.0069***
Donation	54.5%	53.9%	41.0%	49.7%	7.291	0.063*
Survey 2 complete	69.7%	67.5%	70.2%	75.4%	2.663	0.447

Note: These are chi-squared tests with 3 df and a sample size of 627. Sample sizes are N = 145 for the control, N = 154 for the nudge group, N = 161 for the voucher group and N = 167 for the combined group.

4.6.5. Voucher vs non-voucher chi-squared results

We present the following results to examine differences in outcome variables between the voucher and non-voucher treatment groups. Table 6 reports chi-squared proportion tests (either one-sided or two-sided depending on whether the hypothesis was pre-registered) for differences between the voucher and non-voucher groups in terms of willingness to volunteer, donation behaviour and survey two completion.

We find that being offered a voucher significantly increases the probability of both committing to and attending a volunteering event. The ATE of the voucher on commitment is 7.5%, which is an 86% increase in the probability of committing to a volunteering event. The ATE of the voucher on attendance is 6.6%, which is a more than doubling of the probability of attending a volunteering event (132% increase).

Consistent with the overall chi-squared tests earlier, there are no differences between voucher and non-voucher participants in terms of pre-commitment rates and survey 2 completion rates.

Being offered a voucher also significantly reduces the probability of donating to an environmental organisation *immediately after* being offered the voucher.[25] This initially raised concerns about the potential crowding out effects of offering a financial incentive to try volunteering for nature restoration

[25] However, the voucher did not effect the value of the donation, given a donation decision is made.

groups. However, when we examined donation behaviour in survey two (measured in exactly the same way), there were no statistically significant differences between those in the stage one voucher and non-voucher groups (Appendix J). This suggests that any negative spillover effects are both minor (small effect size) and short-lived.

Table 6. Voucher vs non-voucher chi-squared test results

	Mean for treatment groups		*Test details*		
Variable	Non-voucher	Voucher	χ^2	p-value	Hypothesis
Pre-commitment	47.5%	49.4%	0.226	0.317	One-sided
Commitment	8.7%	16.2%	7.911	0.0025***	One-sided
Attendance	5.0%	11.6%	8.721	0.0016***	One-sided
Donation	54.2%	45.4%	4.795	0.0286**	Two-sided
Survey 2 complete	68.6%	72.9%	1.402	0.236	Two-sided

Note: These are chi-squared proportion tests with a sample size of 627. Sample sizes are N = 299 for the non-voucher group and N = 328 for the voucher group. The first three rows are one-sided tests in-line with our pre-registered hypotheses. The final two rows report on two-sided tests because we did not pre-register hypotheses for these variables.

4.6.6. Combined vs individual treatments chi-squared tests

We use the following results to compare the effects of the individual treatment conditions (voucher and nudge) with the combined treatment. In Table 7, we compare willingness to volunteer in the combined treatment group with those in the nudge group and voucher groups respectively. We find that the combined treatment is better than both the nudge and voucher alone at promoting commitment to and attendance at volunteering events. For example, the ATE of the voucher on commitment is a 4.8% increase and when the voucher is combined with the nudge, the ATE increases significantly to 12.2%. We also find that the nudge alone is not significantly different from the control group for all outcome variables. These results are summarised in Figure 11, which show the pre-commitment, commitment and attendance rates by treatment group. Our results suggest that there are positive synergies between nudges and financial incentives in the context of nature restoration volunteering and adds to the literature on the interaction between nudges and incentives (Drews et al., 2020).

Table 7. Results for combined vs individual treatment effects on volunteering behaviour.

	ATE for treatment group			*Comb v Nudge*		*Comb v Voucher*	
Variable	Nudge	Voucher	Combined	χ^2	p-value	χ^2	p-value
Pre-commitment	-2.90%	1.30%	-0.50%	0.185	0.334	0.107	0.628
Commitment	2.10%	4.80%	12.20%	6.325	0.0059***	3.258	0.0355**
Attendance	0.40%	3.90%	9.60%	7.517	0.0031***	2.578	0.0542*

*Note: These are one-sided chi-squared proportion tests in-line with our pre-registered hypotheses. ATEs are relative to the proportion committing (for example) in the control group. Sample sizes are N = 154 for the nudge group, N = 161 for the voucher group and N = 167 for the combined group. * p < 0.10, ** p < 0.05, *** p < 0.01.*

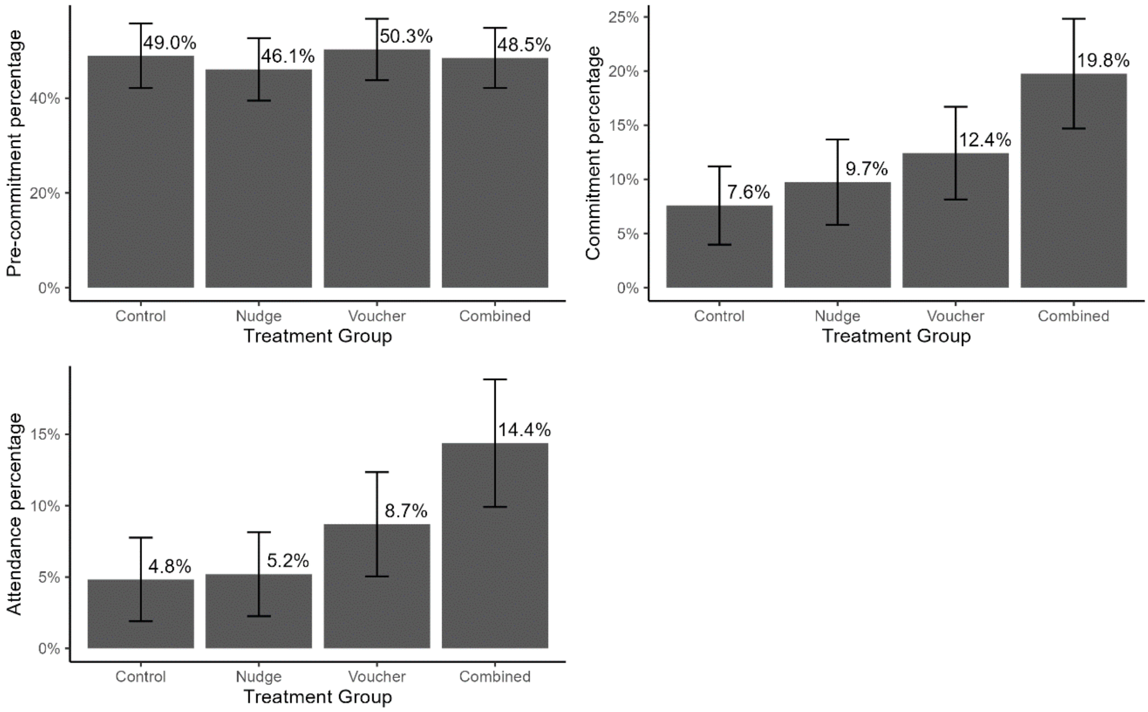

Figure 11. Summary graph of volunteering rates by treatment group. Error bars show 90% confidence intervals. Pre-commitment is in the top left, commitment in the top right and attendance in the bottom left.

4.6.7. Regression model controls

As we have collected a wide range of data from participants, it is pertinent to avoid over-fitting our regression models or including control variables that may induce multicollinearity problems. To investigate this issue, we estimate a correlation matrix for our set of potential control variables and present this in Figure 12. We are particularly interested in situations where there are high correlations between control variables and we may be able to reduce dimensionality by removing a variable from the set for the regression models.

Figure 12 shows that there are a few clusters of strong positive or negative correlations between our control variables. Firstly, there are strong correlations between our employment status dummies. This makes sense because being “full-time” should be very strongly negatively correlated with being “part-time” as you generally cannot be in both states of employment. As such, these correlations are not of concern. Neither is the strong positive correlation between age and being retired – again, this is a feature of the “retired” variable because you generally must exceed a certain age to be considered retired.

The correlations we are concerned about are the strong correlations between our Environmental Identity (EID), Environmental Locus of Control (LOC) and Pro-Environmental Behaviour (PEB) indices. In particular, EID is strongly correlated with both LOC and PEB. These concepts are inter-related and including all three may introduce multicollinearity issues. Given that the EID index is strongly correlated with both the LOC and PEB indices, and the EID index is more generic (asks about how environmentally friendly one is rather than asking about specific behaviours), we decide to remove the EID index from our models. When we do so, we notice small improvements in our regression model adjusted R^2 values (or AIC statistics in the case of our non-linear models). For all further stage one and stage two regressions, we exclude the EID index covariate to avoid multicollinearity issues.

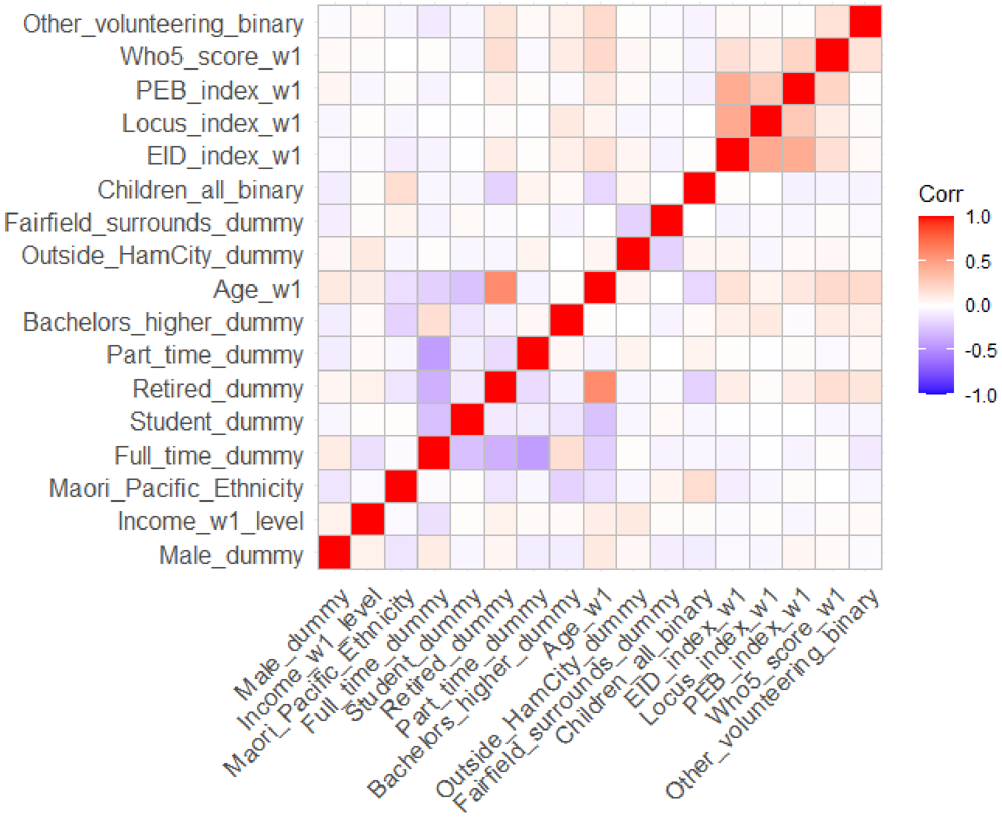

Figure 12. Correlations between all possible covariates.

4.6.8. Regression results

Here, we present the results from a series of regression models to predict both commitment and attendance at volunteering events. We do not look at pre-commitment given the null results from our earlier sections.

Commitment regression model

Our commitment regression model allows us to examine the impacts of our treatments on commitment probability while controlling for demographics, environmental attitudes and wellbeing. We also control for the general availability reported at the pre-commitment stage (as this will affect the probability of being *able* to commit).

Our results in Table 8 are in line with our hypothesis test results. We find the combined treatment group significantly increases commitment to volunteer. The voucher group on average (across the two voucher treatments) also increases commitment to volunteer, while the nudge alone is ineffective.

The results also show that LOC beliefs are an important predictor of commitment to volunteer and so are the number of days people initially stated they were available – this is an availability effect. Those who are outside Hamilton are less likely to commit and so are those who classify themselves as "high income". There is also a positive association between wellbeing and commitment probability

Table 8. LPM regressing commitment on treatment groups and controls.

	Commitment probability	
	(1)	(2)
Nudge	0.028	
	(0.032)	
Voucher	0.039	
	(0.033)	
Combined	0.130***	
	(0.036)	
Voucher		0.072***
		(0.025)
Total days available	0.023***	0.023***
	(0.004)	(0.004)
LOC index	0.030**	0.028**
	(0.013)	(0.013)
PEB index	0.006	0.009
	(0.013)	(0.014)
Who5 score	0.004*	0.004*
	(0.003)	(0.003)
Other volunteering	0.007	0.005
	(0.031)	(0.032)
Male	-0.012	-0.014
	(0.028)	(0.028)
Low income	0.022	0.021
	(0.035)	(0.036)
High income	-0.071**	-0.065**
	(0.032)	(0.033)
Maori/Pacific	0.031	0.027
	(0.030)	(0.031)
Full time	0.050	0.049
	(0.034)	(0.034)
Student	0.107	0.091
	(0.067)	(0.068)
Retired	0.056	0.059
	(0.054)	(0.054)
Part time	0.031	0.025
	(0.042)	(0.042)
Bachelors or higher	-0.007	-0.011
	(0.026)	(0.027)
Age	0.001	0.001
	(0.001)	(0.001)
Outside Hamilton City	-0.075***	-0.074**
	(0.029)	(0.029)
Near Fairfield	-0.016	-0.015
	(0.034)	(0.034)
Children dummy	0.045	0.039
	(0.028)	(0.028)
Intercept	-0.304***	-0.285***
	(0.109)	(0.105)
Observations	627	627
R^2	0.147	0.136
Adjusted R^2	0.117	0.109
Residual Std. Error	0.312 (df = 605)	0.313 (df = 607)
F Statistic	4.955*** (df = 21; 605)	5.042*** (df = 19; 607)

*Note: * p < 0.10, ** p < 0.05, *** p < 0.01. BM robust SEs in parentheses.*

Attendance regression model

We run four variations on the LPM to predict attendance (Table 9). In the first two columns we include dummies for all three treatment groups (relative to the control). In the final two columns, we include a single dummy for the voucher treatment groups. Columns 2 and 4 also include a commitment dummy variable, which allows us to examine the impact of treatments on attendance, holding commitment constant.

From columns 1 and 3, we can see that the combined treatment and the voucher treatments on average increase the probability of attending a volunteering event. We can see that our proxy for availability (days available) is also a significant predictor across all models. Once we control for commitment (in columns 2 and 4), we can evaluate the effects of our treatments on conversion from commitment to attendance.

We find that none of the treatments significantly increase the probability of attending once a person has committed to attending an event. Indeed, the only significant predictor of conversion to attendance is our availability variable. This suggests that the effects we observe in our hypothesis testing are driven by the commitment stage. Once individuals have committed, other factors (like treatment status) matter significantly less. For example, in column 4, the only two variables that significantly predict attendance are availability and pre-commitment.

This is an interesting finding that speaks to the literature on commitments, commitment devices and implementation intentions (Bryan et al., 2010; Crompton & Kasser, 2009; Gollwitzer, 1999). We can think of our pre-commitment variable as an individual's stated intentions, the commitment variable as more of a traditional commitment (as described in previous literature) and attendance as observed behaviour. Our results support previous literature that shows commitment significantly increases the probability of engaging in particular behaviours (Bryan et al., 2010; Hines et al., 1987; Lokhorst et al., 2013; Rogers et al., 2014). Moreover, we also find that the commitment effect appears so strong that financial incentives (in our case, a supermarket voucher) has no effect on actual behaviour once commitment is taken into account. This is a relatively novel finding and one that deserves to be explored further in future research. It would also suggest that policies targeting increases in commitment could result in significant changes in behaviour at relatively low cost. As other authors have shown, public commitment are particularly useful because they also engage with individuals social identity (Grilli & Curtis, 2021).

Table 9. LPMs regressing attendance on treatment status and controls.

	Dependent variable: Attendance probability			
	All Treatments		Voucher vs Non-Voucher	
	(1)	(2)	(3)	(4)
Nudge	-0.003	-0.021		
	(0.024)	(0.017)		
Voucher only	0.030	0.005		
	(0.027)	(0.017)		
Combined	0.099***	0.015		
	(0.030)	(0.018)		
Voucher			0.067***	0.021
			(0.021)	(0.013)
Days available	0.021***	0.007***	0.021***	0.006***
	(0.004)	(0.002)	(0.004)	(0.002)
Committed		0.646***		0.646***
		(0.053)		(0.053)
LOC index	0.010	-0.010	0.009	-0.009
	(0.010)	(0.007)	(0.010)	(0.006)
PEB index	0.002	-0.002	0.005	-0.002
	(0.011)	(0.007)	(0.012)	(0.007)
Who5 score	0.004**	0.002	0.004**	0.002
	(0.002)	(0.001)	(0.002)	(0.001)
Other Vol.	-0.006	-0.011	-0.007	-0.010
	(0.026)	(0.017)	(0.026)	(0.017)
Male	-0.011	-0.003	-0.012	-0.003
	(0.024)	(0.015)	(0.024)	(0.015)
Low income	-0.009	-0.024	-0.009	-0.022
	(0.030)	(0.018)	(0.030)	(0.018)
High income	-0.036	0.010	-0.033	0.009
	(0.030)	(0.013)	(0.031)	(0.013)
Maori/Pacific	-0.001	-0.021	-0.005	-0.022
	(0.024)	(0.017)	(0.024)	(0.017)
Full time	0.031	-0.001	0.031	-0.0004
	(0.029)	(0.018)	(0.029)	(0.018)
Student	0.122**	0.053	0.110*	0.051
	(0.062)	(0.033)	(0.062)	(0.033)
Retired	0.034	-0.002	0.036	-0.002
	(0.042)	(0.029)	(0.043)	(0.029)
Part time	-0.018	-0.038	-0.022	-0.038
	(0.031)	(0.025)	(0.031)	(0.025)
Bachelors +	0.005	0.009	0.002	0.009
	(0.021)	(0.014)	(0.022)	(0.014)
Age	0.001	0.0004	0.001	0.0004
	(0.001)	(0.0005)	(0.001)	(0.0005)
Out of Hamilton	-0.054**	-0.005	-0.053**	-0.005
	(0.025)	(0.014)	(0.026)	(0.013)
Near Fairfield	-0.039	-0.029	-0.037	-0.028
	(0.024)	(0.019)	(0.025)	(0.019)
Children	0.010	-0.019	0.007	-0.018
	(0.022)	(0.014)	(0.023)	(0.014)
Intercept	-0.164*	0.032	-0.166**	0.018
	(0.085)	(0.057)	(0.083)	(0.055)
Observations	627	627	627	627
R^2	0.157	0.663	0.149	0.662
Adjusted R^2	0.127	0.651	0.122	0.651
Residual Std. Error	0.260 (df = 605)	0.165 (df = 604)	0.261 (df = 607)	0.164 (df = 606)
F Statistic	5.348***	53.995***	5.582***	59.372***

*Note: * p < 0.10, ** p < 0.05, *** p < 0.01. BM robust SEs in parentheses.*

4.7. Hypothesis testing conclusions

We used the preceding results (chi-squared tests and regressions) to test our main and additional hypotheses for stage one. Below, we briefly summarise whether each of these hypotheses are supported given our results. These are the main results for stage one.

In the next section of this chapter, we will explore the heterogeneity in voucher treatment effects. The final section will summarise the key findings and insights from stage one of our field experiment.

4.7.1. Main hypotheses

> ***H1.1.*** *All three treatments will increase the likelihood of volunteering (relative to the control).*

Partially supported. Our results show the voucher and combined treatments increase the probability of committing to and attending a volunteering event. However, the nudge alone is ineffective at increasing volunteering. We also find that none of the treatments affect pre-commitment to volunteer, which may be suggestive of a moderate behaviour-intention gap.

> ***H1.2.*** *The voucher conditions will be more effective at increasing the likelihood of volunteering than the information treatments (nudge and control).*

Supported. Our results show that the voucher treatment significantly increases the probability of committing to and attending a volunteering event relative to the information-only treatment groups.

> ***H1.3.*** *The combined treatment will be better than the nudge or voucher treatment separately at increasing the likelihood of volunteering.*

Supported. Our results show that the combined treatment is significantly more effective than either the voucher treatment or nudge treatment alone. Indeed, the nudge alone was ineffective at shifting volunteering behaviour but was effective in conjunction with a voucher.

4.7.2. Supplementary hypotheses

> **H1.A1.** *Being offered a voucher will affect donation behaviour.*

Partially supported. We found that being offered a voucher reduces the probability of donating in the immediate after-math of being offered the voucher. However, these effects do not persist in survey two (approximately one month after survey one). Moreover, there were no immediate effects of the voucher on the size of the donation, given a donation is made.

> **H1.A2.** *Treatment status in stage one will affect the response rate to survey two.*

Not supported. We find no support for this hypothesis. Treatment status in stage one does not affect the probability of responding to survey two (which is approximately 75%).

4.8. Heterogeneity results and discussion

In the penultimate section of this chapter, we will start by presenting the voucher heterogeneity results for commitment to attend a volunteering event. We will then present the heterogeneity results for attending a volunteering event. The results are largely consistent with the theoretical model we present in Chapter 2 and the commitment results are generally in good alignment with the attendance results. There are some differences in the covariates that explain heterogeneity between the commitment and attendance modelling, which we discuss below.

4.8.1. Heterogeneity in voucher effects on commitment

We plot the distribution of estimated ITEs of the voucher on commitment in Figure 13. We have re-scaled the ITEs into a percentage increase in the probability of committing to an event, relative to the average probability of committing to an event in the control group (so the ITEs have been multiplied by $\frac{1}{\bar{Y}_0} * 100$). The reason for scaling these coefficients is to show the relative size of the treatment effects, relative to the baseline level of commitment in the control group.

Figure 13 shows there is significant heterogeneity in the predicted ITEs, with the mean ITE being 67% (consistent with our earlier results) and the coefficient of variation (CV) being 1.54. Most individuals have positive ITEs, but there are a cluster of individuals who have negative expected ITEs.

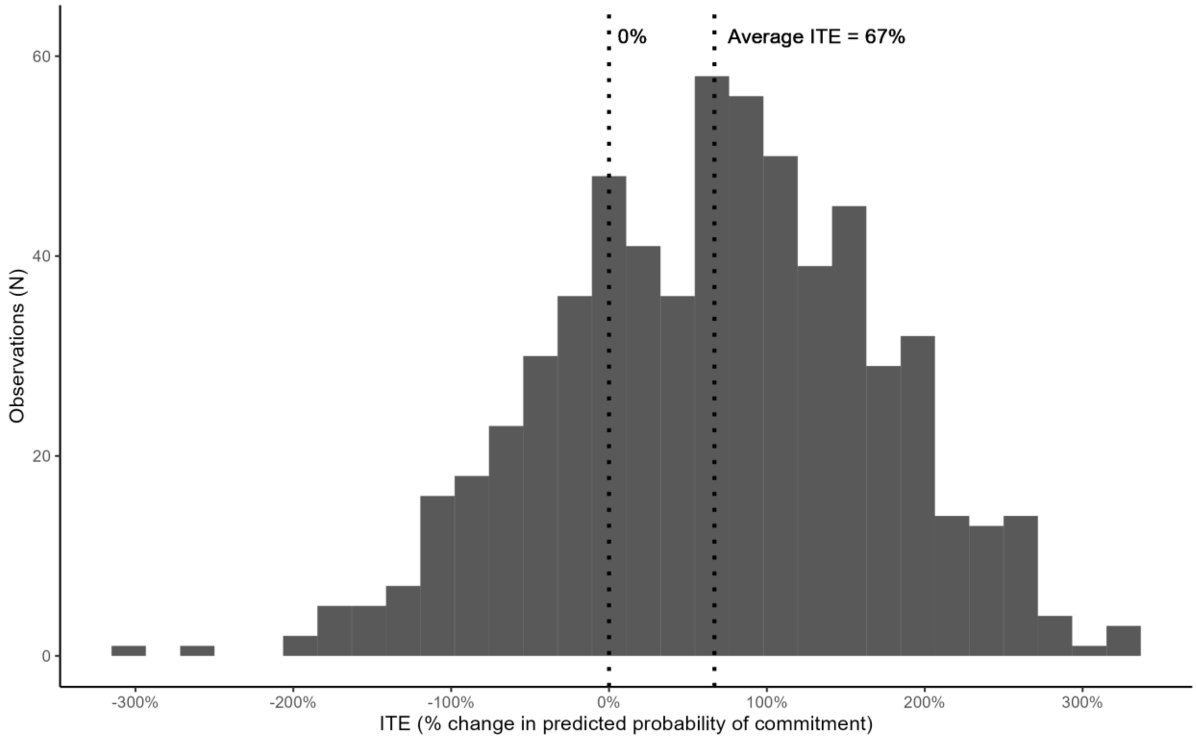

Figure 13. Distribution of expected ITEs of the voucher on commitment probability.

We then run simple regressions of ITEs on each covariate and report the R^2 results in Appendix K. The five most important variables are the male, children, bachelor's or higher and part-time dummy variables, alongside the PEB index (proxying for environmental attitudes). We explore the heterogeneity driven by these five variables below. In Table 10, we show the results from an OLS regression of ITEs on the five most important covariates. Together, they explain over ¾ of the variation in expected ITEs. We plot the ITE predictions at different levels of the PEB covariate (holding all others at their means) in Figure 14 (we do not plot the other variables as they are dummy variables so plotting the interaction is less informative). As we state in the methods, this is a useful exploratory approach but is not meant to precisely identify heterogeneous effects. Future researchers could extend these methods by adding further robustness checks and using more complex machine learning methods for exploring heterogeneous treatment effects (Athey & Wager, 2019).

We note that we have not scaled the ITEs and coefficients for the results below, so the effects are interpreted as the absolute increase in the predicted probability of committing to an event (rather than the percentage increase in the probability, relative to the control average – see Figure 13).

Table 10. Regression of voucher ITEs on five most important covariates for heterogeneity.

	ITE Voucher Effect
Male	-0.114***
	(0.004)
Children dummy	0.077***
	(0.004)
Bachelors or higher	-0.077***
	(0.004)
PEB index	0.038***
	(0.002)
Part time	0.060***
	(0.005)
Intercept	-0.088***
	(0.012)
Observations	627
R^2	0.741
Adjusted R^2	0.739
Residual Std. Error	0.049 (df = 621)
F Statistic	355.138***

*Note: * p < 0.10, ** p < 0.05, *** p < 0.01. BM robust SEs in parentheses.*

We find that the average ITE is 11.4% lower for males than females and gender diverse individuals. From Table 10, it appears that the supermarket voucher incentive has very little effect on males and a substantial positive impact on females and gender diverse individuals. This could indicate that men are less receptive to financial incentives to volunteer or that they are less receptive to the type of financial incentive we used (a supermarket voucher). Recent research from the US suggests that women still do more supermarket shopping than men and this may mean the supermarket incentive is of less salience to men, on average (Hardin-Fanning & Gokun, 2014; Storz et al., 2022). However, this result deserves more attention in future research.

We find that those with children have a 7.7% higher ITE on average than those without children. However, the average ITE for those without children, holding all other factors constant, is still positive and around 4% (the voucher increases the probability of committing by 4%). This could be because households with children have greater financial need or higher supermarket costs, which raises the perceived value and salience of the voucher incentive.

We also find that those with at least a Bachelor's degree or higher are less likely to respond to the supermarket voucher incentive. While the ITEs on average are still positive, they are 7.7% lower on than the ITEs for individuals with less than a Bachelor's level education. In line with our theoretical model, those with greater levels of education are likely to have higher opportunity costs in terms of foregone salary (this is a common theoretical finding in economics - see for example Murnane & Olsen, 1989). Hence, our theory would predict these individuals would need a larger incentive on average to encourage volunteering.

Our results show that as PEB index increases, proxying for general pro-environmental attitudes and behaviour, so does the average ITE from the voucher. Figure 14 shows that when the PEB index is very low, the effect of the voucher is negative. However, for those with moderate pre-existing levels of PEB, the average ITE is positive and this becomes significantly stronger for those with high scores on the PEB index. This is an encouraging result and suggests that we are not seeing any significant crowding out effects from offering a financial incentive, which we would expect to be larger in individuals with higher pre-existing intrinsic motivation (Bénabou & Tirole, 2006; Bowles & Polania-Reyes, 2012; Frey & Jegen, 2001). This adds to the literature on the crowding in and out effects of financial incentives for pro-environmental behaviour, for which there are mixed results (Ling & Xu, 2021; Vorlaufer et al., 2023). Moreover, this result aligns with our theoretical model, which would argue that individuals with high pre-existing PEB scores are probably closer to net positive territory initially and would need less of an incentive to encourage initial volunteering uptake.

Finally, we find that those who work part-time have a significantly larger average ITE than other individuals. This is the least important of the five variables for explaining heterogeneity, but nonetheless implies vouchers are 6.0% more effective for those working part-time. This could be because those who work part-time have lower incomes (and thus greater need for a voucher) and a lower opportunity cost of time, which again aligns well with our theoretical model.

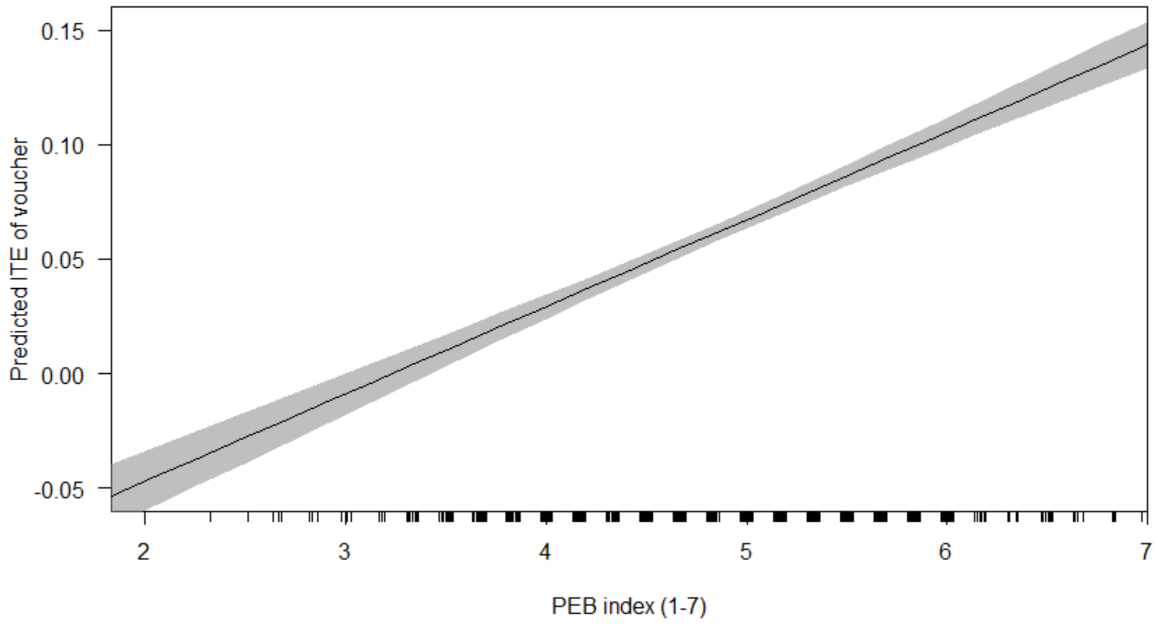

Figure 14. Predicted voucher ITEs on commitment at different levels of the PEB index variable

4.8.2. Heterogeneity in voucher effects on attendance

The attendance heterogeneity results show similar patterns to the commitment results, with some important differences. To start, Figure 15 plots the distribution of ITEs and shows that the ITEs are larger on average, but we have a similar spread in terms of heterogeneity, with a near identical coefficient of variation of 1.52. The average ITE is 114%, which indicates that on average, being offered the voucher just over doubles the probability of attending a volunteering event.

We include the R^2 results for our simple regressions of ITEs on each covariate in Appendix K. Like the commitment modelling, the male dummy, children dummy and PEB index variables are three of the most important drivers of heterogeneity. Though, in contrast to the commitment models, the outside of Hamilton and the student dummy variables are also key predictors of treatment effect heterogeneity. As we have discussed the male, children and PEB variable results in the previous section, we will focus on the outside of Hamilton and student dummy variable results here. The regression results reveal that the five aforementioned factors explain over ¾ of the observed heterogeneity in treatment effects, which is similar to the commitment modelling results (Table 10).

We include a plot of the predicted ITEs across the PEB index in Appendix L (as it is very similar to the plot for commitment graph in Figure 14).

Table 11 shows that the voucher is essentially ineffective on those living outside of Hamilton. This makes sense as those living outside of Hamilton have further to travel and thus larger transaction and time costs associated with attending a volunteering event. Our theoretical model would predict that the required incentive would be significantly greater for these individuals because it needs to overcome these additional costs.

Table 11 also shows that the average ITE is stronger for students than it is for others (although, the average ITE is still positive for non-students). Students have an ITE that is higher by 8.2% on average, which is a substantial increase in the probability of attending a volunteering event. Like previous findings, we can explain these results using our theoretical model. Students, on average, are likely to have more free time and a lower opportunity cost of attending a volunteering event (this is one of the reasons students are so often used for lab experiments in economics - Levitt & List, 2007). As such, our theory would suggest they face lower costs and thus need less of an incentive to encourage initial uptake of volunteering.

The fact that the outside of Hamilton and student dummy variables influence the attendance but not commitment ITE results is potentially because both of those variables have strong relationships with the costs of actually attending an event and they both have some effect on the flexibility of an individual in their ability to attend an event.

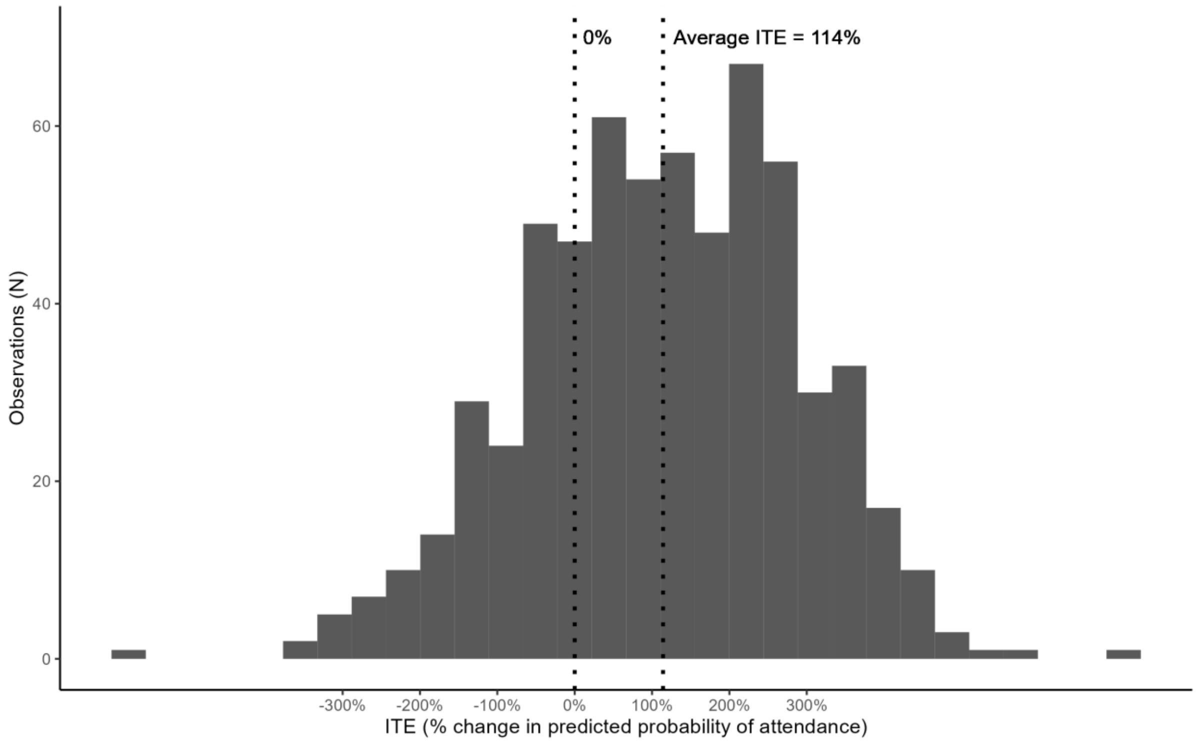

Figure 15. Distribution of expected ITEs of the voucher on commitment probability.

Table 11. Regression of voucher ITEs on five most important covariates for heterogeneity on attendance outcomes.

	ITE Voucher Effect
Male	-0.118***
	(0.004)
PEB index	0.051***
	(0.002)
Children dummy	0.056***
	(0.004)
Outside Hamilton City	-0.072***
	(0.005)
Student	0.082***
	(0.007)
Intercept	-0.174***
	(0.010)
Observations	627
R^2	0.769
Adjusted R^2	0.768
Residual Std. Error	0.044 (df = 621)
F Statistic	414.379*** (df = 5; 621)

*Note: * p < 0.10, ** p < 0.05, *** p < 0.01. BM robust SEs in parentheses.*

4.9. Conclusion

We cover a large amount of ground in this chapter and this final section focuses on summarising the key results and takeaways from stage one of our field experiment, where we evaluate interventions designed to increase volunteering for nature restoration groups.

Firstly, through random assignment, we show that offering a financial incentive significantly increases commitment to and attendance at nature volunteering events. Our exploratory heterogeneity analysis suggests that this positive effect is stronger for those with higher pre-existing environmental motivation and for groups where vouchers are more salient relative to probable income (for students and those with less education) and where travel costs are low. The stronger effect size for those with high pre-existing environmental motivation suggests that the voucher incentives are not crowding out intrinsic motivation to volunteer. We also show that there may be a small initial crowding out effect on willingness to donate to an environmental organisation (immediately after being offered a voucher), but this does not persist in the long run so crowding out concerns are less of a worry in this context. These findings add to the growing literatures on interventions to increase volunteering and on the effects of financial incentives for engaging in pro-environmental behaviour (Ling & Xu, 2021; Sloot & Scheibehenne, 2022).

Secondly, we find that nudging participants alone has no effect on volunteering behaviour. However, there are considerable synergies between the nudge and voucher incentive, with the voucher effectiveness being significantly enhanced when coupled with a nudge. For policymakers, this suggests that the efficacy of incentive-based interventions to encourage the uptake of nature restoration volunteering (and potentially other PEBs) could be enhanced by coupking the intervention with a low-cost nudge. These results add to recent literature that examines the presence of synergies between nudges and financial incentives (Drews et al., 2020; Fanghella et al., 2021; Sloot & Scheibehenne, 2022). Most studies focus on energy consumption as the behaviour of choice and we are the first to study this synergy (as far as we are aware) in the context of nature restoration volunteering. This is pertinent because Drews et al. (2020) show that synergies can be positive, negative and null, depending on the context, and policymakers need more empirical evidence to evaluate possible synergies in different contexts. Many studies show negative synergies (nudges distract participants from incentives and vice versa) so it is important to assess synergies empirically before coupling nudges and incentives in large-scale interventions (Drews et al., 2020; Fanghella et al., 2021).

Thirdly, the effective treatments (the voucher only and voucher and nudge combined) increase volunteering behaviour through increasing commitment. Our initial results show that all the treatments have no effect on pre-commitment to volunteer. We also show that the treatments have no effect on attendance, *once we control for commitment*. Hence, our treatments operate by increasing the probability of a participant committing to an event and once commitment is made, the treatments have no further effect on behaviour. This adds to the literature on commitments and commitment devices, which have been shown to be effective tools in many other contexts (Bryan et al., 2010; Lokhorst et al., 2013; Rogers et al., 2014). However, the mechanisms and theory behind why the interventions have no effect on pre-commitment and no effect on attendance (conditional on commitment) remain unclear. Future research could investigate this further to help further our understanding of the effects of nudges and financial incentives on volunteer behaviours (and other PEBs).

Finally, we provide some empirical support for the predictions made by our simple theoretical model in Chapter 2. One key piece of evidence is that there are individuals in the control and nudge groups ("informational" treatment groups) who start volunteering (or commit to volunteering). This is consistent with our theory which suggests that information uncertainty and inaccuracy is a barrier to the initial uptake of pro-social behaviours like nature volunteering. Because our design is such that all participants were not previously volunteering, we can likely attribute any volunteering behaviour to the experimental conditions. Hence, as predicted by our model, information treatments alone are enough to increase volunteering for some individuals. Furthermore, the combined effect being larger was a prediction from our model and so was the sizeable effect of the voucher incentive. In addition, the exploratory heterogeneity results are well explained by the net benefits function in our theoretical model.

It is also important to recognise the limitations of the research and point out directions for future research. As we outlined at the start of this chapter, research on ways to increase nature volunteering is limited. We add to that limited literature but are only able to test two different types of interventions (voucher and nudge) and a combined intervention. Future research could expand on these results by testing different types of interventions and targeting different sub-groups of the population. For example, testing the effects of intervening in the workplace, potentially through corporate volunteering schemes that are growing in popularity (Boštjančič et al., 2018; Loosemore & Bridgeman, 2017). Moreover, due to time constraints and limits on sample size, we take an exploratory approach to testing for heterogeneity in voucher treatment effects. Future research could aim to target places with larger populations to increase sample size and statistical power, which will aid in the evalution of treatment effect heterogeneity and the use of more complex methods (like machine learning-based methods - Athey & Wager, 2019).

In addition, we only deployed one variation of the financial incentive (in terms of value and the framing of the incentive). We designed our incentive to limit crowding out of intrinsic motivation by emphasising the one-off nature of the incentive and that it was to help people try volunteering for the first-time. We show that there is no significant crowding out or negative spillover effects, but cannot say whether this was due to the framing, value of the incentive or context (volunteering for nature restoration) or a combination of all three. The literature on the spillovers effects of incentives are mixed and future research could consider deploying different values of incentives and using different framings to evaluate crowding-in or out potential in the context of nature restoration volunteering.

Finally, as we suggest earlier, future research could look into the pre-commitment, commitment and attendance distinction. This design (getting pre-commitment, commitment and then evaluating behaviour) is relatively novel and there is no clear theory as to why interventions have no effect on behaviour, given commitment. This type of theory would fall into the literature on commitments and commitment devices, and the mechanisms and interactions between pre-commitment, commitment and attendance could be expanded on in the future to help inform policy. Knowing which point to intervene (before commitment, after commitment or during commitment) and why and how that varies by context would be useful for maximising the efficacy of behavioural interventions.

Chapter 5: Stage Two Methods and Results

5.1. Introduction

This chapter presents the methods and results for stage two of our field experiment. In stage two, we estimate the causal impacts of having a first-time experience volunteering on future volunteering behaviour and other important outcomes of interest (for example, environmental identity, locus of control beliefs and wellbeing).

Psychological wisdom says "past behaviour predicts future behaviour", or past behaviour is one of the best predictors of future behaviour (for a review, see Albarracín & Wyer, 2000). However, there is little causal evidence for this phenomenon because it is hard to randomise or effectively randomise past behaviour. It is also difficult to disentangle the reasons past behaviour and future behaviour are correlated, which has been a theme addressed by many of the popular theoretical models in psychology (Ajzen, 2011; Hines et al., 1987; P. Sparks & Shepherd, 1992; Terry et al., 1999a). In this chapter, we exploit our experimental design that focuses on first-time volunteers to estimate the causal impacts of volunteering on future behaviour and other important outcomes. Because we focus on first-time volunteers and can find conditionally random assignment to volunteering for the first-time, we can overcome the issues of endogeneity that prevent the causal assessment of how past behaviour affects future behaviour.

We also provide insights into why a first-time experience volunteering in nature might affect future volunteering behaviour. As we show in our theory section of this chapter, there are competing mechanisms that could drive the relationship between past volunteering behaviour and future volunteering behaviour. On the one hand, as our theoretical model suggests, individuals may be *a priori* uncertain and inaccurate in their estimates of the benefits from volunteering (Chapter 2). As such, a first-time experience will provide experiential information to participants that may affect their future decision-making (Czajkowski et al., 2015). On the other hand, spending time volunteering in nature could strengthen environmental identity and attitudes, which in turn affects preferences and future behaviour (Balundė et al., 2019). Our approach allows us to make comments about which mechanism, if any, appears to be driving any relationship between first-time volunteering and future behaviour.

Moreover, we add to some recent papers on public transit use that suggest policymakers could crowd-in future behaviour by helping individuals try something new (or experiment). For example, Larcom et al. (2017) shows that a temporary disruption to commuter rail lines shifted behaviour permanently because it allowed people to try new routes which turned our to be better. Gravert & Olsson Collentine (2021) also show that a temporary incentive for public transit use crowded in future behaviour amongst those not previously using public transit. This relates closely to the literature on satisficing, which suggests individuals (in the face of search costs) stop searching for new behaviours and goods once they reach a certain level of satisfaction (Caplin et al., 2011; Simon, 1955). This is a rational decision, but means there may be behaviours, goods or services that if consumed would make the individual better off. Studies like these are limited and we add to this growing body of evidence on the benefits of helping people experiment. This is particularly pertinent if policymakers can identify cases where behaviours would benefits individuals and society more broadly (like volunteering).

As alluded to above, we also examine the causal effects of volunteering on several other important outcomes where evidence (and particularly, causal evidence) is limited. For example, the effects of volunteering on locus of control beliefs and environmental identity – both of which are highly predictive of future pro-environmental behaviour (PEB) and pro-environmental policy support (Allen & Ferrand, 1999; Andor et al., 2022; Crompton & Kasser, 2009; van der Werff et al., 2013). We also assess whether volunteering for the first-time generates spillover effects to other pro-environmental behaviour, adding to the literature on PEB spillovers and suggesting a potential further benefit from helping people experiment (which was not covered in previous studies). We do this in-part by using a semi-incentivised measure of donation behaviour, which is, as far as we can tell, a relatively novel approach to measuring donation behaviour and is a good middle ground between fully-incentivised measures and self-reported measures (which often suffer from significant biases - Kormos & Gifford, 2014).

Overall, we make several contributions to the literature on PEB change, environmental economics and environmental psychology. Most of these contributions stem from estimating the causal impacts of a first-time experience volunteering in nature. Such causal inference research is well suited to the field of economics, yet there are few papers in environmental economics that consider topics like environmental identity in any capacity (some exceptions include recent papers by Bonan et al., 2021; Gleue et al., 2022; Panzone et al., 2021; Zemo & Termansen, 2022).

We start this chapter by reporting our pre-registered hypotheses for stage two – these guide the methods and analysis in this chapter. We then describe the relevant theory and data, highlighting how we measured future volunteering behaviour and the other outcome variables of interest. Next, we present the general methods for analysing our hypotheses, which include covering how we overcome endogeneity issues (because volunteering for the first-time is not unconditionally random). We then present the results and discussion, starting with descriptive statistics before reporting evidence to support our causality assumption. We then present our full set of empirical results, including weighted hypothesis tests and regression models to assess support for our stage two hypotheses. We round the chapter off with discussion of how the results relate to our theoretical model in Chapter 2 and a summary of the key findings.

5.2. Hypotheses

In this section, we present the main hypotheses and supplementary hypotheses for stage two. All of these hypotheses, including the supplementary hypotheses, were pre-registered. We use the same notation for hypotheses as the previous chapter (Chapter 4).

5.2.1. Main hypotheses

Our three main hypotheses are:

H2.1. *A first-time experience volunteering will lead to increases in future volunteering.*

H2.2. *The effect size will be stronger for those who live near the community group where the event is held.*

H2.3. *There is a difference in the treatment effect depending on if the volunteers came from a voucher group in stage one.*

Our main hypotheses are guided by our theoretical model in Chapter 2 and experimental design in Chapter 3. Our theoretical model predicts that providing individuals with their first-time experience volunteering will provide important information that may correct inaccurately low prior estimates of the benefits of volunteering, reduce uncertainty around the benefits of volunteering and help mitigate against high adjustment costs when switching to a new behaviour. We also discuss in Chapter 3 that an experience volunteering in nature could strengthen pro-environmental identity and attitudes, which may further crowd in future volunteering behaviour. As we outline in Chapter 3, these two mechanisms (a – providing valuable information and reducing adjustment costs and b – shifting environmental attitudes and identity) lead us to hypothesis that a first-time experience volunteering will lead to increase in future volunteering (hypothesis H2.1).

The final two main hypotheses (H2.2 and H2.3) relate to heterogeneity in the effects of a first-time experience volunteering on future volunteering behaviour.

In relation to H2.2, we hypothesise that those who live closer to the Fairfield Project (where the volunteering events are held) will experience stronger crowding-in effects of future volunteering behaviour. This comes from the literature on volunteering, which suggests a sense of community and sense of place is an important factor in decisions to volunteer and the enjoyment one gets from volunteering (Caissie & Halpenny, 2011; Ganzevoort & van den Born, 2020; Tierney et al., 2022; Volunteering New Zealand, 2023). As such, we may expect those who live closer to Fairfield to have a stronger sense of connection to the local area and community and thus derive more enjoyment (or benefit) from the volunteering events. Returning to our theoretical model, after volunteering for the first time, these individuals may have higher estimates of the net benefits of volunteering on average than others who participated and thus be more likely to volunteer in the future. Moreover, for local residents, the events themselves could further strengthen connection to community, which may increase the expected of benefits of volunteering in the future.

For H2.3, we hypothesise that there will be some difference in treatment effects based on whether an individual was incentivised to attend through a voucher in stage one. We do not have *a priori* predictions for the direction of this effect because, like in the previous chapter, we are faced with competing explanations. Firstly, being given a voucher after attending an event may create a gift exchange effect whereby participants feel inclined to return and volunteer again to "repay" the voucher (Falk, 2007). On the other hand, on average, those who were incentivised to attend through a voucher may have lower underlying motivation to volunteer on average than those who attended without the voucher incentive (the control and nudge groups in stage one). As we do not know the relative importance of these factors *a priori*, we do not specify an effect direction for H2.3.

5.2.2. Supplementary hypotheses

We also pre-registered a larger set of supplementary hypotheses, motivated by various strands of literature (see Chapter 3). The reason we measured and assessed against so many variables is that there is still limited evidence on the relationship between volunteering (particularly, nature volunteering) and the outcomes listed below and most of the evidence is correlational. Therefore, we took the opportunity with our design to incorporate the estimation of the causal effects of nature volunteering on these outcome variables.

Rather than label each supplementary hypothesis individually, we label our set of supplementary hypotheses collectively as H2.Sup.

H2.Sup. *Being treated in stage 2 (attending one of our volunteering events) will increase the following outcomes:*

- *EID index*
- *LOC index*
- *Who5 score*
- *Willingness to volunteer (restoration)*
- *Knowledge of restoration groups*
- *Perceptions of restoration groups*
- *Connection to nature*
- *Connection to community*
- *PEB index*

- *Donation binary*
- *Donation value*

We discuss the importance of these variables in Chapter 3. Several of these variables (connection to nature, environmental identity, connection to community) also relate to one of the key mechanisms through which we theorise that a first-time experience volunteering may affect future behaviour (see main hypotheses and Chapter 3). Moreover, the PEB index and donation outcome variables are self-reported (PEB index) and semi-incentivised (donation behaviour) measures of pro-environmental spillovers. We may expect to see these increase if a first-time experience strengthens environmental attitudes and identity.

In the following sections, we discuss the theory, data and methods for our stage two analyses.

5.3. Theory

In this section, we outline some basic theory behind stage two, then provide details on the full set of outcomes for stage two and finish the chapter with a discussion of the assignment of treatment status in stage two.

In stage two, we test whether attending a volunteering event (treatment in stage two) affects future volunteering behaviour and other outcomes of interest. Our theory chapter (Chapter 2) outlines why we may expect a first-time experience volunteering to impact future volunteering behaviour. Our theoretical model argues that a first-time experience will provide important information about the experience of nature volunteering more generally and may correct inaccurately low estimates of the individual benefits from volunteering. This would serve to reduce uncertainty and inaccuracy, raising the certainty equivalent expected payoffs from volunteering.[26] Moreover, by providing a first-time experience in our experiment, we can help individuals overcome the initial adjustment costs associated with shifting to new activities and behaviours.

Furthermore, a first-time experience nature volunteering may crowd in future volunteering by strengthening environmental identity and environmental attitudes. Indeed, several studies provide correlational evidence that spending time in nature strengthens environmental identity and connectedness to nature (Balundė et al., 2019; Rosa & Collado, 2019). The literature also shows a positive association between connectedness to nature, environmental identity and pro-environmental behaviour (Alcock et al., 2020; Bonan et al., 2021; Rosa & Collado, 2019). Therefore, if the volunteering events strengthen connectedness to nature or environmental identity (because people are

[26] As we outline in our theory chapter, reducing uncertainty will raise the expected value of participating if an individual is risk averse.

having an experience in nature), we may also see increases in the uptake of other pro-environmental behaviours (PEBs). Hence, our initial interventions in stage one to encourage first-time volunteering may have positive spillover effects to future volunteering behaviour and other pro-environmental behaviour.

In Figure 16, we summarise the channels through which a first-time experience volunteering may impact future behaviour (both volunteering and wider PEBs). This follows the intuition from our theoretical model and the arguments made above.

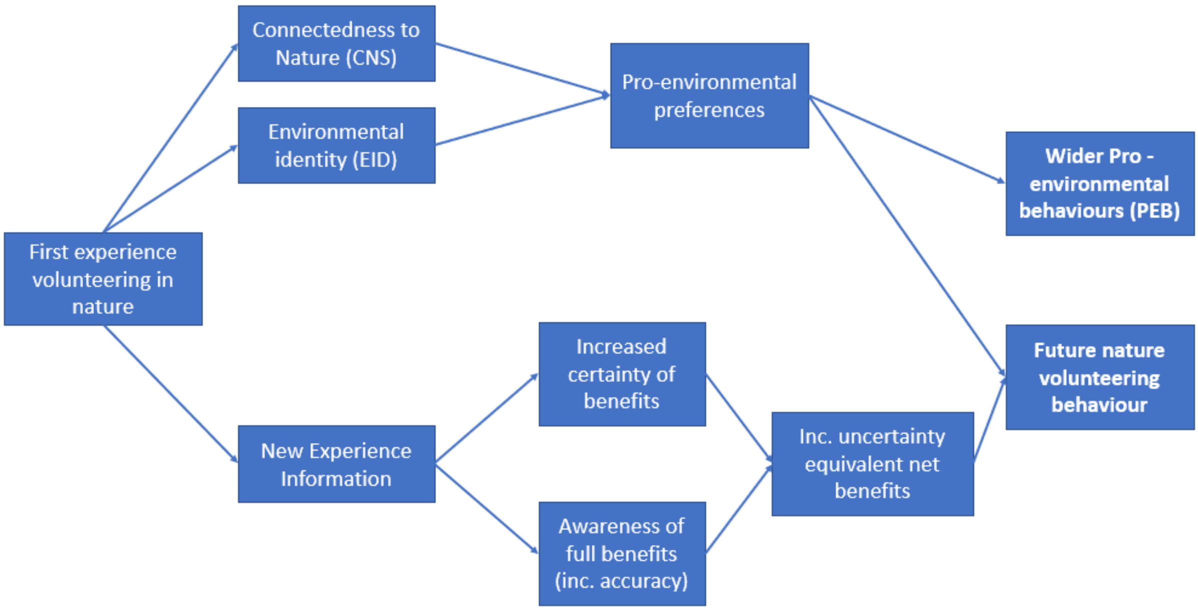

Figure 16. Schematic depiction of the channels through which a first-time experience volunteering in nature may affect future volunteering behaviour and other PEBs.

5.4. Data

For the analysis of stage two, we use data from our main surveys, the stage one commitment surveys, the attendance sheets from the Fairfield Project and the experience survey immediately following the volunteering events.

5.4.1. Surveys one and two

Our first primary source of data is from our main surveys (surveys one and two). These surveys collected data on demographics, environmental attitudes, the full range of supplementary outcomes for stage two and willingness to volunteer, measured by pre-commitment to attend a volunteering event. Survey two also contained the commitment choice within the survey, rather than requiring a separate response as we did in stage one. The question was framed in an identical manner with the same level of information as the commitment choice in stage one. Appendices A and B contains the full surveys for more detail.

Survey two was completed approximately one month after survey one. Survey one was started on the 20th of January 2023 and had a median response date of the 30th of January and a mode of the 31st of January (where N = 97 responses were recorded). Survey two was started on the 5th of March, which was also the mode response date (N = 322 responses) and had a median response date of the 6th of March.

The rate of attrition between the two surveys was 26% (74% of survey one respondents completed survey two). As we demonstrated in Chapter 4, attrition was not impacted by treatment status in stage one. After removing incomplete responses (less than 75% of the survey was complete) and duplicates, we had 561 responses to survey two. In both surveys, we asked for respondents' first and last name as well as an email address and phone number. We combined the first and last name variables and removed non-letter characters (spaces, commas and apostrophes) to produce a standardised name variable. We used the fuzzyjoin package in R to match and merge the survey one and survey two data (Robinson et al., 2020). This approach allows for slight deviations in characters within the name variable (for example, someone using Rob on survey one and Robert in survey two). There were a few cases where matches could not be made with the fuzzyjoin package (Robinson et al., 2020). We were able to manually join these observations using the email address and phone number contact fields.[27] Our final merged balanced panel dataset consisted of N = 561 individuals with observations over several time periods (Figure 17).

[27] Because survey two was only sent to those who completed survey one, each survey two response should have a corresponding survey one entry. In some cases, there were significant deviations to first names (for example, some people had completely different first names). However, the contact information was the same and linked to the survey one. In these cases, we adjusted the names to match but created a new variable identifying whether we had to make major changes to the name. This was relevant in N = 7 cases and removing these individuals as a robustness check does not affect any of the conclusions in this chapter.

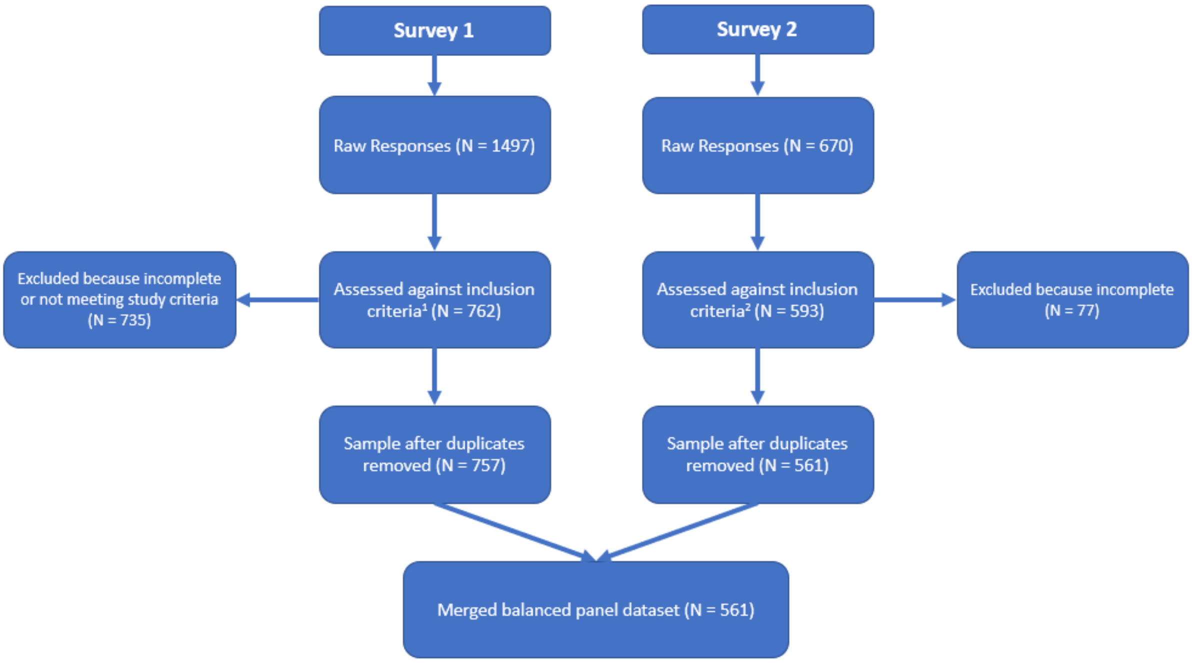

Figure 17. Data cleaning and merging processes for survey one and survey two.

5.4.2. Follow-up volunteering

In stage two, a primary focus is understanding whether volunteering for a first time affects future volunteering behaviour. Future pre-commitment and commitment to volunteer are measured during survey two (see Chapter 3). Future attendance is determined by whether a respondent attends the large public volunteering event on the 25th of March that all respondents get the opportunity to pre-commit and commit to. We also track whether any study participants attend the monthly working bee at the Fairfield Project on the 18th of March (information was also publicly available for this event).

As some respondents may have started volunteering elsewhere after participating in our study, during survey two we asked participants to identify any specific instances where they had volunteered for a nature restoration group since survey one. We manually went through these answers and recoded results where appropriate because some people stated they had made a mistake, others clearly referred to volunteering outside of the nature restoration context and others reported volunteering at one of our study events, which is not an additional "follow-up" volunteering experience. Moreover, if a respondent could not provide any details in relation to when and where they volunteered, they were coded as a zero for this variable. In total, N = 35 respondents reported engaging in nature volunteering activities outside of our study events between survey one and survey two. Most of these respondents (N = 32) were part of the "already volunteering" sample, so were not included in the main evaluations of stage one or stage two.

To summarise, first-time volunteers were coded as attending a follow-up nature volunteering event if they met any of the following criteria:

- they attended the public volunteering event on the 25th of March at the Fairfield Project
- they attended the public working bee on the 18th of March at the Fairfield Project
- they volunteered for a nature restoration group outside of our study events between responding to survey one (approximately the 30th of January) and survey two (approximately the 6th of March)

5.4.3. Outcome variables for stage two

As we have alluded to, in stage two we measure the impact of a first-time experience volunteering on other outcomes of interest. This is in addition to the future pre-commitment, commitment and attendance volunteering outcomes that we discussed earlier.

In this brief section, we will report on the theoretical reasons behind including these outcome variables and how we measure them. We list these additional outcome variables (all pre-registered) below and provide a short motivation for including these in Table 12. All items are measured with seven-point Likert scales, except for the Who-5 items which have their own scale. The set of control variables are identical to those in Chapter 4.

Table 12. Additional outcomes for stage two

Variable	Description	Justification	Measure
Environmental identity (EID) index	Measures beliefs about how environmentally friendly one is.	Environmental identity has been widely studied in psychology and has strong associations with pro-environmental behaviour (A. C. Sparks et al., 2021; Whitmarsh & O'Neill, 2010). Volunteering and experiences in nature may shift environmental identity – see previous section.	We deploy the widely used environmental self-identity scale (EID) from van der Werff et al. (2013). This is a five-item scale that is replicated exactly for our surveys.
Environmental locus of control (LOC) index	Measures beliefs about how much control one has over environmental outcomes and issues.	General locus of control (LOC) beliefs have recently been linked causally with pro-social behaviour (Andor et al., 2022). However, there is long-standing theory and empirical work on the importance of LOC beliefs for pro-environmental behaviour (Allen & Ferrand, 1999; Hines et al., 1987). However, no studies (we are aware of) look at what causally shifts LOC beliefs – we look at whether volunteering shifts LOC beliefs.	We adapt the 17-item scale developed by Cleveland et al. (2012) which has been validated and widely used since its inception (Afsar et al., 2018; Cleveland et al., 2020). We elect to use most items in the "Advocate" and "Activist" sections, which are most relevant to our study. We remove items that are very similar to save space in the survey. The final scale has five items.
Short-term Who-5 wellbeing score	Measures short-term (past two weeks) wellbeing.	There is an emerging, but still limited, body of literature assessing the causal impacts of volunteering on subjective wellbeing (Dolan et al., 2021; Meier & Stutzer, 2008). We add to this literature and are the first to assess the impacts on the short-term Who-5 wellbeing scale.	We use the widely recognised and validated World Health Organisation (WHO) subjective wellbeing measure – Who-5 (Topp et al., 2015). We do not make any changes to the five-item scale.
Willingness to volunteer (restoration)	Measures general willingness to volunteer.	We include willingness to volunteer as a more general measure of volunteering attitudes and intentions. These types of questions are common outcome variables in pro-environmental behaviour papers that use surveys and self-reported intentions (i.e., Alacevich et al., 2021; Lazarus et al., 2021).	We measure willingness to volunteer with a single item, which is a common approach in the literature for measuring intentions to carry out a specific behaviour. To strengthen the validity of the item, we include the specific timeframe *"within the next 12 months"* (Gryczynski et al., 2015; Kalton &

			Schuman, 1982; Walentynowicz et al., 2018).
Knowledge of restoration groups	Measures perceived knowledge about local nature restoration groups.	Our theoretical model predicts that attending a volunteering event reveals information about volunteering. This variable is one way of assessing whether individuals gain new information or knowledge.	We measure this with a single item on a standard seven-point Likert scale.
Perceptions of restoration groups	Measures general perceptions of local nature restoration groups.	Similar to above – we are interested in whether perceptions change after information is revealed (individuals have an experience volunteering).	We measure this with a single item on a standard seven-point Likert scale.
Connectedness to nature	Measures general connection to nature.	There is significant work showing a relationship between time in nature and connection to nature, however, little causal evidence to date (Balundė et al., 2019; Rosa & Collado, 2019). As per the previous section, we predict an experience volunteering in nature may strengthen connection to nature. Like EID, connectedness to nature may affect support for pro-environmental policy and create wider spillovers to other pro-environmental behaviours.	We elect to measure connectedness to nature with a single item rather than use one of the existing large scales (Mayer & Frantz, 2004). This was due to limits on space in the survey.
Connectedness to community	Measures general connection to local community.	Researchers show connectedness to community has a positive association with pro-social and pro-environmental behaviour (Duong & Pensini, 2023). We add to the literature on factors that may influence connectedness to community by evaluating whether spending time volunteering affects connectedness to community.	As above, we measure connectedness with a single seven-point Likert item.
Pro-environmental behaviour (PEB) index	Measures overall self-reported pro-environmental behaviour and intentions.	This variable allows us to capture overall pro-environmental attitudes and self-reported behaviour across a range of domains. We use this scale to test for positive spillovers – see previous section or literature on spillovers (Alacevich et al., 2021).	We took common items (individual behaviours) that appeared across the literature to form our six-item PEB scale. See Markle (2013) for a review of studies measuring PEB using self-reported survey responses.
Donation behaviour	Measures willingness to donate to environmental charities.	Like the previous variable, this outcome allows us to test with a semi-incentivised measure whether there are	Novel semi-incentivised measure of donation behaviour. We ask whether and how much ($0-$80) respondents would

		spillovers of a volunteering experience to other pro-environmental behaviors (charitable donations).	like to donate if they are selected as a winner of the survey prize draw.

5.5. Methods

5.5.1. Treatment and control groups for stage two

The identification of the effects of a first-time experience volunteering on future behaviour (and on other outcomes) rests on the assumption that having a first-time experience volunteering can be treated as exogenous, conditional on our control variables. As we set out in the design chapter (Chapter 3), we do not randomly assign individuals to attend an event and have their first-time experience volunteering. However, our experimental design is set up in a way that allows us to construct a credible control group for those who attended an event (those who are attended are referred to as those who are treated in stage two).

The qualitative description of our control group is: *"a group of individuals who are highly similar to those who attended an event (were treated) but could not attend due to random idiosyncratic differences in availability"*. The principle is that we could have two individuals who are essentially identical in every way (including propensity to volunteer) except one individual happens to be available on one of the days of our events and the other is not.

We estimate this control group using two key steps:

1) Reduce the sample to *only* those who pre-committed to volunteer in survey one.
2) Use weights to balance the control group on three sets of factors:
    a. Availability
    b. Voucher assignment in stage one
    c. Demographics and environmental attitudes

The first step in constructing our control group is to restrict the merged data to only those who pre-committed in survey one. This already significantly reduces the variation in underlying environmental attitudes and demographics between those who attend (are treated) and those who do not (control).

However, there are still likely to be key differences between the treated and control groups, so we use weights to balance the groups. The first difference is that the treated group are likely to have greater availability on average, which increases the probability of being available on one of the two possible event dates. Secondly, the treated group are more likely to have been randomly offered a voucher in stage one. This follows directly from the stage one analyses in Chapter 4. Finally, there may also be some residual differences in demographics and environmental attitudes. However, if our assumptions that random differences in availability drives treatment assignment is correct, we would hope to see very few differences in demographics and environmental attitudes.[28]

Once we account for these factors, we are left with two essentially identical groups that differ randomly in terms of specific availability, which means the people in the treated group randomly match their availability with our event dates and the people in the control group do not. We discuss random assignment further in section 5.5.6.

5.5.2. Weighting overview

To appropriately balance our treatment and control groups, we estimate optimisation-based weights using the optweight package (Greifer, 2022). Optimisation weights maximises the effective sample size of the control group given a set of balancing constraints, which is attractive given our smaller sample size (Wang & Zubizarreta, 2020; Zubizarreta, 2015). Under standard smoothness conditions, optimisation weights are consistent estimators of true inverse probability weights (IPW) of being treated (Wang & Zubizarreta, 2020). We estimate the weights using the average treatment effects on the treated (ATT) estimand because we are interested in the effects on those who actually attend a volunteering event (Austin, 2009; Greifer, 2023).

We specify balance constraints for all our standard control variables in stage one, in addition to our availability variable (see Chapter 4) and voucher dummy variable. We specify two types of constraints – one for the standardised mean differences (SMDs) between the treatment and control group and one for the size of the variance ratios.

Calculating balance statistics

The SMD for a continuous covariate is calculated as:

$$d = \frac{(\bar{x}_1 - \bar{x}_0)}{s_1} \tag{18}$$

where d is the SMD, $\bar{x}_1$ is the mean of covariate x in the treatment group ($Z = 1$), $\bar{x}_0$ is the mean of covariate x in the control group ($Z = 0$) and s_1 is the standard deviation of covariate x in the treatment group. Similarly, the SMD for a binary covariate is calculated as:

$$d = \frac{(\bar{p}_1 - \bar{p}_0)}{\bar{p}_1(1 - \bar{p}_1)} \tag{19}$$

[28] If we see little differences in our results, that lends support to the credibility of our control group for causal identification.

where d is the SMD, $\bar{p}_1$ is the mean of proportion for covariate x in the treatment group ($Z = 1$), $\bar{p}_0$ is the mean of proportion for covariate x in the treatment group ($Z = 0$) and the denominator is the standard deviation of covariate x in the treatment group.

The variance ratio is simply:

$$v = \frac{s_1^2}{s_0^2} \tag{20}$$

where s_1^2 is the variance of covariate x in the treatment group and s_0^2 is the variance of covariate x in the control group.

Thresholds for balancing statistics

For standardized mean differences, thresholds have commonly been proposed as 0.25 and 0.1. However, Stuart et al. (2013) found that 0.1 was better at assessing for imbalance that would bias effect estimation. Therefore, we use a hard limit of 0.1 for the standardized mean differences.

Variance ratios are another important tool for assessing balance and has been recommended by several practitioners (Austin, 2009; Ho et al., 2007). For variance ratios, thresholds of 0.5 and 2 are generally used in the literature (for a review, see Stuart, 2010). That is, the variance ratio must fall between 0.5 and 2.0. Variance ratios close to 1 indicate good group balance (Austin, 2009).

Using both SMDs and variance ratios is recognised as a superior to using t-tests (or equivalent) to assess for balance (for a review of balancing tests, see Ali et al., 2015). One of the key reasons is that t-tests are highly influenced by sample size. With small samples, researchers may be unable to find statistically significant differences between the groups because of low test power (Ali et al., 2015; Linden, 2014). Moreover, balancing is a feature only of the samples at hand, not the population, so hypothesis tests for balance make little sense.

Weights estimation

As we state above, we generate weights by optimising the following problem:

$$\max \frac{N}{\sum_{i=1}^{N} w_i} \; s.t. \begin{cases} d \leq 0.1 \\ 0.5 < v < 2 \end{cases} for\ all\ \boldsymbol{X} \tag{21}$$

where weights w_i are calculated to maximise the effective sample size subject to the balance constraints discussed earlier for the SMDs, d, and the variance ratios, v, for all $\boldsymbol{X}$ (which includes availability, the voucher dummy variable and the full set of controls used in stage one).

5.5.3. Weighted hypothesis tests

As per our pre-registration and consistent with the stage one analyses, we carry out non-parametric hypothesis testing as our main tool for evaluating the stage two hypotheses (H2.1, H2.2, H2.3 and H2.Sup). However, in this case, we weight the hypothesis tests by the inverse probability weights[29] computed using the methods described in the previous section.

Now, we estimate the average treatment effects (ATEs) as:

$$ATE = E[Y_i|Z_i = T \mid w_i] - E[Y_i|Z_i = C \mid w_i] \tag{22}$$

where the ATE is equal to the mean outcome for the treatment group less the mean outcome for the control group, conditional on weights w_i (alternatively, the weighted mean difference in outcomes between the treated and control units).

As we do in Chapter 3, we non-parametrically test the alternative hypothesis that these ATEs are greater than zero for each of our variables of interest. For binary outcome variables (like pre-commitment, commitment and attendance), we use non-parametric chi-squared tests (for more detail, see McHugh, 2013). For continuous outcomes, we use non-parametric Mann-Whitney U tests (Mann & Whitney, 1947).[30]

The key identifying assumption for estimating the causal effects of our treatment Z_i (attending a first event) is that potential outcomes are independent of treatment status, conditional on our weights. That is:

$$Y_{0i}, Y_{1i} \perp Z_i|w_i \;\; where\; w_i = W(Availability_i, Voucher_i, \boldsymbol{X_i}) \tag{23}$$

where $\{Y_{0i}, Y_{1i}\}$ are the two potential outcomes for Y_i in the treated and untreated states, Z_i is a binary treatment variable and weights w_i are a function of availability, voucher assignment in stage one and our set of demographic and attitudinal covariates from stage one ($\boldsymbol{X_i}$). On the face of it, this appears to be a reasonable assumption that will give us consistent estimates of the ATEs (Solon et al., 2015). We believe that selection into the treatment group is primarily function of general availability, voucher assignment in stage one and random idiosyncratic differences in specific availability (for example, being available on a Tuesday, but not a Wednesday). If we can weight on the two pertinent selection criteria (availability and voucher assignment in stage one), we are left with essentially random variation in the probability of being treated. We also weight for a full set of control variables, which adds to the credibility of the "no confounding variables" or un-confoundedness assumption. Nonetheless, in our

[29] As we mention in the previous section, the optimisation weights are consistent estimators of inverse probability weights (IPWs).
[30] We model our seven-point Likert variables as continuous variables.

results, we will discuss the validity of this assumption further and cases where this assumption may be violated.

5.5.4. Robustness check for supplementary outcomes – weighted lagged regression models

When we evaluate our supplementary hypotheses, we can make further use of the panel nature of our data to test the robustness of our findings. In general, we evaluate our supplementary hypotheses using the weighted hypothesis testing procedure documented above.

However, we can check whether our results are robust to conditioning on the exact value of the outcome variable in survey one (lagged outcome variable). Some of these lagged outcome variables are already included in our standard weights (for example, the survey one PEB index, Who-5 wellbeing score and LOC index). Others, however, are not included in the standard set of controls for multicollinearity reasons (please see Chapter 4). As a result, one might be concerned that treatment assignment is positively correlated with the lagged outcome variable (after controlling for all the variables in our stage one analyses) and this lagged outcome variable is of course positively correlated with the future outcome variable, generating omitted variable bias. Our weights should control for this already, as they control for past environmental attitudes, demographics and treatment status. However, using a lagged dependent variable (LDV) linear regression model is an attractive way to ensure we explicitly account for the past value of our outcome variable (Wilkins, 2018; Wooldridge, 2010).

We run LDV models regressing the outcomes of interest on treatment status and the lagged outcome variable (value from survey one). We include our estimated weights in all of these LDV regressions and use Bell-McCaffrey (BM) robust standard errors (Bell & McCaffrey, 2002; Pustejovsky & Tipton, 2018).[31] The basic regression equation we estimate, subject to our weights w_i, is:

$$y_i = \alpha + \delta Z_i + \theta y_{i(t-1)} + \varepsilon_i \tag{24}$$

where y_i is the outcome of interest, Z_i is our treatment variable and $y_{i(t-1)}$ is the lagged (survey one) value of our outcome of interest. Weighted least squares regression for the model above consists of minimising the sum of the weighted squared errors (Wilkins, 2018):

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[31] We are using Bell and McCaffrey robust SEs in stage 2 because standard sandwich variance estimators (Liang & Zeger, 1986) can be severely downward biased in small samples. We find this to be the case in our stage two analyses, with our sandwich robust SEs being substantially lower than our naïve SEs. This adjustment has become the recommended approach over standard Huber-White or Liang and Zeger (LZ) sandwich variance estimators for empirical researchers dealing with small to medium-sized samples (and even large samples – see Imbens & Kolesár, 2016).

$$S_w(\alpha,\delta,\theta) = \sum_{i=1}^{N} w_i * \left(y_i - \alpha + \delta Z_i + \theta y_{i(t-1)}\right)^2 \tag{25}$$

where $S_w(\alpha,\delta,\theta)$ are the weighted estimates for parameters α, δ and θ. To put this in context, for the hypothesis that being treated (attending an event) increases environmental identity (EID index), we would regress EID in survey two on treatment status and the EID value in survey one, weighting for our full set of controls, voucher status in stage one and general availability.

5.5.5. Multiple hypothesis corrections

Given the large number of supplementary hypotheses, it becomes highly relevant to perform some multiple hypothesis corrections to minimise the risk of making errors in inference. Therefore, when evaluating our supplementary hypotheses, we use the Benjamini-Krieger-Yekutieli (BKY) adaptive procedure to control the false discovery rate (FDR) in our multiple hypothesis testing (method demonstrated by Anderson, 2008; developed by Benjamini et al., 2006). The BKY builds on the popular Benjamini-Hochberg (BH) method (Benjamini & Hochberg, 1995) and provides sharper control for the FDR (Anderson, 2008).

We favour the BKY approach because controlling for the FDR is a higher-powered approach over the more conservative Family-Wise Error Rate (FWER) corrections. This is particularly important in our context as we already have relatively low power due to sub-setting our field experiment sample to only those who pre-committed in stage one. Moreover, the BKY adjustment is a widely used approach in experimental and environmental economics (for example, see Bonan et al., 2021; Dorner, 2019; Feine et al., 2023; Gosnell & McCoy, 2023). In our results, we will also report the more conservative BH test results (Benjamini & Hochberg, 1995).

5.5.6. Random assignment notes

As we note earlier, attendance at a volunteering event is not randomly assigned. However, we can create a plausibly exogenous control group by looking only at respondents that pre-committed to an event and conditioning on other important factors that predict self-selection into attending an event.

These important factors include general availability (number of days in the month that an individual is available), treatment in stage one, demographics and environmental attitudes. By selecting only those who pre-committed, we reduce the differences between those who attend and do not attend. Furthermore, they may be a group of individuals who pre-committed initially with no intention of following up. These individuals will clearly differ from those who self-select into our treatment (attending an event). Fortunately, these individuals are also less likely to respond to our second survey, which means they will automatically be excluded from our analysis (this type of sorting behaviour has been observed in other contexts - Andreoni et al., 2017). We should, therefore, expect to have reasonable balance between those who pre-committed and attended (treatment) and those who pre-committed, did not attend and responded to survey two.

Nonetheless, it is likely that those who attend may have higher general availability than those who do not attend, because availability increases the probability of being able to attend one of our specific events. They may also be more likely to belong to a particular randomly assigned treatment group in stage one (if one treatment is more effective than others). If we control for these exogenous factors, as well has demographics and environmental attitudes, the remaining variation in attendance is plausibly random and arises from the following types of situations (these are examples, not an exhaustive list):

- People being available on Tuesdays but not Wednesdays (when the weekday events were held)
- People being available on Sundays but not Saturdays (when the weekend events were held)
- People having one-off commitments on the specific days of the events
- People being out of town during the week the volunteering events are offered
- People are unable to attend because of sudden family emergencies
- People are unable to attend because of other sudden and unexpected issues or events.

These are all plausibly random factors governing whether an individual is treated (attends an event) or not. Importantly, these are not made-up factors – these were all actual reasons reported by respondents for not being able to attend an event. When we perform our analyses, we will check for balance across out treatment and control groups pre and post weighting for availability and treatment status in stage one before proceeding.

5.6. Results

5.6.1. Demographic descriptive statistics

We start by presenting the demographics of our panel dataset (Table 13). These variables do not change between the two surveys (survey one and two). The summary statistics are very similar to the stage one summary statistics which contain the full set of respondents for survey one (while this table only presents the statistics for participants that responded to both surveys). This suggests that attrition is reasonably random. One key difference is the proportion of first-time volunteers who respond to survey two (70.8%), relative to the proportion of those already volunteering who respond to survey two (86.9%). This is consistent with the ideas presented in Chapter 4. Completing a second survey about volunteering and pro-environmental behaviour is a costly exercise and those with higher pro-environmental motivation are more likely to complete this task. And as we show in stage one, those already volunteering scored higher on average on all of the pro-environmental attitudes and behaviour scales.

Table 13. Demographic summary statistics for participants who respond to survey one and two.

	Full (N = 557)		First-time vol. (N = 444)		Already vol. (N = 113)	
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Age	44	16	44	16	45	17
Māori and Pacific Ethnicity	20%	-	21%	-	14%	-
Bachelors or higher	60%	-	58%	-	65%	-
Income (perceived)						
Low income	24%	-	25%	-	19%	-
Middle income	64%	-	63%	-	65%	-
High income	13%	-	12%	-	16%	-
Gender						
Female	71%	-	74%	-	57%	-
Male	28%	-	25%	-	39%	-
Gender diverse	2%	-	1%	-	4%	-
Employment status						
Full time	47%	-	47%	-	49%	-
Student	7%	-	7%	-	7%	-
Retired	12%	-	12%	-	12%	-
Part time	16%	-	16%	-	18%	-
Other employment	17%	-	18%	-	15%	-
Geographic location						
Resides outside Hamilton City	16%	-	15%	-	23%	-
Resides near Fairfield	17%	-	17%	-	16%	-
Children						
Has a child	35%	-	36%	-	29%	-
Has a child under 14 yrs	27%	-	29%	-	22%	-
Other volunteering						
Never volunteers elsewhere	37%	-	40%	-	24%	-
Infrequently volunteers elsewhere	40%	-	38%	-	51%	-
Sometimes volunteers elsewhere	12%	-	13%	-	9%	-
Frequently volunteers elsewhere	11%	-	10%	-	16%	-

5.6.2. Descriptive statistics for attitudinal, wellbeing and identity variables

Below, we record the overall summary statistics for our attitudinal, wellbeing and identity variables in survey one and survey two (these are supplementary outcome variables for stage two). The results are from the balanced panel dataset (N = 557 total). This allows us to observe any general trends in these outcome variables for the whole sample (including those who are already volunteering). This is useful to help understand how much these variables change over time (a one month period), which is often overlooked in the literature.

Table 14 shows that on average, the outcome variables do not change much between the two waves. This was generally expected given that the surveys took place only a month apart and many of the outcome variables measure deeper attitudes or beliefs that tend to be relatively stable (like the concept of connection to nature - Clayton & Opotow, 2003; Mayer & Frantz, 2004). We note that these results do not imply that there weren't individual shifts or shifts within sub-groups (our treatment group, for example), which we will explore in the later sections.

Table 14 is sorted by the absolute value of the standardised mean difference (SMD) between survey one and two. The largest changes were in knowledge and perceptions of community groups (increased 0.19 and 0.25 SDs from survey one, respectively) and the Who-5 wellbeing score (increased 0.20 SDs). On the other hand, EID, willingness to volunteer, connection to community and connection to nature saw the smallest changes between surveys, on aggregate. Again, this is consistent with the variables that measure deeper concepts of values, identity and attitudes, that are unlikely to change over a short period of time with no intervention.

Table 14. Differences in attitudinal, wellbeing and identity variables between survey one and two.

	Survey one		Survey two		Change
Variable	Mean	Std. Dev.	Mean	Std. Dev.	SMD
Perceptions of restoration groups	5.4	1.2	5.7	1.1	0.25
Who5 index	14	5.1	15	5.1	0.2
Knowledge of restoration groups	3.1	1.6	3.4	1.5	0.19
Donation value	$24	$27	$28	$28	0.15
Locus of control scale	5.2	0.9	5.3	0.91	0.11
PEB Index	4.9	0.87	5	0.84	0.11
Connection to nature	5.8	1.1	5.7	1.1	-0.09
Connection to community	4.4	1.5	4.5	1.4	0.07
Willingness to volunteer (restoration)	5	1.4	4.9	1.6	-0.07
EID scale	5.6	0.92	5.6	0.91	0

Note: Donation percentage is not included because we can not calculate a comparable SMD. However, donation percentage increases from 52% in survey one on average to 57% in survey two.

5.6.3. Causality assumption

In this section, we provide evidence that supports our claim that treatment assignment is plausibly independent of potential outcomes, given our weights. This is the main assumption we need to satisfy to identify the causal effects of a first-time experience volunteering on future behaviour and other outcomes.

Predicting self-selection into the treatment group

Our assumptions for conditional exogeneity rely on availability and voucher assignment being the key factors predicting self-selection into our treatment group (attending a volunteering event). As we argue in the methods section and in Chapter 3, once we control for availability and voucher assignment, the remaining variation should arise from random differences in availability which affect one's ability to attend an event.

While we also control for demographics and attitudes in our weights, if these are major predictors of treatment assignment, it raises concerns that there might be other attitudinal or underlying demographic variables that were not observed but affect both treatment assignment and our outcome variables (creating omitted variable bias). It would also call into question the argument that a significant portion of the variation is due to random idiosyncratic differences in availability.

To examine the factors predicting treatment assignment, we run regression models of treatment assignment (attendance at one of our events) on our full set of control variables. This includes our availability variable and stage one voucher dummy variable. As outlined in Chapter 4, our preferred modelling specification for binary dependent variables is the linear probability model (LPM). However, inverse probability weights are usually estimated using non-linear models to ensure probability weights fall with the unit interval [0,1]. Therefore, we present the average marginal effects (AMEs) of regressors on treatment probability from LPM and logistic (logit) regression models (Table 15). This also serves as a useful explicit comparison of the AMEs obtained using the LPM and logit models.

Moreover, we run an additional LPM and logit model with only the availability and voucher variables – the two key hypothesised predictors of treatment self-selection. We can compare these with the full models to see if the additional covariates improve our predictive power.

Our results in Table 15 are clear – availability and voucher assignment are the most important predictors of treatment assignment. This is consistent with our expectations and justification for conditional exogeneity. All the coefficients on the demographic and attitudinal controls are insignificant at the 5% level. While the adjusted R^2 is slightly better for the LPM with controls (0.081 vs 0.063), the AIC is higher for the logit model with controls (224.2 vs 212.3) which indicates a worse model fit with controls. We also perform LR tests between the two logit models and the two LPM models (McFadden, 1987). We find that adding controls does not improve model fit (p-value of 0.198 for the LPM models and 0.179 for the logit models). These results lead us to conclude that availability and voucher assignment are indeed the main factors predicting self-selection into the treatment group. The generally low R^2 values also support the notion that there is significant underlying random variation in treatment assignment.

Table 15. Regression models predicting treatment assignment

	Dependent variable: Treatment (attendance at a volunteering event)			
	Full model		Voucher and availability only	
	LPM	Logit	LPM	Logit
	(1)	(2)	(3)	(4)
Voucher	0.190***	0.187***	0.186***	0.193***
	(0.063)	(0.063)	(0.060)	(0.065)
Days available	0.019***	0.018***	0.016***	0.014***
	(0.006)	(0.005)	(0.006)	(0.005)
LOC index	-0.036	-0.037		
	(0.043)	(0.042)		
PEB index	0.001	0.004		
	(0.044)	(0.043)		
Who5 score	0.012*	0.013*		
	(0.007)	(0.007)		
Other volunteering	-0.023	-0.011		
	(0.076)	(0.076)		
Male	0.003	0.007		
	(0.083)	(0.074)		
Low income	-0.013	-0.022		
	(0.085)	(0.079)		
High income	-0.104	-0.130		
	(0.108)	(0.097)		
Maori/Pacific	-0.024	-0.020		
	(0.075)	(0.079)		
Full time	0.058	0.071		
	(0.096)	(0.097)		
Student	0.204	0.195		
	(0.132)	(0.125)		
Retired	0.119	0.106		
	(0.144)	(0.137)		
Part time	-0.162	-0.173		
	(0.101)	(0.128)		
Bachelors or higher	-0.019	-0.021		
	(0.069)	(0.068)		
Age	0.001	0.001		
	(0.003)	(0.003)		
Outside Hamilton City	-0.139*	-0.152		
	(0.083)	(0.112)		
Near Fairfield	-0.085	-0.096		
	(0.082)	(0.094)		
Children dummy	-0.041	-0.052		
	(0.071)	(0.068)		
Model Constant	0.084	-2.411	0.086*	-1.987
	(0.327)	(1.989)	(0.046)	(2.084)
Observations	189	189	189	189
R^2	0.174	-	0.073	-
Adjusted R^2	0.081	-	0.063	-
AIC	-	224.2	-	212.3

*Note: * p < 0.10, ** p < 0.05, *** p < 0.01. BM robust SEs in parentheses for the LPM models. Logit SEs in parentheses. The controls used here are our standard set – see stage 1.*

Signalling and unobservables

Another potential threat to our conditional exogeneity assumption ($Y_{0i}, Y_{1i} \perp Z_i | w_i$) is that there are some underlying factors that we cannot observe that mean some people will pre-commit with no intention of actually following through. Bias in self-reported measures of pro-environmental behaviour is an issue frequently discussed in the literature and is often caused by things like social desirability bias and signalling effects (Koller et al., 2023; Kormos & Gifford, 2014).

However, our design minimises the threats from these types of individuals because the people who mis-represent their intentions or try to signal that they are an environmentally friendly person (Bénabou & Tirole, 2011; Brent et al., 2017) are also probably more likely to self-select out of future communications from our research team. These respondents may feel guilty about mis-representing their intentions and as Truelove et al. (2014) argues guilt is one of the leading causes of negative spillovers (so, for example, they may not engaging in future research communications about pro-environmental behaviour). Moreover, in a different context, DellaVigna et al. (2012) find that people who feel social pressure to behave in a particular way will opt out of situations where they may feel that social pressure. In their study, DellaVigna et al. (2012) show that given the chance, people will deliberately make sure they are not home to avoid having to decide whether to give money to a door-to-door charitable fundraiser.

We find evidence for a similar phenomenon in our study. We look at attrition rates between four distinct groups:

- Those already volunteering
- First-time volunteers who did not pre-commit
- First-time volunteers who pre-committed and then avoided our commitment surveys
- First-time volunteers who pre-committed and followed up with our commitment surveys

The idea here is that we can identify individuals that, shortly after completing survey one (two weeks on average), did not follow-up on their pre-commitment. As we discuss in Chapter 3, we ask every pre-committed individual to tell us whether they can make either of the specific volunteering events available to them. Responding to us takes less than two minutes and we very clearly ask everyone to reply, regardless of whether they can attend or not. If an individual had no intention of following up on their pre-commitment, they will be unlikely to respond to our commitment requests. As we show in Figure 18, they are also considerably less likely to complete survey two.

We can see that those who pre-commit and do not follow up are significantly less likely to participate in survey two than those who pre-commit and follow up (29.5% vs 79.4%). Moreover, these individuals are also less likely to respond to survey two than those who did not pre-commit at all (and likely have lower pro-environmental motivation).

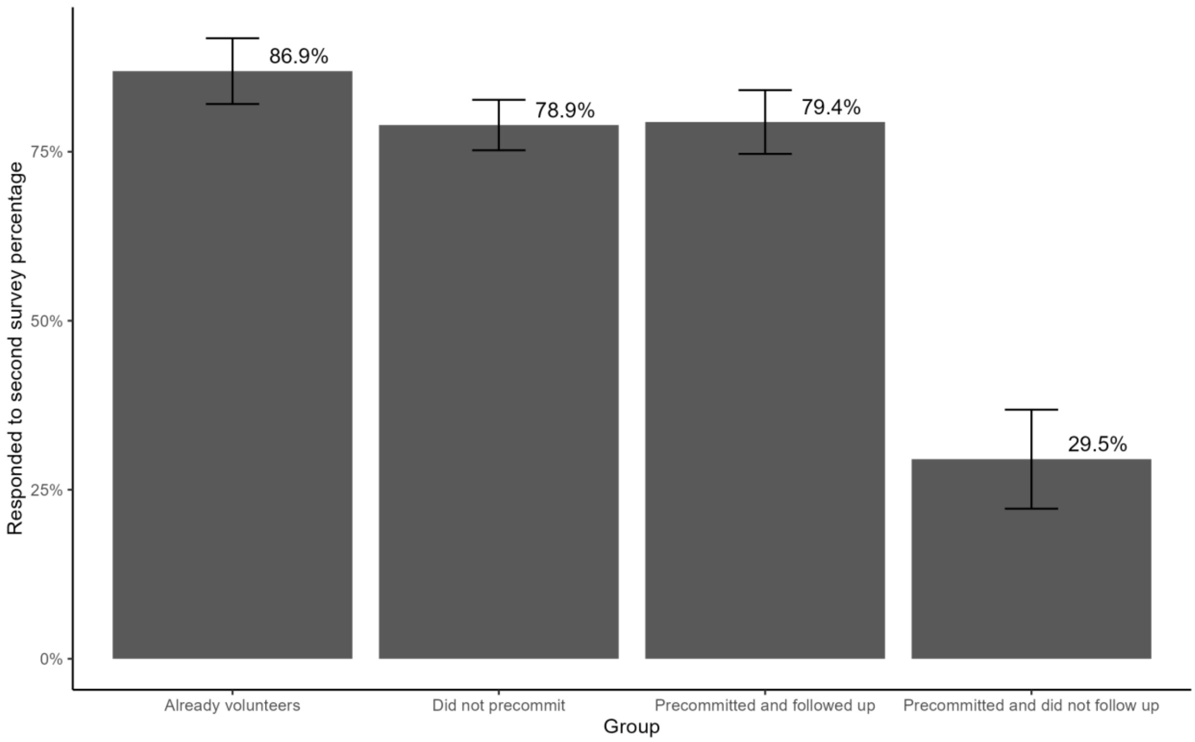

Figure 18. Survey 2 response rates based on precommitment status. The $\chi 2$ for the difference between those who pre-committed and followed up vs pre-committed and did not follow up is 72.693 and significant at the 1% level.

Additionally, we look at the differences between these four groups in terms of their PEB, EID and environmental LOC indices. We do this to check whether those who pre-committed and did not respond report high levels of pro-environmental motivation, which would be consistent with signalling effects. That is, they pre-committed to remain consistent with their self-reported pro-environmental identity but had no real intention of behaving pro-environmentally (Truelove et al., 2014).

Table 16 shows that those who do not pre-commit consistently rank the lowest on the EID, LOC and PEB indices. Those who are already volunteering tend to score higher on the indices – this is consistent with our earlier summary statistics. On the other hand, there is very little difference between those who pre-commit and follow up and those who pre-commit and do not follow up. Indeed, those who do not follow up score higher (albeit insignificantly higher) on the EID index. This shows that the individuals who do not follow up do have a relatively strong sense of environmental identity and pro-environmental attitudes.

Overall, Figure 18 and Table 16 suggest that individuals engaged only in social signalling in survey one are significantly less likely to respond to survey two and are therefore automatically excluded from our stage two analyses. The weighted control group for the stage two testing consists only of individuals that pre-committed and then responded to our second survey.

Table 16. Environmental attitudes and behaviour statistics from survey one for four groups.

		EID Index (1-7)		Env. LOC Index (1-7)		PEB Index (1-7)	
Group	N	Mean	SD	Mean	SD	Mean	SD
Already volunteers	130	5.91	0.90	5.35	0.92	5.13	0.86
No pre-commit	323	5.30	0.92	5.03	0.95	4.69	0.88
Pre-commit + follow up	199	5.69	0.91	5.44	0.77	5.02	0.79
Pre-commit + no follow up	105	5.75	0.83	5.23	0.94	5.04	0.88

5.6.4. Treatment effects on future volunteering

Over the following sections, we present our weighted hypothesis testing results. We achieved good balance between our treatment and weighted control group on availability and voucher assignment (the two key predictors of treatment self-selection – see previous section) and our full set of stage one controls. We report the balancing statistics in Appendix M.

We report on weighted non-parametric chi-squared tests to examine whether attending one of our events (being treated) increases future volunteering behaviour. This is to test hypothesis H2.1.

As we do in stage one, we measure volunteering behaviour in three ways (see Chapter 3 for full details):

1) Pre-commitment to an upcoming event in March (before date is known).
2) Commitment to an upcoming event in March (once date is known).
3) Attendance at the event in March (or another nature volunteering event).

Unlike stage one, we provide no incentives or encouragement to any of the participants.

Table 17 reports the results of our one-sided chi-squared tests. We find strong evidence that those who have a first-time experience volunteering in our study are significantly more likely to pre-commit, commit and attend future volunteering events. This finding is conditional on balancing pre-existing environmental attitudes, demographics, availability and treatment status in the stage one (this is done by the weights). We present a summary of these results graphically in Figure 19.

The relative magnitude of the effect is greatest for attendance, where the probability of attending a future event increases by 17% (which is a more than six-fold increase in the probability of attending a future event). The probability of pre-committing increases by 15.2%, which is a 19% relative increase compared to the control. The probability of committing to an event increases by 20.3%, which is a 44% increase in probability relative to the control,

These results are consistent with our theoretical model which predicts that providing a first-time experience volunteering will crowd in future volunteering behaviour for some individuals. It is also consistent with the literature on experimentation and satisfying, which we discuss in Chapter 2 (Caplin et al., 2011; Larcom et al., 2017).

While the percentage of individuals volunteering in the future is relatively small overall, this is only captured over a short time-frame (2-4 weeks). The true effect size could be larger over a longer period of time because more respondents are able to find times where they are free that align with local volunteering activities.

Table 17. Effects of attending a first-time volunteering event on future volunteering behaviour.

	Mean for group		*Test output*	
Future behaviour	Control	Treatment	χ^2	p-value
Pre-commitment	78.9%	94.1%	5.035	0.0075***
Commitment	46.4%	66.7%	6.141	0.0065***
Attendance	2.6%	19.6%	8.721	<0.001 ***

*Note: These are one-sided chi-squared proportion tests in-line with our pre-registered hypotheses. Sample sizes are N = 138 for the control group and N = 51 for the voucher group. The effective sample size for the control after weighting is N = 111. * p < 0.10, ** p < 0.05, *** p < 0.01.*

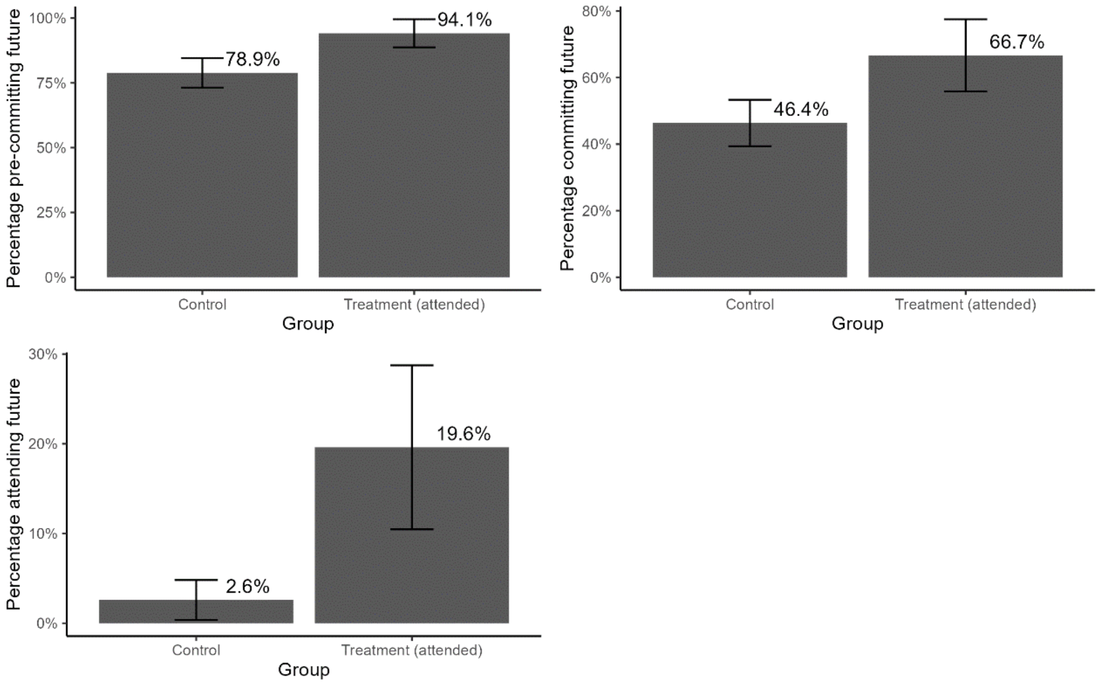

Figure 19. Future attendance rates by treatment group in stage 2. Error bars show 90% CI.

5.6.5. Heterogenous treatment effects on future volunteering

To explore hypotheses H2.2 and H2.3, which hypothesise about treatment effect heterogeneity, we run regression models predicting future volunteering behaviour and use interactions to explore the heterogeneity described in the pre-registered hypotheses.

As we do earlier, we present the results for the outcome of pre-commitment, commitment and then attendance separately. In each table (Tables 18 to 20), column one reports a basic LPM using treatment to predict the volunteering outcome variable. In column two, we report the same LPM but weight on the probability of being treated (weighted least squares regression). In column three, we run the same model but include dummy variables for whether the individual was offered a voucher in stage one and whether they live near Fairfield. These are the two variables hypothesised to drive treatment effect heterogeneity in hypotheses H2.2 and H2.3. In column four, we interact treatment status and being offered a voucher in stage one – this addresses hypothesis H2.2. In column five, we interact treatment status with the close to Fairfield dummy – this addresses hypothesis H2.3.

Over all three outcomes, we find null and generally precise null results for hypothesis H2.3.[32] The effects of a first-time experience volunteering on future volunteering behaviour are not mediated by whether the individual was offered a voucher or not in stage one. We note that in the commitment and attendance models (Tables 21 and 22), the standard errors on the treatment and voucher interactions increase dramatically (which causes the main effect, while similar in absolute magnitude, to become insignificant). This is because there are substantially fewer individuals who were not offered a voucher and attended an event (were treated – this comes directly from our stage one results that show how impactful the vouchers are).

In general, it is important to note that our power to detect interaction effects diminishes significantly when looking at the future commitment or attendance models (given the small sample sizes).

Fairfield interaction

For hypothesis H2.2, we find no statistically significant evidence that living near Fairfield has an impact on future pre-commitment and commitment to volunteer. However, we find that living near Fairfield reduces the effect (to zero) of being treated on future attendance at volunteering events. This was surprising to us, given our pre-registered hypothesis H2.2. This does not support the idea that living closer to the community group where the first experience occurs increases enjoyment and thus volunteering "re-occurrence". While this could still be happening, there are clearly other factors influencing the results that are more influential.

We looked at our stage two sample (first-time volunteers that pre-committed in stage one) to see if there were any noticeable differences between those who live near Fairfield and the rest of the sample. We found that on average, those living near Fairfield had a lower pre-existing general willingness to engage in nature volunteering in survey one (F stat of 2.38, not statistically significant). They also have a lower willingness to engage in nature volunteering in survey two (F stat of 7.15, statistically significant at the 1% level).

[32] When we say precise, we mean that the coefficients themselves are close to zero. This is in contrast to a situation where coefficients are moderate to large in magnitude, but are statistically insignificant due to a lack of power.

There was also evidence to suggest those living near Fairfield had lower incomes on average, with 37% self-reporting being low income, relative to 25% in the rest of the sample (F stat of 2.08, not significant). Significantly more participants in and around Fairfield stated that they worked in part-time positions (F stat of 4.01, significant at the 5% level). And in both surveys, those in or near Fairfield donated less often (not significant) and chose to donate lower values (F stat of 3.34 for survey one and 5.54 for wave two, significant at the 10% and 5% levels respectively). These trends in donation behave likely reflect the lower incomes of those in or near Fairfield and their generally lower willingness to volunteer for nature restoration groups. These factors may explain why there is no treatment effect for those from Fairfield, although further work is needed to explore this area.

Another noteworthy point is that the Fairfield interaction results may be a positive sign for more general and scalable behaviour change initiatives. We find that the crowding in of future volunteering behaviour occurs primarily in attendees that did not live near the nature restoration group. This may imply that semi-centralised "experimentation events" (events where people can try a new activity or behaviour) may have significant positive spillovers to future behaviour. That is, people do not necessarily need to live right beside the location of the "experimentation event" in order to generate positive spillovers. Future researchers may want to test such approaches, coupling a central "experimentation event" with more localised information after the event. In our case, we could have told individuals where and when their local nature restoration groups operated, based on the geographic location information they provided to us. We did not do this specifically, but we did add those who were willing to an email list to hear about other local nature volunteering opportunities.

Table 18. Stage two interaction models for the effects of treatment on future pre-commitment.

	Future pre-commitment				
	(1)	(2)	(3)	(4)	(5)
Treatment	0.166^{***}	0.152^{***}	0.156^{***}	0.210^{***}	0.150^{***}
	(0.049)	(0.050)	(0.051)	(0.058)	(0.056)
Voucher			-0.024	-0.006	-0.025
			(0.059)	(0.076)	(0.059)
Fairfield			0.025	0.026	0.014
			(0.077)	(0.077)	(0.096)
Treatment*Voucher				-0.076	
				(0.089)	
Treatment*Fairfield					0.053
					(0.104)
Intercept	0.775^{***}	0.789^{***}	0.800^{***}	0.788^{***}	0.802^{***}
	(0.036)	(0.038)	(0.052)	(0.060)	(0.054)
IPW Weights	NO	YES	YES	YES	YES
Observations	189	189	189	189	189
R^2	0.037	0.032	0.034	0.036	0.034
Adjusted R^2	0.032	0.027	0.018	0.015	0.013
Residual Std. Error	0.379 (df = 187)	0.372 (df = 187)	0.373 (df = 185)	0.374 (df = 184)	0.374 (df = 184)
F Statistic	7.128^{***} (df = 1; 187)	6.261^{**} (df = 1; 187)	2.160^{*} (df = 3; 185)	1.694 (df = 4; 184)	1.630 (df = 4; 184)

*Note: * p < 0.10, ** p < 0.05, *** p < 0.01. BM robust SEs in parentheses.*

Table 19. Stage two interaction models for the effects of treatment on future commitment.

	Future commitment				
	(1)	(2)	(3)	(4)	(5)
Treatment	0.217^{***}	0.203^{**}	0.206^{**}	0.179	0.224^{**}
	(0.079)	(0.082)	(0.084)	(0.151)	(0.089)
Voucher			0.001	-0.008	0.003
			(0.082)	(0.097)	(0.082)
Fairfield			0.054	0.053	0.086
			(0.110)	(0.111)	(0.124)
Treatment*Voucher				0.039	
				(0.182)	
Treatment*Fairfield					-0.160
					(0.284)
Intercept	0.449^{***}	0.464^{***}	0.454^{***}	0.460^{***}	0.448^{***}
	(0.042)	(0.048)	(0.070)	(0.076)	(0.071)
IPW Weights	NO	YES	YES	YES	YES
Observations	189	189	189	189	189
R^2	0.037	0.033	0.034	0.034	0.036
Adjusted R^2	0.032	0.027	0.018	0.013	0.015
Residual Std. Error	0.493 (df = 187)	0.494 (df = 187)	0.496 (df = 185)	0.498 (df = 184)	0.497 (df = 184)
F Statistic	7.236^{***} (df = 1; 187)	6.295^{**} (df = 1; 187)	2.170^{*} (df = 3; 185)	1.630 (df = 4; 184)	1.715 (df = 4; 184)

*Note: * p < 0.10, ** p < 0.05, *** p < 0.01. BM robust SEs in parentheses*

Table 20. Stage two interaction models for the effects of treatment on future attendance.

	Future attendance				
	(1)	(2)	(3)	(4)	(5)
Treatment	0.160^{***}	0.170^{***}	0.167^{***}	0.091	0.187^{***}
	(0.058)	(0.058)	(0.057)	(0.099)	(0.063)
Voucher			-0.006	-0.032	-0.004
			(0.035)	(0.028)	(0.035)
Fairfield			-0.068^{***}	-0.070^{***}	-0.031^{**}
			(0.022)	(0.023)	(0.015)
Treatment*Voucher				0.108	
				(0.122)	
Treatment*Fairfield					-0.186^{***}
					(0.064)
Intercept	0.036^{**}	0.026^{**}	0.040	0.057^{**}	0.033
	(0.016)	(0.013)	(0.028)	(0.025)	(0.028)
IPW Weights	NO	YES	YES	YES	YES
Observations	189	189	189	189	189
R^2	0.069	0.086	0.094	0.101	0.104
Adjusted R^2	0.064	0.081	0.079	0.082	0.084
Residual Std. Error	0.262 (df = 187)	0.248 (df = 187)	0.248 (df = 185)	0.248 (df = 184)	0.248 (df = 184)
F Statistic	13.838^{***} (df = 1; 187)	17.510^{***} (df = 1; 187)	6.400^{***} (df = 3; 185)	5.176^{***} (df = 4; 184)	5.338^{***} (df = 4; 184)

*Note: * p < 0.10, ** p < 0.05, *** p < 0.01. BM robust SEs in parentheses.*

5.6.6. Supplementary hypothesis testing results

In this section, we report the effects of treatment on our supplementary outcomes by using non-parametric weighted hypothesis testing (chi-squared tests for binary and categorical variables and Mann-Whitney U test for continuous variables). As effect directions for each of these variables were explicitly pre-registered, we perform one-sided hypothesis tests. Moreover, as we are testing a relatively large number of additional outcomes (N = 11), we use multiple hypothesis testing corrections in this stage (see methods section).

In Table 21, we present the weighted differences between the treated and control respondents for stage two across our supplementary outcomes. The largest differences between the groups are in willingness to volunteer, perceptions of restoration groups and knowledge of restoration groups (differences are between 0.5 and 0.75 SDs). We also find small-medium sized differences in EID, environmental LOC, wellbeing and self-reported PEB (0.2-0.3 SDs). Moreover, the treated respondents appear to donate more often than the untreated respondents (74.5% vs 58.5%), however, donation value is relatively constant.

We find very small to no difference in connection to nature and connection to community. These questions target much deeper feelings and attitudes and may be why we see little change over the short run. We evaluate the significance of these differences in Table 22 with multiple hypothesis testing corrections.

Table 21. Summary statistics for the weighted differences between the stage two treatment and control groups in the full set of supplementary outcomes.

Variables survey 2	Control (N = 138) Wt. Mean	Control (N = 138) Wt. SD	Treatment (N = 51) Wt. Mean	Treatment (N = 51) Wt. SD	Difference in Std. devs
EID index	5.64	0.902	5.84	0.728	0.22
LOC index	5.42	0.818	5.66	0.699	0.29
Who5 score	14.5	4.84	15.9	5.04	0.29
Willingness to volunteer (restoration)	5.44	1.12	6.14	1.22	0.62
Knowledge of restoration groups	3.05	1.23	3.73	1.37	0.55
Perceptions of restoration groups	5.62	1.03	6.39	0.874	0.75
Connection to nature	5.86	1.03	5.82	0.994	-0.04
Connection to community	4.47	1.36	4.51	1.29	0.03
PEB index	5.1	0.7	5.29	0.722	0.27
Donation binary	58.5%	-	74.5%	-	-
Donation value	28.4	28.8	31.1	25.3	0.09

Note: The difference in standard deviation units is the weighted mean of the treatment minus the weighted mean of the control, divided by the weighted standard deviation of the control.

In Table 22, we report the one-sided hypothesis testing results using our preferred BKY multiple hypothesis corrections and the more conservative BH corrections. We find support for most of our hypotheses and show that having a first-time experience volunteering increases:

- Environmental self-identity (EID)
- Environmental locus of control (LOC) beliefs
- Short-term wellbeing
- Willingness to volunteer for a restoration group
- Knowledge of restoration groups
- Positive perceptions of restoration groups
- Self-reported pro-environmental behaviour (PEB)
- Willingness to donate

We have the strongest support for the hypotheses that having a first-time experience volunteering increases willingness to volunteer, perceptions of restoration groups and knowledge of such groups. The smallest significant difference is in the EID index, which increases by 0.22 SDs.

Our results are promising for nature restoration groups, as treated individuals report higher levels of willingness to volunteer for these groups, have more knowledge of these groups and have generally higher perceptions of these groups. Wellbeing is another key outcome that increases following a first-time experience volunteering (we see a moderate increase in the Who-5 scale of 0.29 SDs). This reinforces the limited (but growing) literature on the causal effects of both volunteering and spending time in nature on wellbeing (Dolan et al., 2021; Meier & Stutzer, 2008; M. P. White et al., 2019).

We also show that a first-time experience volunteering does not increase connection to community and nature. This makes sense as these affective measures of connection run much deeper to the core and identity of an individual than the other outcomes we measure. This also further supports our weighting approach and methodology, because we do not see differences in deep underlying connections and values between the treatment and control groups.[33]

On the other hand, we do see a small increase in the EID index, which focuses on self-identification as an environmentally friendly person. However, there is wide recognition that environmental self-identity (EID) and affective measures of identity, like Mayer & Frantz' (2004) connectedness to nature scale or Clayton & Opotow's (2003) environmental identity scale, are fundamentally different concepts (van der Werff et al., 2013). The latter affective measures capture fundamental beliefs and values towards the environment and nature, while the self-identity scale captures self-perceptions of environmental friendliness. Moreover, our results are consistent with the literature that shows if you make environmental self or social identity more salient, pro-environmental behaviour tends to increase (to remain consistent with ones identity - Bénabou & Tirole, 2011; Bonan et al., 2021; Truelove et al., 2014). We show the opposite may be true – after individuals engage in a costly pro-environmental behaviour (more costly than say, recycling), their environmental self-identity increases. This is an encouraging sign, because if environmental self-identity increases now and that increase is sustained, individuals will be more likely to engage in other pro-environmental behaviours in the future (Alacevich et al., 2021; Truelove et al., 2014). This mirrors findings from Gneezy et al. (2012) who show that costly pro-social behaviours (like volunteering) serve as larger signals of pro-social identity than low-cost behaviours. This subsequently crowds in future pro-social behaviour as individuals act to remain consistent with their new self-perceptions (A. Gneezy et al., 2012). Moreover, results from Terry et al. (1999) show that environmental self-identity is an important predictor of pro-environmental behaviour, even after controlling for attitudes, subjective norms and locus of control beliefs (the core components of the Theory of Planned Behaviour - Ajzen, 2011).

While we can not track longer-term changes in pro-environmental behaviour and environmental identity, we do show that the both the PEB index and the environmental donation probability increases following a first-time experience volunteering. This supports our suggestions above, indicating that

[33] If we did see such differences, this would suggest that there are underlying differences in our groups that were not captured appropriately by our weights.

there appears to be positive spillovers to other pro-environmental behaviours. These wider positive spillovers are what we would expect to see if environmental identity had strengthened.

We would also expect to see these spillovers from strengthening locus of control beliefs (LOC), which we find strong support for in our results (Table 22). Again, the fact that we see increases in environmental LOC beliefs is strongly encouraging. LOC beliefs are well known as key drivers of pro-environmental and pro-social behaviour, so increasing LOC beliefs may generate wider behaviour change and crowd in pro-environmental policy support (Andor et al., 2022; Hines et al., 1987; M. Kim et al., 2022). LOC beliefs are often seen as a necessary pre-cursor to action, because if one believes their actions will not have an impact on environmental issues, they are unlikely to engage in costly pro-environmental behaviours because they will perceive very little benefit from doing so (M. Kim et al., 2022). In an age of increasing climate worry and anxiety, maintaining and strengthening LOC beliefs are more important than ever to ensure people have hope and belief that their individual actions can make a difference (Ojala et al., 2021; Whitmarsh et al., 2022).

Table 22. Supplementary hypothesis testing results.

	Wtd. mean for group		*p-values from one-sided test*		
Future behaviour	Control	Treatment	Naïve	BKY adj.[34]	BH adj.
EID index (1-7)	5.64	5.84	0.088*	0.048**	0.121
LOC index (1-7)	5.42	5.66	0.035**	0.047**	0.061*
Who5 score (0-25)	14.5	15.9	0.030 **	0.047**	0.061*
Willingness to vol. (restoration) (1-7)	5.44	6.14	<0.001***	<0.001***	<0.001***
Knowledge of restoration groups (1-7)	3.05	3.73	0.0015***	0.0048***	0.0058***
Perception of restoration groups (1-7)	5.62	6.39	<0.001***	<0.001***	<0.001***
Connection to nature (1-7)	5.86	5.82	0.374	0.120	0.412
Connection to community (1-7)	4.47	4.51	0.437	0.120	0.437
PEB index (1-7)	5.1	5.29	0.039**	0.047**	0.061*
Donation binary	58.5%	74.5%	0.023**	0.047**	0.061*
Donation value ($)	28.4	31.1	0.187	0.076*	0.228

*Note: These are one-sided Mann-Whitney U non-parametric test results, except for the test of binary donation behaviour which uses a chi-squared proportion test (χ^2 = 3.99). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

5.6.7. Robustness check for supplementary hypothesis testing

As a robustness check for the supplementary hypothesis testing, we re-evaluate each hypothesis conditioning on the exact value of the outcome variable in survey one (lagged outcome variable). We report our key AME results for the lagged dependent variable (LDV) modelling (see methods) in Table

[34] Note: As Anderson (2008) notes, the sharpened p-values from the BKY FDR adjustment can be lower than the naïve p-values.

23. We include the weights in all of these LDV regressions, use BM robust standard errors and report the one-tailed p-values. We perform multiple hypotheses corrections as before.

The LDV regression results are largely consistent with the base results in Table 22 and if anything, our findings become more strongly supported under our robustness check. For example, the increase in EID is now significant at the 5% level across all p-value calculation approaches.

Table 23. Supplementary hypothesis testing results from LDV regressions.

	Regression output		*p-values from one-sided test*		
Future behaviour	AME	Std. error	Naïve	BKY adj.[35]	BH adj.
EID index (1-7)	0.17	0.09	0.034**	0.026**	0.047**
LOC index (1-7)	0.27	0.10	0.003***	0.006***	0.009***
Who5 score (0-25)	1.11	0.56	0.025**	0.026**	0.039**
Willingness to vol. (restoration) (1-7)	0.17	0.10	<0.001***	<0.001***	<0.001***
Knowledge of restoration groups (1-7)	0.60	0.18	<0.001***	0.001***	0.002***
Perception of restoration groups (1-7)	0.77	0.16	<0.001***	<0.001***	<0.001***
Connection to nature (1-7)	0.02	0.14	0.432	0.069*	0.432
Connection to community (1-7)	0.12	0.16	0.233	0.061*	0.256
PEB index (1-7)	0.17	0.09	0.022**	0.026**	0.039**
Donation binary	0.13	0.06	0.018**	0.026**	0.039**
Donation value ($)	$3.66	$3.49	0.148	0.058*	0.180

*Note: BM robust standard errors. * p < 0.10, ** p < 0.05, *** p < 0.01.*

5.7. Hypothesis testing conclusions

We use the preceding results to make conclusions about whether each of our stage two hypotheses are supported. test our main and additional hypotheses for stage one.

Main hypotheses

H2.1. *A first-time experience volunteering will lead to increases in future volunteering.*

Supported. Our results clearly show that being treated (having a first-time experience volunteering) significantly increases the probability of pre-committing and committing to and attending future volunteering events.

H2.2. *The effect size will be stronger for those who live near the community group where the event is held.*

Not supported. Our results show no difference in treatment effects for the pre-commitment and commitment outcomes between those who do and do not live near Fairfield. However, in our attendance modelling, we find that the effect is significantly lower (precisely zero) for those who live near Fairfield.

[35] Note: As Anderson (2008) notes, the sharpened p-values from the BKY FDR adjustment can be lower than the naïve p-values.

H2.3. *There is a difference in the treatment effect depending on if the volunteers came from a voucher group in stage one.*

Not supported. This is not supported across all three outcomes (pre-commitment, commitment and attendance). There are no differences in treatment effects based on whether the individual came from a voucher group in stage one. In saying that, our statistical power is low for these results.

H2.Sup *Being treated in stage 2 (attending one of our volunteering events) will increase the following outcomes:*

- *EID index:* ***Supported***
- *LOC index:* ***Supported***
- *Who5 score:* ***Supported***
- *Willingness to volunteer (restoration):* ***Supported***
- *Knowledge of restoration groups:* ***Supported***
- *Perceptions of restoration groups:* ***Supported***
- *Connection to nature:* ***Not supported***
- *Connection to community:* ***Not supported***
- *PEB index:* ***Supported***
- *Donation binary:* ***Supported***
- *Donation value:* ***Not supported***

Mostly supported. Of the eleven additional outcome variables, we find significant positive treatment effects on eight of them. This includes increases in environmental self-identity, LOC beliefs, the PEB index, willingness to volunteer and knowledge of restoration groups.

5.8. Theory discussion

The results from stage two provide further support for our theoretical model in Chapter 2. The first main hypothesis for stage two (H2.1) was derived from our theoretical model, which predicts that by providing a first-time experience volunteering, we can crowd in future volunteering behaviour for some individuals. We find strong support for this process. In our model, the underlying drivers of behaviour crowd-in (positive spillovers) were reducing uncertainty, inaccurately low estimates of the benefits of volunteering and helping individuals overcome adjustment costs. In the theory section, we also highlighted predictions that an experience volunteering in nature could also crowd-in future behaviour by shifting environmental values, attitudes and identity. While we are unable to distinguish between the relative weight of each of these channels, we find evidence to support both mechanisms.

5.8.1. Information mechanism

In our theoretical model and Chapter 3, we postulate that a first-time experience volunteering provides a suite of important experiential information that can help reduce uncertainty and correct inaccurately low priors as to the benefits of volunteering.

Our supplementary results for stage two show that knowledge and perceptions of nature restoration groups increase significantly following a first-time experience volunteering. These are the largest standardised increases out of all the supplementary outcome variables. This firstly highlights that uncertainty has likely been reduced, given participants report feeling more knowledgeable about nature restoration groups. Secondly, this suggests that prior perceptions of nature restoration groups were lower on average than the realised perceptions (after individuals have had an experience with a group). This supports the hypothesis that expectations of the benefits from volunteering were lower on average than the true value.

We find further support for the presence of inaccurately low prior estimates of the benefits from volunteering. Immediately after the volunteering events, we sent a text message and email to participants asking them to confidentially tell us about their experience volunteering. We had a good response from participants, with N = 49 completed experience surveys. We asked two questions of pertinence to the inaccuracy proposition.

Firstly, we asked participants to reflect on how enjoyable their experience volunteering was by asking them the following question:

"*Overall, how enjoyable was your experience volunteering?"*

Participants responded on a seven-point Likert scale from not at all enjoyable to very enjoyable. This question proxies for the private benefits of volunteering, given that feelings of enjoyment are one of the main benefits from volunteering (see Chapter 1 – for example, warm glow utility will likely manifest as enjoyment). The results show 12.2% of respondents enjoyed the experience and 87.8% found the experience very enjoyable.

We then asked participants:

"With respect to your level of ***enjoyment****, would you say that the experience volunteering was* ***below, met*** *or* ***exceeded*** *your expectations?"*

Participants also responded on a seven-point Likert scale from far below expectations to far exceeded expectations. This question looks at whether the experienced benefits (enjoyment) exceeded the individual's priors on average (as predicted by our theory). The important thing to note is that this is the first-time experience for our participants, so there will be inherent uncertainty in their expectations of the benefits of volunteering.[36] However, if their estimates were only uncertain and not inaccurately low, we would expect an approximately normal distribution of responses to the question above, with most responses being "met" expectations, some being "above" and some being "below".

We find that a minority say their experience met their expectations (16.3%) and everyone else (83.7%) reports having an experience that at least somewhat exceeded their expectations. Moreover, the majority of respondents (65.3%) reported having an experience that exceeded or far exceeded their expectations. There may have been a few respondents who had an experience below their expectations and this lead them to not respond to the experience survey. However, even if everyone who attended and did not respond to the experience survey had a below-expectations experience, the overwhelming majority would still have had an above expectations experience.

In addition, those who over-estimate the benefits (or at least, do not under-estimate them) are more likely to attend our events because their expected net benefits function would have been higher (Chapter 2). This should create a downward bias on the probability of seeing people who under-estimated the benefits given they attended an event. As such, our results could be interpreted as conservative and provide strong evidence that people had inaccurately low priors about the benefits of volunteering.

5.8.2. Environmental attitudes and spillovers mechanism

As we discuss in Chapter 3, a first-time experience volunteering in nature may strengthen environmental attitudes and identity, leading to positive spillovers on future volunteering behaviour. While we do not model this scenario explicitly in our theoretical model chapter (Chapter 2), this type of scenario can easily be included in our theoretical framework by shifting the underlying level of benefits one receives from engaging in volunteering. This would capture a shift in underlying attitudes and preferences, rather than revealing ones preferences or type (this aligns well with an emerging literature on endogenous preferences - Czajkowski et al., 2015; Mattauch et al., 2022).

Our results provide strong support for this mechanism too. While we find that a first-time experience is not sufficient to shift deeper connections to nature and community, we show that the experience does generate increases in environmental self-identity and environmental LOC beliefs. This is further supported by our results which show self-reported pro-environmental behaviour outside of volunteering (PEB index) increases and donation behaviour increases, both features we would expect to see if environmental attitudes more broadly have strengthened (Rosa & Collado, 2019; Truelove et al., 2014).

[36] This uncertainty is captured in our theoretical model.

There may also be wider positive spillovers beyond what we measure, like increased pro-environmental policy support (Sparkman et al., 2021).

We are unable to pinpoint the relative importance of changes in general environmental attitudes, environmental self-identity (EID) and environmental LOC beliefs in generating these positive spillovers. Based on the magnitudes alone for the effects of volunteering on EID and LOC beliefs, our results suggest the changes in LOC beliefs may be important for generating these spillovers. However, further research is needed to confirm this.

5.9. Conclusion

In this chapter, we exploit our novel experimental design to identify the causal effects of volunteering in nature for the first time on future volunteering behaviour and other outcomes of interests. Our design is such that we can fill gaps in several areas where causal estimation has been hard and where evidence on the effects of volunteering are limited. We show that our design creates plausible random assignment to volunteering for the first-time, conditional on availability and whether the participant was offered a voucher to attend. We summarise and briefly discuss our key results below (much of the discussion is in the “Results and discussion” sections of this chapter).

Firstly, we find that a first-time experience volunteering in nature increases the probability of pre-committing and committing to future volunteering events and attending events in the future. The relative effect size is significant, with the probability of attending a future event increasing more than six-fold for those who had their first-time experience. This is in line with much of the literature in psychology that argues past behaviour predicts future behaviour (though most studies are correlational, unlike our study - Albarracín & Wyer, 2000). Our results also add to the burgeoning literature on interventions that help individuals experiment with new behaviours. Previous studies have shown that helping individuals experiment with public transit or new transit routes crowds in future behaviour after the intervention period (Gravert & Olsson Collentine, 2021; Larcom et al., 2017). We show that a similar principle applies for nature restoration volunteering – an intervention to encourage experimentation with volunteering crowds in future volunteering behaviour. This crowding-in effect is important to consider when designing and evaluating policy because the benefits will include the future behaviour change that is crowded-in following the immediate impacts of the intervention.

Secondly, an important consideration is why does a first-time experience crowd-in future volunteering behaviour? We provide evidence on the mechanisms driving this effect. In our theory section, we postulate that this crowding-in effect may occur for two main reasons: a) the first-time experience provides information about the benefits of volunteering, which reduces inaccuracy and uncertainty (see our theoretical model in Chapter 2) and b) the first-time experience shifts environmental attitudes and identity, which crowds in future PEB. We find evidence that both mechanisms occurring and are likely driving the observed effect. Consistent with our theoretical model (Chapter 2), we found that most first-time participants under-estimated the benefits (proxied by enjoyment) they would attain from volunteering. We also found that knowledge and positive perceptions of nature restoration groups increased significantly following the first-time experience volunteering. This supports the information mechanism. Furthermore, we find that a first-time experience volunteering in nature strengthens environmental identity (EID) and locus of control (LOC) beliefs, consistent with the shifting of attitudes and preferences and the emerging literature on preference endogeneity (Mattauch et al., 2022). We also find that there are increases in donation behaviour and other self-reported PEBs, which is consistent with a broader shift in environmental attitudes.

Thirdly, we show that a first-time experience volunteering in nature increases several other outcomes of interest. As we mention above, a first-time experience increases EID and LOC beliefs, which are both important pre-cursors to wider pro-environmental behaviour and policy support (Allen & Ferrand, 1999; Andor et al., 2022; Crompton & Kasser, 2009; Sharpe et al., 2021; van der Werff et al., 2013). Indeed, we also show that volunteering for the first-time likely crowds-in other PEBs. In an age of increasing environmental pressures and rising anxiety in relation to these pressures (Whitmarsh et al., 2022), interventions or activities that strengthen EID and LOC beliefs could prove to be very useful. We also find that a first-time experience volunteering in nature increases short-term wellbeing, which adds to the literature showing causally that volunteering (more broadly) increases life satisfaction and wellbeing (see for example, Dolan et al., 2021; Meier & Stutzer, 2008). We find no effect of an experience volunteering in nature on connection to nature and community, which is consistent with these measures being deeper relational measures that develop over time (so one experience is likely to have a negligible effect).

Overall, our results show that helping individuals experiment with volunteering in nature generates significant positive spillovers to future volunteering behaviour and wider PEBs. A large share of the first-time volunteers were originally incentivised to attend using a carefully framed voucher incentive, so we also add to the literature on the crowding in and out effects of financial incentives, for which there are mixed results (Gravert & Olsson Collentine, 2021; Ling & Xu, 2021; Vorlaufer et al., 2023). Our results from both stages of the experiment show that using a carefully framed financial incentive encourages experimentation with nature restoration volunteering and subsequently generates substantial crowding-in effects of future behaviour and wider PEBs.

Of course, our results have some notable limitations. When analysing the results for stage two of the field experiment, we took a smaller sample size so that we could plausibly say volunteering for the first-time was conditionally random. The relatively small sample size means we have less statistical power to detect effects, and in particular, interaction effects that would highlight some of the heterogeneity in the effects of a first-time experience on future behaviour. Indeed, we generally found no statistically significant interactions for our pre-registered heterogeneity analyses, which may be due to the lack of power. Future researchers may want to carry out similar studies with greater sample sizes to focus particularly on the heterogeneity of a first-time experience volunteering.

Second, our results may have a context-specific component and there is a need for more causal evidence in many of the areas we touched on in this chapter. The findings we present here are, in some cases, the first causal evidence for particular relationships or the findings add to only a small group of other papers. Therefore, we would recommend future researchers examine these relationships in different contexts, especially given the significant positive spillovers we identify here.

Moreover, our design and results are considered in relation to our simple theoretical model in Chapter 2 and the brief theory section in this chapter. We do not formalise our model in such a way that structural modelling and parameter estimation can take place. This means we cannot delineate the relative importance of the information and attitudes (or identity) channels in driving the increase in future volunteering behaviour. However, we can show both are occurring and a first-time experience volunteering provides new information and likely shifts attitudes, identity and preferences. Future researchers may want to formalise our theory further and take efforts to identify the relative importance of shifting attitudes and information provision.

Unlike stage one, we could not achieve perfect randomisation in stage two. Instead, we rely on our assumption of conditional random assignment, which we find good support for. However, there may still be some endogeneity present and future research could consider alternative research designs to add to the robustness of our results. For example, future designs could use an over-allocation principle, where too many people sign-up for an event so the participants are randomly selected (this is similar to the evaluation of a Covid-19 micro-volunteering programme in the UK - Dolan et al., 2021).

Finally, we only monitor participants over a relatively short period of time (approximately 1-2 months on average). This means we are unable to conclusively make assertions about long-run behaviour change, even though we find results that may indicate longer-run changes in behaviour (increases in environmental identity, for example). Future researchers could track participants over longer periods of time and perhaps have several intervention points. Then, rather than evaluating the effects of just one experience volunteering, researchers could look at the compounding effects of several experiences volunteering. Researchers may find over the long-run, there are changes in deeper relational variables (like connection to nature) and larger shifts in variables like environmental identity and locus of control beliefs.

Chapter 6: Conclusion

The aim of this thesis was to further our understanding of pro-environmental behaviour (PEB) change efforts to enhance and protect nature and biodiversity – an under-researched area in the behavioural science literature (Nielsen et al., 2021). This is important now more than ever as we face monumental environmental challenges in climate change and biodiversity loss that demand shifts in individual behaviour. Existing behavioural research tends to focus on climate related PEBs or PEBs that are easily measured and monitored (Brent et al., 2017). Far fewer studies focus on PEBs that aim to directly influence biodiversity outcomes and restore nature.

In this thesis, we presented theoretical and empirical evidence from a large field experiment in Aotearoa New Zealand on increasing volunteering for nature restoration. We selected volunteering as our focus PEB because of the out-sized impact behavioural interventions could have on actual environmental outcomes. We discussed this explicit PEB selection process in Chapter one and show that it is a relatively novel feature across behavioural science disciplines (and one that is being increasingly advocated for by leading scholars). In Chapter one, we also show that there are a wide range of private benefits from volunteering, which could be magnified in the case of volunteering for nature. These benefits include increased mental and physical health, higher levels of life satisfaction, greater wellbeing and enhanced social and human capital.

In light of the substantive range of private benefits, we postulate that there is a group of individuals who are under-investing in volunteering for nature restoration. Hence, in Chapter 2, we present a new theoretical model that shows how three key factors (uncertainty, inaccuracy and high adjustment costs) potentially reduce the uptake of pro-social behaviours that are welfare-enhancing for society and for the individual themselves. Our flexible model makes predictions about how incentives will affect behaviour change efforts and predicts that helping individuals experiment with these behaviours could generate crowding-in effects for future behaviour.

With the background from Chapters 1 and 2, we report on the design, methods and results from our field experiment in Chapters 3 to 5. In Chapter 3, we summarise our field experiment design and research questions. The field experiment consists of two stages: in stage one, we set out to evaluate the effects of three randomly assigned treatments (a nudge, a supermarket voucher and both combined) on volunteering behaviour for those not already volunteering (first-time volunteers). In stage two, we aim to evaluate the effects of a first-time experience volunteering on future volunteering behaviour and other outcomes of interest (like wellbeing, pro-environmental attitudes, environmental identity and locus of control beliefs). Our field experiment has full ethics approval and was pre-registered.

Our stage one results in Chapter 4 show that offering a $50 NZD supermarket incentive significantly increases attendance rates at volunteering events and commitment rates to attend volunteering events. On the other hand, an environmentally and socially motivated nudge in isolation has no effect on volunteering behaviour. However, combining the nudge with the voucher incentive enhances the efficacy of either treatment alone (the voucher is more effective when used with a nudge). Being offered a voucher to attend an event also reduces the probability of donating to an environmental organisation *immediately* after being offered the voucher. However, we show that this small crowding out effect does not persist approximately one month later. In line with our theoretical model, we explore the heterogeneity in the treatment effects from the voucher incentive. We find that males, those living outside of Hamilton City and those who are well-educated are less receptive to the voucher incentive on average. We also find that, on average, those with higher pre-existing environmental attitudes are *more* receptive to the voucher. This is an encouraging sign that the voucher is not crowding out intrinsic motivation as we would expect to see smaller not larger treatment effects for those with high pre-existing environmental motivation (if there were indeed crowding out effects).

In Chapter 5, we show that we can estimate the causal impacts of volunteering for the first time on future behaviour and other outcomes by exploiting random variation in peoples' availability. We show that once we control for general availability and voucher assignment in stage one, there are no observable covariates that predict stage two treatment assignment (attending a volunteering event). Instead, we argue that assignment to the groups (conditional on general availability and voucher assignment) is largely due to idiosyncratic differences in specific availability. For example, some individuals are available on Wednesdays (and thus, match our volunteering event dates) and some on Thursdays (so do not match the event dates), but are essentially identical in every other way. We present empirical and theoretical evidence supporting this notion and the assumption that potential outcomes are independent of treatment assignment conditional on a set of weights we estimate (which are a function of general availability, voucher assignment and control variables).

Our empirical results in Chapter 5 show that a first-time experience volunteering significantly increases the likelihood of committing to and attending future volunteering events. We also show that a first-time experience volunteering increases short-term wellbeing, general willingness to volunteer, positive perceptions of nature restoration groups and knowledge of restoration groups. Moreover, being treated in stage two also increases environmental self-identity and locus of control beliefs, which are two major pre-cursors to a wider range of PEBs. Indeed, our results also show a first-time experience generates positive spillovers to environmental donations and other self-reported PEBs. Overall, our results support our theoretical model and the hypotheses we made in earlier chapters. This includes the key finding that helping people to try volunteering for the first time (through an intervention like a voucher incentive) generates substantial positive spillover effects for future volunteering behaviour, other PEBs and environmental attitudes and beliefs. The first-time experience does this by a) shifting environmental identity and environmental beliefs (like locus of control beliefs) and b) increasing the information individuals have about volunteering activities and their associated benefits.

As we outlined in Chapter 1, this thesis makes several substantial contributions to the literatures on pro-environmental behaviour, field experiments, volunteering and environmental economics. However, significant gaps still exist and we concur with other scholars who call for more research on shifting behaviour to enhance and protect nature (Nielsen et al., 2021). We hope this thesis stimulates exciting new research with the potential to create positive change for the environment and the results are used to inform policy and intervention designed to increase the uptake of volunteering and other PEBs.

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Appendices

Appendix A. Survey one full copy

*** Note: This does not show the complete set of flows and logic carried out through the survey. Further details can be obtained from the author if desired. The blocks are not necessarily shown in the order that they are documented here. For example, the blocks asking for pre-commitment (which differ based on treatment group) are shown towards the end of this document.

Start of Block: Introduction block

Intro **Information Sheet**

Understanding Community Engagement with Restoration Groups in Kirikiriroa Hamilton

Overview

My name is Robbie Maris. As part of my Master of Management Studies I am undertaking a research project with the Biological Heritage National Science Challenge – Ngā Koiora Tuku Iho to better understand environmental stewardship and kaitiakitanga.

You are being invited to participate in a study on community engagement with restoration groups in Kirikiriroa Hamilton. The research is led by myself and my supervisors, Dr Zack Dorner and Dr Susan Olivia.

What will you have to do and how long will it take?

This is an online survey that will take roughly 10 minutes. We may contact you with a follow up survey in around one month, which will also take roughly 10 minutes.

If you complete the first survey, you will go in the draw to win one of five $100 Prezzy Cards. Once the research is complete, we will be happy to share our findings with you, if you wish.

The survey is best completed on a computer or tablet but can also be completed on your smart phone. Please note that the exact version of the survey you receive may be different to other participants. We ask that only one member of your household fills out the survey.

As part of the survey there will be opportunities to volunteer with a local restoration group. These are completely optional. If you take up one of these opportunities we will send you reminders and verify your attendance.

What will happen to the information collected?

Participating in this study is entirely voluntary. If you start the survey and decide you do not want to continue, you have the right to leave at any stage and we will delete your response from the dataset. Your responses will be used for academic and research purposes only.

As part of this study you will be asked to provide your name and email address. Only myself and my supervisors will have access to this information. We will only use personal identifiers to contact you regarding the Prezzy Card draw, volunteering, the second survey, and to verify your identity at a volunteering opportunity (if you attend).

Two months after the second survey, we will delete all personal identifiers so that the data is anonymised. Before using the data for any further research work, we will ensure the data is completely anonymised and that no personal information is disclosed. The anonymised data may be used for publications, conference presentations, BioHeritage National Science communications and reports for community groups.

Declaration to participants

If you take part in the study, you have the right to:
• Refuse to answer any particular question, and to stop the survey at any point.
• Withdraw from the study and have your responses removed from the dataset. This can be done up until the point that personal identifiers have been deleted (see above).
• Be given access to a summary of the findings from the study when it is concluded.

If you have any questions about this research project, you can contact me using the details below.

We very much appreciate your valuable input and we welcome any questions, thoughts, suggestions or comments you may have regarding the research.

Mr Robbie Maris
Email: robbiem8910@gmail.com
Ph: +64 27 325 7877

Page Break

Consent form **Consent Form for Participants**

I have read the Information Sheet for Participants for this study. I clearly understand what will be involved in this survey, the risks and benefits of participation, and how my data will be protected and used.

I also understand that I am free to withdraw from the survey at any time up until 2 months after the second survey, or to decline to answer any particular questions in the survey. I agree to participate in this study under the conditions set out in the Information Sheet on the previous page.

○ Yes

○ No

Skip To: End of Survey If Consent Form for Participants I have read the Information Sheet for Participants for this study.... = No

End of Block: Introduction block

Start of Block: Preliminary Screening Questions

Q8 Thank you for agreeing to participate. We first have a few questions to see if you qualify for our study.

Do you normally reside in or within easy travel distance to Hamilton?

○ Yes

○ No

Q36 Are you aged 18 years or older?

- Yes
- No

Q3 To the best of your knowledge, have any other members of your household completed this survey?

- Yes
- No
- Unsure

End of Block: Preliminary Screening Questions

Start of Block: End Early Block

Q45 Thank you for completing the survey, but you do not fit the current target group for our study.

We really appreciate your time and we would also be grateful if you could recommend and share this survey with your friends by sending them this link (https://waikato.qualtrics.com/jfe/form/SV_5bfoe2JT7nUltmS) or posting on social media.

End of Block: End Early Block

Start of Block: Volunteering screen

Q1 Have you volunteered for a community nature restoration group in the last three years?

- Yes
- No
- Unsure/I can't remember

Q2 Within the next 12 months, how willing are you to volunteer for a community restoration group?

	Very unwilling	Unwilling	Somewhat unwilling	Neither willing nor unwilling	Somewhat willing	Willing	Very willing
Willingness	○	○	○	○	○	○	○

End of Block: Volunteering screen

Start of Block: Already Volunteer

Q54 Thinking back over the last year, roughly how often did you volunteer for nature restoration groups?

- Never
- Once a year
- Several times a year
- Once a month
- Several times a month
- Once a week
- Several times a week
- Every day

Q55 Have your volunteering experiences included restoration groups in or in close proximity to Hamilton?

- Yes
- No

End of Block: Already Volunteer

Start of Block: Demographics

Q7 The first set of questions ask about some of your personal details and will only ever be used to match your responses to later surveys and contact you about future volunteering opportunities and the Prezzy Card prize draw. Any information you provide will remain completely confidential to the researchers only, and will be deleted in two months time.

Q4 Please provide some details about yourself below:

- What is your first name? ____

- What is your last name? ____

- What is your email address? ____

- What is your mobile phone number? ____

Page Break

Q10 Thank you! Now we will ask you some more general questions about yourself.
Which suburb do you normally live in?

- ○ Beerescourt
- ○ Cambridge
- ○ Chartwell
- ○ Claudelands
- ○ Dinsdale
- ○ Fairfield
- ○ Fairview Downs
- ○ Flagstaff
- ○ Forest Lake
- ○ Frankton
- ○ Glenview
- ○ Horsham Downs
- ○ Hamilton CBD and Lake
- ○ Hamilton East
- ○ Hillcrest
- ○ Huntly
- ○ Matangi
- ○ Melville
- ○ Nawton
- ○ Ngāruawāhia
- ○ Pukete

- Rototuna
- St. Andrews
- Tamahere
- Temple View
- Other (please specify) ____

Page Break

Q11 What is your age?

____

Q12 What is your gender identity?

- Male
- Female
- Non-binary/Gender diverse

Q34 Which ethnic group do you belong to? (Please select as many that apply to you)

- ☐ New Zealand European
- ☐ Māori
- ☐ Samoan
- ☐ Cook Islands Māori
- ☐ Tongan
- ☐ Niuean
- ☐ Chinese
- ☐ Indian
- ☐ Other (please specify) ____

Q35 How would you describe your household income level?

- ○ High income
- ○ Middle income
- ○ Low income

Q49 What is your current employment status?

- ○ Full-time
- ○ Part-time
- ○ Self-employed
- ○ Retired
- ○ Student
- ○ Unpaid family worker
- ○ Other (please state) ____

Q50 What is your highest qualification?

- ○ No Qualification
- ○ Level 1 Certificate
- ○ Level 2 Certificate
- ○ Level 3 Certificate
- ○ Level 4 Certificate
- ○ Level 5 Diploma
- ○ Level 6 Diploma
- ○ Bachelor Degree and Level 7 Qualification
- ○ Post-graduate and Honours Degrees
- ○ Masters Degree
- ○ Doctorate Degree
- ○ Overseas Secondary School Qualification
- ○ Not elsewhere included

Page Break

Q13 Please tell us about the people in the household (the people that normally live in your home) excluding yourself. *Please put a number in each box (could be 0).*

	Number
Infants and young children aged 0-5	
Children aged 6-13	
Children aged 14-17	
Adults aged 18-64	
Adults aged 65+	

Q14 Do any household members aged 14 and above require special care?

- Yes
- No

Page Break

Q37 Please indicate how strongly you disagree or agree with this statement:

I know a lot about community restoration groups in Hamilton.

- Strongly disagree
- Disagree
- Somewhat disagree
- Neither agree nor disagree
- Somewhat agree
- Agree
- Strongly agree

Q38 What are your perceptions of community restoration groups in Hamilton?

- Very negative
- Negative
- Somewhat negative
- Neither positive nor negative
- Somewhat positive
- Positive
- Very positive
- I don't know

Q39 Thinking back over the last year, roughly how often did you engage in other volunteering activities (outside of restoration activities)?

- Never
- Once a year
- Several times a year
- Once a month
- Several times a month
- Once a week
- Several times a week
- Every day

Page Break

Q42 Please indicate how strongly you agree with the following statements.

	Strongly disagree	Disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Agree	Strongly agree
If I use plastic, I try to recycle it.	o	o	o	o	o	o	o
If I have the choice, I take public transport.	o	o	o	o	o	o	o
If I have the choice, I choose to walk instead of driving.	o	o	o	o	o	o	o
If I notice litter, I pick it up.	o	o	o	o	o	o	o
If I see pollution, I report it.	o	o	o	o	o	o	o
When I leave the room, I turn the lights off.	o	o	o	o	o	o	o

End of Block: Demographics

Start of Block: Outcome indicies

Q15 The next set of questions asks more about your attitudes and values. The following statements represent different points of view or opinions. Remember, the best answer is your own opinion.

Page Break

Locus of control Please indicate how strongly you disagree or agree with each statement.

	Strongly disagree	Disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Agree	Strongly agree
The efforts deployed by environmental groups (such as Forest and Bird) have a positive impact on many environmental challenges.							
By making a donation to environmental groups (such as Forest and Bird), I can help make a positive difference on the state of the environment.							
By giving money to environmental groups, I help increase their probability of success.							
Pro-environmental groups make a difference in fighting local environmental issues.							
I am able to convince some of my friends to take some kind of action with regards to environmental challenges.							

Page Break

Environmental ID Please indicate how strongly you disagree or agree with each statement.

	Strongly disagree	Disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Agree	Strongly agree
Acting environmentally friendly is an important part of who I am	○	○	○	○	○	○	○
I am the type of person who acts environmentally friendly	○	○	○	○	○	○	○
I see myself as an environmentally friendly person.	○	○	○	○	○	○	○

Page Break

Conn Community Please indicate how strongly you disagree or agree with each statement.

	Strongly disagree	Disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Agree	Strongly agree
I often feel connected to my local community	○	○	○	○	○	○	○
I often feel connected to nature	○	○	○	○	○	○	○

Page Break

Wellbeing Who5 Please indicate for each of the five statements which is closest to how you have been feeling over the **past two weeks.**

	At no time	Some of the time	Less than half of the time	More than half of the time	Most of the time	All of the time
I have felt cheerful and in good spirits.	○	○	○	○	○	○
I have felt calm and relaxed.	○	○	○	○	○	○
I have felt active and rigorous.	○	○	○	○	○	○
I woke up feeling fresh and rested.	○	○	○	○	○	○
My daily life has been filled with things that interest me.	○	○	○	○	○	○

End of Block: Outcome indicies

Start of Block: Close

Display This Question:

If If The volunteering events will start at approximately 10:00 AM and finish at around 12:00 PM, where kai will be provided. Please click on any dates where you would be willing and able to attend a vol... Text Response Is Empty

Q29 Thank you for your time and answers. We very much appreciate your valuable input and we welcome any questions, thoughts, suggestions or comments you may have regarding the research.

You are automatically in the draw for **1 of 5 $100 Prezzy Cards.** We may contact you with a follow up survey in around one month, which will also take roughly 10 minutes.

We would love to share our progress and research results with you. If you would like to receive updates from us, please check the box below.

○ Yes, I would like to receive updates

○ No, I would not like to receive updates

Display This Question:

If If The volunteering events will start at approximately 10:00 AM and finish at around 12:00 PM, where kai will be provided. Please click on any dates where you would be willing and able to attend a vol... Text Response Is Not Empty

Q48 Thank you for your time and answers. We very much appreciate your valuable input and we welcome any questions, thoughts, suggestions or comments you may have regarding the research.

You are automatically in the draw for **1 of 5 $100 Prezzy Cards.** We may contact you with volunteering opportunities in line with your availability, and a follow up survey in around one month, which will also take roughly 10 minutes.

We would love to share our progress and research results with you. If you would like to receive updates from us, please check the box below.

○ Yes, I would like to receive updates

○ No, I would not like to receive updates

Page Break

Q43 We are also giving participants the opportunity to anonymously donate some of their Prezzy Card to either Forest and Bird, GoEco orGreenpeace Aotearoa.

This donation will occur if you are randomly selected to receive a Prezzy Card. The final amount of your Prezzy card will be $100 minus the amount you have chosen to donate.

If selected as a winner, are you willing to donate some (up to $70) of your Prezzy Card to either of these groups? If so, please select the group below.

○ Forest and Bird

○ GoEco

○ Greenpeace Aotearoa

○ I do not wish to donate

End of Block: Close

Start of Block: Donation amount

Display This Question:

If We are also giving participants the opportunity to anonymously donate some of their Prezzy Card t... != I do not wish to donate

*

Q58 **If you are selected to receive a Prezzy Card for completing the survey**, how much of it would you like to donate (up to $70) to ${e://Field/Qpipe}? Please enter a numeric value (i.e., 20 means $20).

____

Q56 Thank you for your time. If you have any comments for us, please add them here.

____

End of Block: Donation amount

Start of Block: Control Brief

Q40 We are looking for volunteers for a series of events with a community restoration group on the eastern side of Hamilton.

These short volunteering events will be in the mornings and last around 2 hours. Activities at the volunteering events may include planting, potting, monitoring and trapping and a range of other activities. No prior skills or experience are required for any of the activities. Lunch will be provided for all volunteers, and you may bring household members with you.

Q33 Would you be willing to participate in one of these volunteering events sometime in the next month?

- Yes
- No

End of Block: Control Brief

Start of Block: No volunteering

Display This Question:

If Would you be willing to participate in one of these volunteering events sometime in the next month? = No

Or We would like to invite you to participate in one of these short volunteering events. Would you b... = No

Or Would you be willing to participate in one of these volunteering events sometime in the next month? = No

Or We would like to invite you to participate in one of these short volunteering events. Would you b... = No

Q59 Could you please tell us the main reason you are not willing to participate in one of these volunteering events over the next month?

- ○ I have no time available over the next month
- ○ I have a physical disability or impairment that will prevent me from participating
- ○ I am not interested in the events
- ○ The location is not suitable for me
- ○ I am not interested in volunteering
- ○ Other (please specify) ____

End of Block: No volunteering

Start of Block: Nudge Only Brief

Q23 We are looking for volunteers for a series of events with a community restoration group on the eastern side of Hamilton.

Participating in one of these events is a great way to give back to your community and the environment while having fun! It is also a good way to meet like-minded people. Studies show that volunteering increases overall wellbeing. You might also learn some new skills that you can apply at home or in your local neighbourhood to positively impact the environment.

These short volunteering events will be in the mornings and last around 2 hours. Activities at the volunteering events may include planting, potting, monitoring and trapping and a range of other activities. No prior skills or experience are required for any of the activities. Lunch will be provided for all volunteers, and you may bring household members with you.

Q32 We would like to invite you to participate in one of these short volunteering events. Would you be willing to participate in one of these volunteering events sometime in the next month?

- ○ Yes
- ○ No

End of Block: Nudge Only Brief

Start of Block: Voucher Only Brief

Q24 We are looking for volunteers for a series of events with a community restoration group on the eastern side of Hamilton.

To recognise volunteers' time commitment and willingness to try something new, volunteers will receive a **one-**

off $50 supermarket voucher at the event. Please note that we can only provide one voucher per household.

These short volunteering events will be in the mornings and last around 2 hours. Activities at the volunteering events may include planting, potting, monitoring and trapping and a range of other activities. No prior skills or experience are required for any of the activities. Lunch will be provided for all volunteers, and you may bring household members with you.

Q31 Would you be willing to participate in one of these volunteering events sometime in the next month?

- Yes
- No

End of Block: Voucher Only Brief

Start of Block: Combined Brief

Q25 We are looking for volunteers for a series of events with a community restoration group on the eastern side of Hamilton.

Participating in one of these events is a great way to give back to your community and the environment while having fun! It is also a good way to meet like-minded people. Studies show that volunteering increases overall wellbeing. You might also learn some new skills that you can apply at home or in your local neighbourhood to positively impact the environment.

To recognise volunteers' time commitment and willingness to try something new, volunteers will receive a **one-off $50 supermarket voucher** at the event. Please note that we can only provide one voucher per household.

These short volunteering events will be in the mornings and last around 2 hours. Activities at the volunteering events may include planting, potting, monitoring and trapping and a range of other activities. No prior skills or experience are required for any of the activities. Lunch will be provided for all volunteers, and you may bring household members with you.

Q26 We would like to invite you to participate in one of these short volunteering events. Would you be willing to participate in one of these volunteering events sometime in the next month?

- Yes
- No

End of Block: Combined Brief

Start of Block: Volunteering uptake QsPr

Display This Question:

If Would you be willing to participate in one of these volunteering events sometime in the next month? = Yes

Or We would like to invite you to participate in one of these short volunteering events. Would you b... = Yes

Or Would you be willing to participate in one of these volunteering events sometime in the next month? = Yes

Or We would like to invite you to participate in one of these short volunteering events. Would you b... = Yes

JS

Q28 The volunteering events will start at approximately 10:00 AM and finish at around 12:00 PM, where kai will be provided. Please click on any dates where you would be willing and able to attend a volunteering event over the next four weeks.

Please note that that not all dates will have volunteering events and there is a limit on numbers. If you available on a date/s where there is an event, we may follow-up after this survey providing further details and confirming your place at the event. We understand that plans change, so selecting a date here does not lock you in to attending an event.

____

Display This Question:

If If The volunteering events will start at approximately 10:00 AM and finish at around 12:00 PM, where kai will be provided. Please click on any dates where you would be willing and able to attend a vol... Text Response Is Empty

And And The volunteering events will start at approximately 10:00 AM and finish at around 12:00 PM, where kai will be provided. Please click on any dates where you would be willing and able to attend a vol... Text Response Is Displayed

Q44 We notice you did not select any of the dates in the calendar. Could you please indicate why?

- ○ I do not have any availability over the next month
- ○ I changed my mind about volunteering
- ○ That time of day does not work for me
- ○ Other ____

End of Block: Volunteering uptake QsPr

Appendix B. Survey two full copy

Start of Block: Introduction block

Intro

Overview

Thank you for your help so far with our research - it is much appreciated.

This is the **final survey** for this research project. It is important for our research project for you to complete this survey if you filled in our first survey. We really appreciate your time. This survey will take approximately five minutes.

Please note: You will see questions that are similar to previous surveys you may have filled out for us. This is normal and we thank you for your answers.

If you complete this survey, you will go in the draw to win **one of five $100 Prezzy Cards.** These are additional Prezzy Cards to those offered in our first survey.

Thank you again - we very much appreciate your valuable input and we welcome any questions, thoughts, suggestions or comments you may have regarding the research.

Mr Robbie Maris
Email: rm291@students.waikato.ac.nz
Ph: +64 27 325 7877
The University of Waikato

End of Block: Introduction block

Start of Block: Details

Q7 So we can match your answers to your earlier survey response, the first few questions ask for some of your personal details. Your personal details will only ever be used to match your responses to earlier surveys and contact you about future volunteering opportunities and the Prezzy Card prize draw.

Q4 Please provide some details about yourself below:

Please fill in each box manually (do not use autofill).

○ What is your first name? ____

○ What is your last name? ____

○ What is your email address? ____

End of Block: Details

Start of Block: Baseline Qs

Q2 Within the next 12 months, how willing are you to volunteer for a community restoration group?

	Very unwilling	Unwilling	Somewhat unwilling	Neither willing nor unwilling	Somewhat willing	Willing	Very willing
Willingness							

Page Break

Q37 Please indicate how strongly you disagree or agree with this statement:

I know a lot about community restoration groups in Hamilton.

- Strongly disagree
- Disagree
- Somewhat disagree
- Neither agree nor disagree
- Somewhat agree
- Agree
- Strongly agree

Q38 What are your perceptions of community restoration groups in Hamilton?

- Very negative
- Negative
- Somewhat negative
- Neither positive nor negative
- Somewhat positive
- Positive
- Very positive
- I don't know

Page Break

Q61 Have you volunteered for a restoration group since you took our first survey (roughly the last month)?

- Yes
- No

Display This Question:

If Have you volunteered for a restoration group since you took our first survey (roughly the last mo... = Yes

Q62 You indicated you have volunteered for a restoration group in the last month.

Could you please briefly describe when and where you volunteered?

____

Page Break

Q42 Please indicate how strongly you agree with the following statements.

	Strongly disagree	Disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Agree	Strongly agree
If I use plastic, I try to recycle it.							
If I have the choice, I take public transport.							
If I have the choice, I choose to walk instead of driving.							
If I notice litter, I pick it up.							
If I see pollution, I report it.							
When I leave the room, I turn the lights off.							

Page Break

End of Block: Baseline Qs

Start of Block: Outcome indicies

Q15 The next set of questions asks more about your attitudes and values. The following statements represent different points of view or opinions. Remember, the best answer is your own opinion.

Page Break

Locus of control Please indicate how strongly you disagree or agree with each statement.

	Strongly disagree	Disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Agree	Strongly agree
The efforts deployed by environmental groups (such as Forest and Bird) have a positive impact on many environmental challenges.							
By making a donation to environmental groups (such as Forest and Bird), I can help make a positive difference on the state of the environment.							
By giving money to environmental groups, I help increase their probability of success.							
Pro-environmental groups make a difference in fighting local environmental issues.							
I am able to convince some of my friends to take some kind of action with regards to environmental challenges.							

Page Break

Environmental ID Please indicate how strongly you disagree or agree with each statement.

	Strongly disagree	Disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Agree	Strongly agree
Acting environmentally friendly is an important part of who I am							
I am the type of person who acts environmentally friendly							
I see myself as an environmentally friendly person.							

Page Break

Page Break

Conn Community Please indicate how strongly you disagree or agree with each statement.

	Strongly disagree	Disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Agree	Strongly agree
I often feel connected to my local community							
I often feel connected to nature							

Page Break

Wellbeing Who5 Please indicate for each of the five statements which is closest to how you have been feeling over the **past two weeks.**

	At no time	Some of the time	Less than half of the time	More than half of the time	Most of the time	All of the time
I have felt cheerful and in good spirits.						
I have felt calm and relaxed.						
I have felt active and rigorous.						
I woke up feeling fresh and rested.						
My daily life has been filled with things that interest me.						

End of Block: Outcome indicies

Start of Block: Volunteering General

Q66 We are looking for volunteers for a *family-friendly* volunteering event in March on the Eastern side of Hamilton.

There will be opportunities to engage in a range of volunteering activities and attend educational workshops. Also lunch will be provided for all attendees.

The event will be in the morning and you will be able to come and go at times that suit your schedule.

I am willing to attend this event if I am available.

- Yes
- No

Display This Question:

If We are looking for volunteers for a family-friendly volunteering event in March on the Eastern si... = No

Q67 Could you please tell us the main reason you are not willing to participate in this event?

- I am not available in March
- I have a disability or impairment that will prevent me from participating
- I am not interested in the event
- The location is not suitable for me
- I am not interested in volunteering
- Other (please specify) ____

End of Block: Volunteering General

Start of Block: Volunteering date

Q64 The volunteering event will be held on the morning of **Saturday the 25th of March** on the Eastern side of Hamilton.

To reiterate, you will be able to come and go at times that suit your schedule.

Are you willing and able to attend this event?

- Yes
- No

Display This Question:

If The volunteering event will be held on the morning of Saturday the 25th of March on the Eastern s... = No

Q65 Could you please tell us the main reason you are not willing to participate in this event?

- I am not available at that time
- I have a disability or impairment that will prevent me from participating
- I am not interested in the event
- The location is not suitable for me
- I am not interested in volunteering
- Other (please specify) ____

Display This Question:

If The volunteering event will be held on the morning of Saturday the 25th of March on the Eastern s... = Yes

Q68 Great! We will be in touch with more information about the event.

Please click next for the final questions.

End of Block: Volunteering date

Start of Block: Donation

Q43 We are also giving participants the opportunity to anonymously donate some of their Prezzy Card to either Forest and Bird, GoEco orGreenpeace Aotearoa.

This donation will occur if you are randomly selected to receive a Prezzy Card. The final amount of your Prezzy card will be $100 minus the amount you have chosen to donate.

If selected as a winner, are you willing to donate some (up to $70) of your Prezzy Card to either of these groups? If so, please select the group below.

- Forest and Bird
- GoEco
- Greenpeace Aotearoa
- I do not wish to donate

End of Block: Donation

Start of Block: Donation amount and close

Display This Question:

If We are also giving participants the opportunity to anonymously donate some of their Prezzy Card t... != I do not wish to donate

*

Q58 **If you are selected to receive a Prezzy Card for completing the survey**, how much of it would you like to donate (up to $70) to ${e://Field/Qpipe}? Please enter a numeric value (i.e., 20 means $20).

____

Q60 Thank you for your answers. Would you like to be added to an email list to hear about future environmental volunteering opportunities?

If you select yes, your name and email address only will be passed on to local environmental groups.

- Yes
- No

Q56 Thank you for your time. If you have any comments for us, please add them here.

____

Q63 We will also be in touch with more details about the volunteering event happening in March. We hope to see you there.

End of Block: Donation amount and close

Appendix C. Commitment survey full copy

*** Note: The initial overview below is what those from the voucher groups in stage one see. Participants from the non-voucher group receive an identical overview with different dates and without the voucher details.

Start of Block: Default Question Block

Q2 **Overview**

Thank you again for completing our earlier survey on volunteering with restoration groups and indicating you are willing to volunteer at an event. We very much appreciate your valuable input and contribution to this research.

We are happy to inform you that there are two events at the Fairfield Project from 10:00 AM till 12:00 PM on **Wednesday the 22nd of February** and **Saturday the 25th of February**. We would love to see at one of these events!

The Fairfield Project

The Fairfield Project is an urban biodiversity and gully restoration group, with a particular focus on environmental and sustainable education for people of all ages and background. They serve a diverse community and manage the restoration of the culturally and ecologically significant Kukutaaruhe Gully.

Event details

The volunteering event will be held at the Fairfield Project and starts at 10:00 AM (https://goo.gl/maps/YMttUgZq1V8BFQ528). Parking is available at the event off College Place, Fairfield.

There will be five activities on offer, including potting, trapping, seed collection, compositing and tracking/monitoring. You will get the opportunity to choose which activities you would like to do. None of the activities require any prior skills, knowledge or experience!

Lunch will be provided for all volunteers at 12:00 PM. This includes any household members you choose to bring with you.

Voucher

As a reminder, to recognise your time commitment and willingness to try something new, you will receive a **one-off $50 supermarket voucher** at the event. Please note that we can only provide one voucher per household.

Contact details

We look forward to hopefully seeing you at the event. If you have any questions or run into any issues, please contact Robbie on 0273257877. Alternatively, email Robbie at robbiem8910@gmail.com.

We will send you all of these details via email when you confirm.

Please click next and tell us if you can or cannot attend. It really helps us with our research if you fill out the survey, regardless of whether you are available.

Page Break

Q3 So we can match your answers to your earlier survey response, the first few questions ask for some of your personal details.

○ What is your first name? ________________________________

○ What is your last name? ________________________________

○ What is your email address? ________________________________

○ What is your mobile phone number? ________________________________

Q4 Which event, if any, would you like to attend?

○ Wednesday the 22nd of February

○ Saturday the 25th of February

○ Neither event

End of Block: Default Question Block

Start of Block: No longer available

Q8 We are sorry to hear that you are not willing or able to attend an event. Can you please tell us the main reason you will not be attending?

○ I am no longer available at this time

○ I have a disability or impairment that will prevent me from participating

○ I am not interested in this event

○ The location is not suitable for me

○ I am no longer interested in volunteering

○ Other (please specify) ________________________________

Page Break

Q9 Thank you for taking the time to fill in this short survey. We appreciate your help with our research and we will be in touch with a final survey in the next month.

There will also be a future volunteering opportunity in March. We will follow-up with further details closer to the time.

Thank you again for your help. If there is anything else you would like us to know at this stage, please write it here.

____

End of Block: No longer available

Start of Block: Still available

Q10 Please indicate how many household members you intend to bring with you (if any). This is just to help us prepare for additional attendees. Only you are considered part of the research project. Please put 0 in each box if not applicable.

	Number
Dependent children under 5 yrs	
Dependent children 5-11 yrs	
Dependent children 12-17 yrs	
Adults	

Page Break

Q6 Could you please rank the five activities from highest to lowest in terms of your interest in the activity. This will help the Fairfield Project prepare activities that people are interested in.

Please note: 1 is the highest and 5 is the lowest

______ Potting
______ Trapping
______ Seed collection
______ Composting
______ Tracking/monitoring

Page Break

Q7 Thank you for taking the time to fill in this short survey. We will follow up with an email including all of the details mentioned here.

We look forward to seeing you at the Fairfield Project!

If there is anything else you would like us to know at this stage, please write it here.

____

End of Block: Still available

Appendix D. Post-experience survey full copy

Start of Block: Default Question Block

Q2 Thank you for your participation at a volunteering event at the Fairfield Project and your valuable input to our research.

In this 2-minute survey, we are going to ask you to reflect on your experience volunteering.

Please note that your responses will remain confidential to the researchers only (not the Fairfield Project) and only be shared after removing all identifying information.

End of Block: Default Question Block

Start of Block: Block 1

Q4 First, we need to confirm who you are.

- What is your first name? ____
- What is your last name? ____
- What is your email address? ____

End of Block: Block 1

Start of Block: Block 2

Q6 Overall, how **enjoyable** was your experience volunteering?

- Very enjoyable
- Enjoyable
- Somewhat enjoyable
- Neither enjoyable nor not enjoyable
- Somewhat not enjoyable
- Not enjoyable
- Not at all enjoyable

Q7 With respect to your level of **enjoyment**, would you say that the experience volunteering was **below**, **met** or **exceeded** your expectations?

- ○ Far exceeded expectations
- ○ Exceeded expectations
- ○ Somewhat exceeded expectations
- ○ Met expectations
- ○ Somewhat below expectations
- ○ Below expectations
- ○ Far below expectations

Q8 How likely are you to **recommend** volunteering with the **Fairfield Project** to friends and family?

- ○ Extremely likely
- ○ Moderately likely
- ○ Slightly likely
- ○ Neither likely nor unlikely
- ○ Slightly unlikely
- ○ Moderately unlikely
- ○ Extremely unlikely

Q9 How likely are you to **recommend volunteering** for restoration groups to friends and family?

- Extremely likely
- Moderately likely
- Slightly likely
- Neither likely nor unlikely
- Slightly unlikely
- Moderately unlikely
- Extremely unlikely

Q10 Within the next 12 months, how **willing** are you to **volunteer** for a restoration group?

- Very willing
- Willing
- Somewhat willing
- Neither willing nor unwilling
- Somewhat unwilling
- Unwilling
- Very unwilling

End of Block: Block 2

Start of Block: Block 3

Q11 Thank you for your responses.

Do you have any feedback for the Fairfield Project (for example, about the activities you did)?

If so, please type the details below. Please note that your response will remain anonymous to the Fairfield Project.

____

End of Block: Block 3

Start of Block: Block 4

Q12 Thank you for completing this short survey. There will be one more survey for our research project and a future volunteering opportunity in March.

We will follow-up with further details closer to the time. Once again, we really appreciate your time, willingness to volunteer and help with this research.

End of Block: Block 4

Appendix E. Email reminders about volunteering events and survey two

Example of commitment email

From: robbie.maris@survey.waikato.ac.nz

Subject: Volunteering research follow-up: Upcoming events

Kia ora {First Name},

Thank you again for completing the survey about restoration groups for the University of Waikato and for expressing an interest in volunteering.

There are volunteering events next week from 10:00 AM till 12:00 PM on Wednesday the 15th of February and Saturday the 18th of February. We would love to see you at one of these events.

Please let us know if you can make it or not by filling in this 2 minute survey:
{Survey Link}

We really appreciate it! It really helps us with our research if you fill out the survey, regardless of whether you are available.

If you have any questions, please reply to this email or contact me (Robbie) at 0273257877.

Kind regards,

Robbie Maris

Follow the link to opt out of future emails:
${l://OptOutLink?d=Click here to unsubscribe}

Follow-up email after survey two about follow-up volunteering event

From: robbie.maris@survey.waikato.ac.nz

Subject: Volunteering event in March: Harvest Festival

Kia ora {First Name},

Thank you again for filling in the survey about restoration groups for the University of Waikato.

There is another family-friendly volunteering and education event at the Fairfield Project on **Saturday the 25th of March.**

This is the Fairfield Project's Garden Festival (Te Maara Kai o Kukutaaruhe Festival) which will be a ***family-friendly*** event focused on celebrating the community gardens. There will be opportunities to engage in a range of volunteering activities and attend educational workshops. There will also be opportunities to do some volunteering in the gully and lunch will be provided for all attendees.

We and the Fairfield Project would love to see you at this event!

Please see below for further details:

Location, time and parking

The volunteering event will be held at the Fairfield Project from 9:00 AM till 1:00 PM (https://goo.gl/maps/YMttUgZq1V8BFQ528). Parking is available at the event off College Place, Fairfield. The parking is out the back of the Fairfield College staff carpark.

You can arrive and leave anytime during the event.

What to bring?

- Covered shoes (for example, trainers or gumboots)
- Comfortable clothing
- Raincoat (if raining)
- Water bottle
- A positive attitude

The event will be going ahead in the case of wet weather.

If there is wet weather, please bring a raincoat. We will try to work under shelter as much as possible.

What will you be doing?

There will be a range of volunteering activities available and you will have the opportunity to pick which activities to participate in. Most activities will be centred around celebrating and having fun at the Fairfield Project's Community Gardens.

No prior skills or experience are needed for any of the activities. Full instructions will be provided. There will also be educational workshops you can attend and activities for the kids! For example, there will be a chance to learn about composting, planting and harvesting.

Lunch will be provided for everyone towards the end of the event (around 12:30 PM).

Contact details

We look forward to seeing you at the event. If you have any questions or run into any issues, please contact Lyn at lynnetterogers25@gmail.com.

Alternatively, contact Marina at the Fairfield Project on 0224517461.

Kind regards,

Robbie Maris
University of Waikato

Email confirming attendance at volunteering events

We present the email sent to those attending voucher events below. The email to those not receiving a voucher was exactly the same without the small voucher section near the end of the email.

From: robbie.maris@survey.waikato.ac.nz

Subject: Volunteering event confirmation

Kia ora {First Name},

Thank you again for helping with our research and thank you for confirming your attendance at the volunteering event on **{Date}.** Here are a few extra details ahead of the event:

Location, time and parking

The volunteering event will be held at the Fairfield Project and starts at 10:00 AM (https://goo.gl/maps/YMttUgZq1V8BFQ528). Parking is available at the event off College Place, Fairfield. The parking is out the back of the Fairfield College staff carpark.

What to bring?

- Covered shoes (for example, trainers or gumboots)
- Comfortable clothing
- Raincoat (if raining)
- A positive attitude

The event will be going ahead in the case of wet weather.

If there is wet weather, please bring a raincoat. We will try to work under shelter as much as possible.

What will you be doing?

There will be five activities at the volunteering events and you will have the opportunity to pick which activities to participate in. Please note you may not get your first choice.

No prior skills or experience are needed for any of the activities. Full instructions will be provided.

- Potting
- Trapping
- Seed collection
- Composting
- Tracking/monitoring

Lunch will be provided for all volunteers at the end of the event (around 12:00 PM).

Voucher details

To recognise your time commitment and willingness to try something new, you will receive a one-off $50 supermarket voucher at the event. You will receive this at the end of the event and we will ask that you sign a form to indicate you have received the voucher. Please note that we can only provide one voucher per household.

Contact details

We look forward to seeing you at the event. If you have any questions or run into any issues, please contact Robbie on 0273257877. Alternatively, email Robbie at robbiem8910@gmail.com.

Kind regards,

Robbie Maris

Appendix F. Geographic location variable coding

For the "Fairfield and surrounding suburbs" variable, we use StatsNZ's SA3 suburb area units. We denote someone as living in Fairfield or a surrounding suburb if they live in either Fairfield or a suburb that immediately borders Fairfield. This includes

a. Chartwell
b. Chedworth
c. Enderly
d. Claudelands
e. Queenwood
f. Saint Andrews
g. Bereescourt
h. Whitiora

Appendix G. Linear probability model (LPM) vs non-linear alternatives

In general, there are more severe inferential consequences from violating assumptions in non-linear models (like logit and probit) than in linear models. For example, in the presence of heteroskedasticity, the linear OLS estimator remains consistent and unbiased (H. White, 1981; Wooldridge, 2010). However, the maximum likelihood estimation (MLE) of parameters in non-linear models are inconsistent in the presence of heteroskedasticity (H. White, 1981). Moreover, the omission of important explanatory variables will cause non-linear MLE estimates to be inconsistent regardless of whether those omitted variables are correlated with included regressors (L.-F. Lee, 1982; Yatchew & Griliches, 1985). In the linear case, if there are omitted variables that are *uncorrelated* with the regressors in the model, the parameter estimates are unbiased (L.-F. Lee, 1982).

> *If the main purpose is to estimate the partial effect of xj on the response probability, averaged across the distribution of x, then the fact that some predicted values are outside the unit interval may not be very important.*

(Wooldridge, 2010)

Of course, there are various critiques of the use of the LPM model for binary dependent variables and we find these critiques to be less concerning in our case than the challenges of non-linear modelling (in particular, the high sensitivity of non-linear models to mis-specification).

One of the final critiques is that the LPM does not estimate the structural parameters of non-linear models so it cannot be used as a substitute in that respect (Horrace & Oaxaca, 2006). For example, the LPM model cannot estimate the probit index coefficients (the change in the probit index from a one-unit change in the explanatory variables) or the log-odds coefficients from a logit model. However, in this paper, I am interested in estimating the marginal effects of treatments on the response probability, rather than the structural parameters of a non-linear model.

Another critique is that using a LPM will invariably introduce heteroskedasticity in the error term and this will cause the covariance matrix to become inconsistent (which would affect inferences made from the results). In response, a common approach is to use *robust* standard errors which account for unknown forms of heteroskedasticity (Wooldridge, 2010).

Another issue raised is that the LPM can give predicted probabilities that fall outside the unit interval [0,1]. Again, this is much more of an issue if we are specifically interested in estimating the predicted probabilities for certain individuals. However, in our case, we are interested in estimating the marginal effect of our treatment on the response probability, averaged over the whole sample. As Wooldridge asserts

"*If the main purpose is to estimate the partial effect of xj on the response probability, averaged across the distribution of x, then the fact that some predicted values are outside the unit interval may not be very important.*" – (Wooldridge, 2010)

Measurement error has also been raised as a cause of concern for LPMs. For example, Hausman (2001) shows that the adverse implications of some types of measurement error are significantly worse for the LPM than other models. However, recent work by Meyer & Mittag (2017) show that measurement error tends to put downward bias on estimates from LPM and probit models. As such, in the presence of non-classical measurement error, our estimates of the marginal effects of x will be conservative. Meyer & Mittag (2017) also show that if measurement error is conditionally random (that is, measurement error is unrelated to our regressor variables), we obtain estimates that are reasonably informative of the true marginal effects. In our case, we randomly assign our treatment variable so we are more confident that measurement error is unrelated to our treatment variable.

Overall, weighing up the trade-offs of both approaches, we elect to use LPMs as our base models and run the equivalent logit models as robustness checks. The LPM is easier to interpret and more robust to model misspecification. However, recognising the critiques of the LPM, we will use logit models for comparison to see whether our LPM results are significantly different to the logit equivalent.

Appendix H. Distributional graphs of environmental attitudes, identity and wellbeing variables in survey one.

Below, we present a series of graphs that show the distributions of responses to a range of attitudinal, identity and wellbeing variables of interest. We divide these graphs into responses from “first-time volunteers” and those who are already volunteering for nature restoration groups.

EID and Locus of Control Index

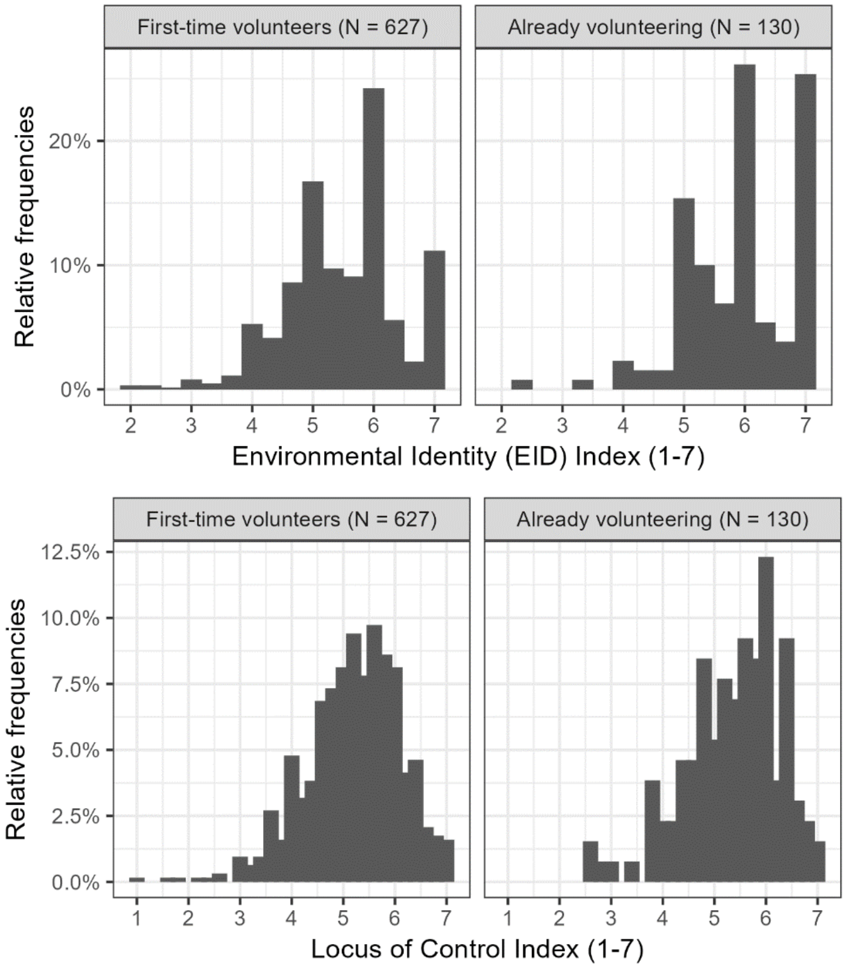

Figure A 1. Environmental identity and locus of control distributions from survey one.

The EID graphs shows that most of the people who are already volunteering “agree” or “strongly agree” that they are environmentally friendly. There are very few people who are already volunteering and do not at least somewhat agree they are environmentally friendly. Environmental identity is also high for the first-time volunteers, but there are significantly more individuals who only “somewhat” agree they are environmentally friendly or remain neutral to the statement.

The Locus of Control (LOC) graphs show a wider spread of LOC beliefs for the first-time volunteer group and that on average, those already volunteering have stronger LOC beliefs (which makes sense, given some of the LOC items relate to the effectiveness of environmental organisations).

Connection to community and nature, perceptions of restoration groups and willingness to volunteer

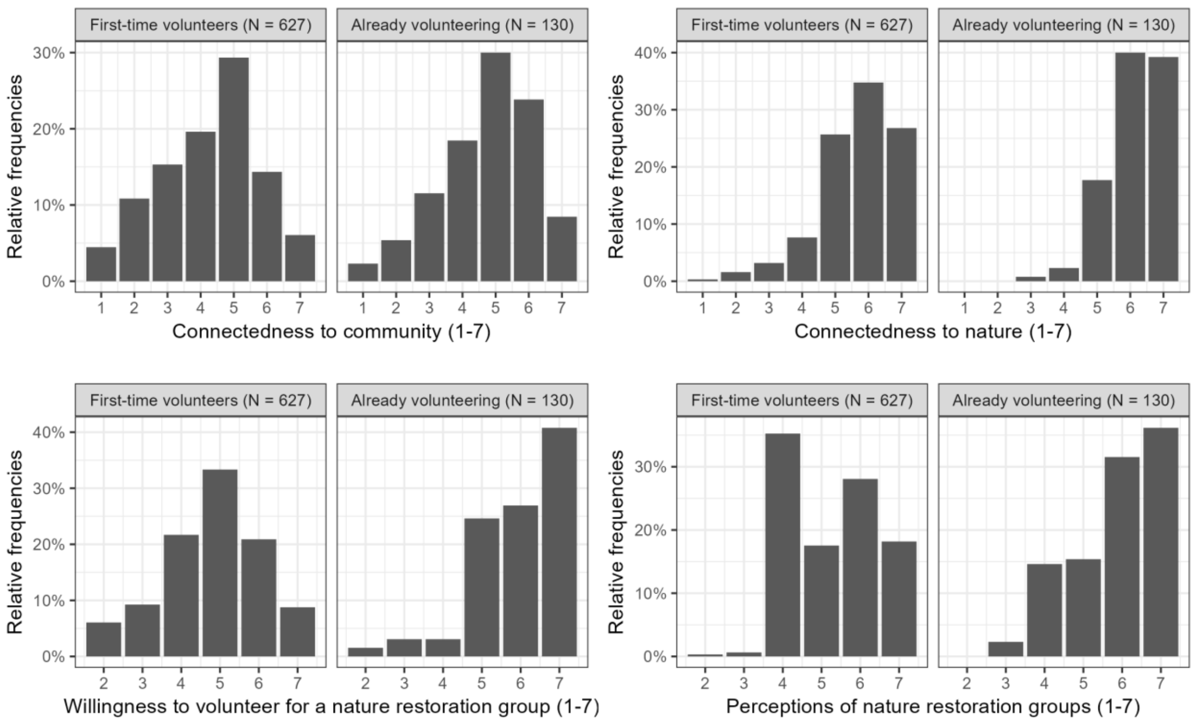

Figure A 2. Distributions of connection to nature and community and perception variables.

Those who are already volunteering have a stronger existing connection to community than the first-time volunteers. In general, few people feel strongly connected to their community, with most responses falling in the "connected" or "somewhat connected" categories. Around 30% of the first-time volunteers feel at least somewhat disconnected to their local community, which compares to 17% in the already volunteering group.

We can see that most people in both samples feel connected to nature, with the already volunteering cohort exhibiting a stronger sense of connection. Overall, only around 5% of the sample feel disconnected from nature.

Willingness to volunteer follows a bell-curved distribution for the first-time volunteers, with some being willing and others being more unwilling (the median is "somewhat willing"). Unsurprisingly, those who are already volunteering exhibit very high average willingness to volunteer.

Finally, the first-time volunteers either have positive or neutral perceptions of nature restoration groups (whereas those already volunteering generally have positive perceptions).

Knowledge of community restoration groups

Another important question we asked was in relation to how much information people had about community restoration groups. Previous studies in New Zealand and work from our wider research team suggest that information may be one of the major barriers to volunteering (Ministry for the Environment (MFE), 2021).

For first-time volunteers, most people stated they did not know much about community restoration groups, with only around 15% of respondents somewhat agreeing or agreeing that they knew much about restoration groups. Those already volunteering knew significantly more about restoration groups, but there were still many respondents who stated they did not know much about restoration groups.

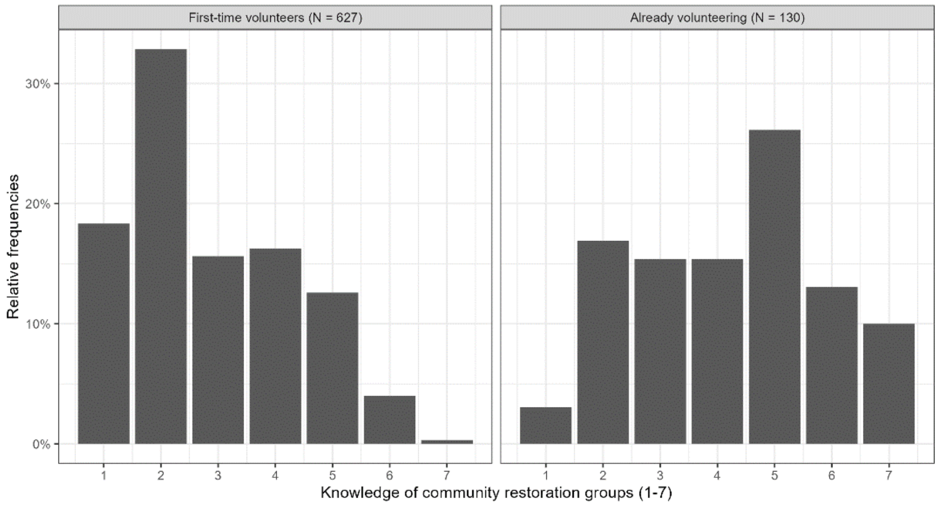

Figure A 3. Distributions of knowledge of community groups from survey one.

Who-5 scores

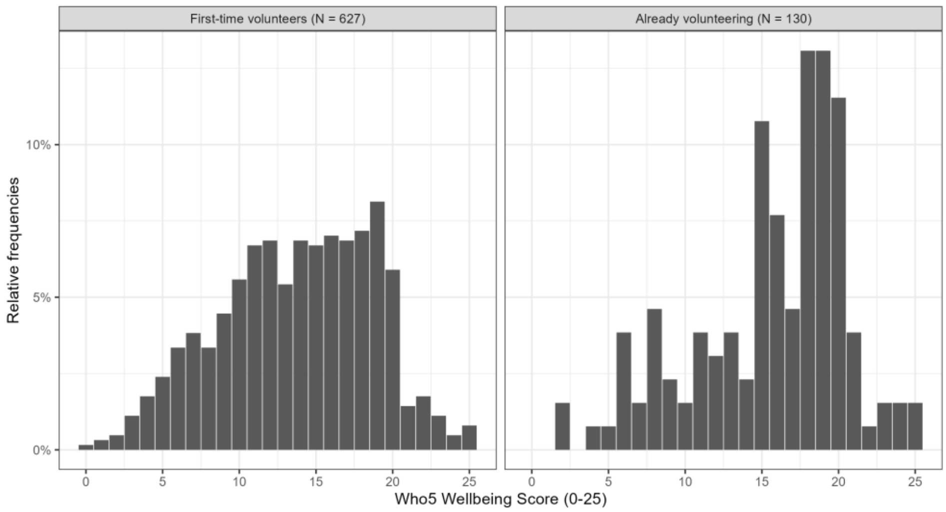

Figure A 4. Distribution of Who-5 wellbeing variable in survey one.

The final graph is of Who-5 wellbeing scores and shows that on average, those already volunteering have slightly higher wellbeing. However, in both graphs, there is a drop-off point at a score of around 20, which suggests that many respondents are stating they feel well "most of the time" but not all of the time.

Appendix I. Models predicting treatment status in stage one

Table A 1. Multinomial model predicting treatment status.

	Treatment group					
	Nudge (1)	Voucher (2)	Combined (3)	Nudge (4)	Voucher (5)	Combined (6)
Male	0.022	-0.015	-0.162			
	(0.274)	(0.277)	(0.272)			
Low income	-0.652*	0.024	0.047			
	(0.335)	(0.307)	(0.304)			
High income	0.382	-0.578	-0.122			
	(0.358)	(0.428)	(0.393)			
Maori/Pacific	0.275	0.237	-0.126			
	(0.296)	(0.290)	(0.296)			
Full time	-0.367	-0.176	-0.180			
	(0.356)	(0.354)	(0.344)			
Student	0.405	1.440**	0.359			
	(0.712)	(0.629)	(0.669)			
Retired	0.124	0.223	0.385			
	(0.536)	(0.544)	(0.510)			
Part time	0.001	0.136	-0.367			
	(0.414)	(0.402)	(0.414)			
Bachelors or higher	0.123	0.238	-0.193			
	(0.260)	(0.256)	(0.250)			
Age	-0.008	-0.012	-0.016*			
	(0.010)	(0.010)	(0.009)			
Outside Hamilton City	0.104	-0.0002	0.149			
	(0.346)	(0.365)	(0.352)			
Near Fairfield	-0.345	-0.239	-0.015			
	(0.325)	(0.312)	(0.299)			
Children dummy	-0.454*	0.105	-0.255			
	(0.262)	(0.253)	(0.254)			
EID	-0.037	-0.048	0.068			
	(0.146)	(0.146)	(0.144)			
Locus of control	-0.266*	-0.093	-0.184			
	(0.147)	(0.150)	(0.145)			
Who5 Score	0.038	0.015	0.020			
	(0.025)	(0.024)	(0.024)			
Intercept	1.774*	0.965	1.488	0.060	0.105	0.141
	(0.982)	(0.985)	(0.966)	(0.116)	(0.114)	(0.114)
N	627	-	-	627	-	-
Akaike Inf. Crit.	1,779.849	-	-	1,742.691	-	-

*Note: Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.*

Appendix J. Long-term impacts of voucher offer on donation behaviour

We run a LPM predicting donations in survey two as a function of being offered a voucher in stage one and attending an event for the first time. We include attendance to avoid omitted variable bias because by stage two, there is a positive correlation between attendance and voucher assignment and a positive correlation between attendance and donation behaviour. Hence, not including attendance may have created upwardly bias estimates for the effects of being offered a voucher on pro-environmental donations.

Table A.2. shows that the voucher has a negative but insignificant effect on long-term donation behaviour.

Table A 2. LPM of donation choice in survey two.

	Donation in survey two
Voucher	-0.061
	(0.047)
Attends an event (first-time)	0.213***
	(0.067)
Intercept	0.576***
	(0.035)
Observations	444
R^2	0.020
Adjusted R^2	0.016
Residual Std. Error	0.492 (df = 441)
F Statistic	4.572** (df = 2; 441)

Note: *BM robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

Appendix K. Ranking covariates for their influence on voucher treatment effect heterogeneity

Table A 3. Model R2 output from simple regressions of voucher ITEs for commitment on covariates.

Covariate	R^2	Cumulative R^2	Importance rank
Male	0.273	0.273	1
Children dummy	0.169	0.442	2
Bachelors or higher	0.116	0.559	3
PEB index	0.095	0.654	4
Part time	0.077	0.731	5
Māori/Pacific	0.052	0.783	6
Who5 score	0.045	0.828	7
Outside Hamilton City	0.037	0.865	8
LOC index	0.021	0.885	9
Near Fairfield	0.010	0.895	10
Relative income	0.007	0.902	11
Other volunteering	0.007	0.909	12
Full time	0.006	0.915	13
Age	0.003	0.918	14
Retired	0.002	0.919	15
Student	0.000	0.919	16

Note: Cumulative R^2 does not add to one because some of the variation is driven by combinations of factors that are not accounted for with simple linear regressions. This is also why the R^2 for the regression with the five most important variables is higher than the cumulative R^2 from the simple linear regressions. See Table 10..

Table A 4. Model R2 output from simple regressions of voucher ITEs for attendance on covariates.

Covariate	R^2	Cumulative R^2	Importance rank
Male	0.345	0.345	1
PEB index	0.191	0.536	2
Children dummy	0.084	0.620	3
Outside Hamilton City	0.079	0.699	4
Student	0.062	0.762	5
LOC index	0.062	0.823	6
Who5 score	0.039	0.862	7
Age	0.022	0.884	8
Maori/Pacific	0.017	0.901	9
Full time	0.010	0.910	10
Relative income	0.007	0.918	11
Near Fairfield	0.007	0.925	12
Bachelors or higher	0.005	0.930	13
Other volunteering	0.002	0.932	14
Part time	0.001	0.933	15
Retired	0.001	0.934	16

Note: Cumulative R^2 does not add to one because some of the variation is driven by combinations of factors that are not accounted for with simple linear regressions. This is also why the R^2 for the regression with the five most important variables is higher than the cumulative R^2 from the simple linear regressions. See Table 11..

Appendix L. Additional heterogeneity graphs for voucher impacts on attendance

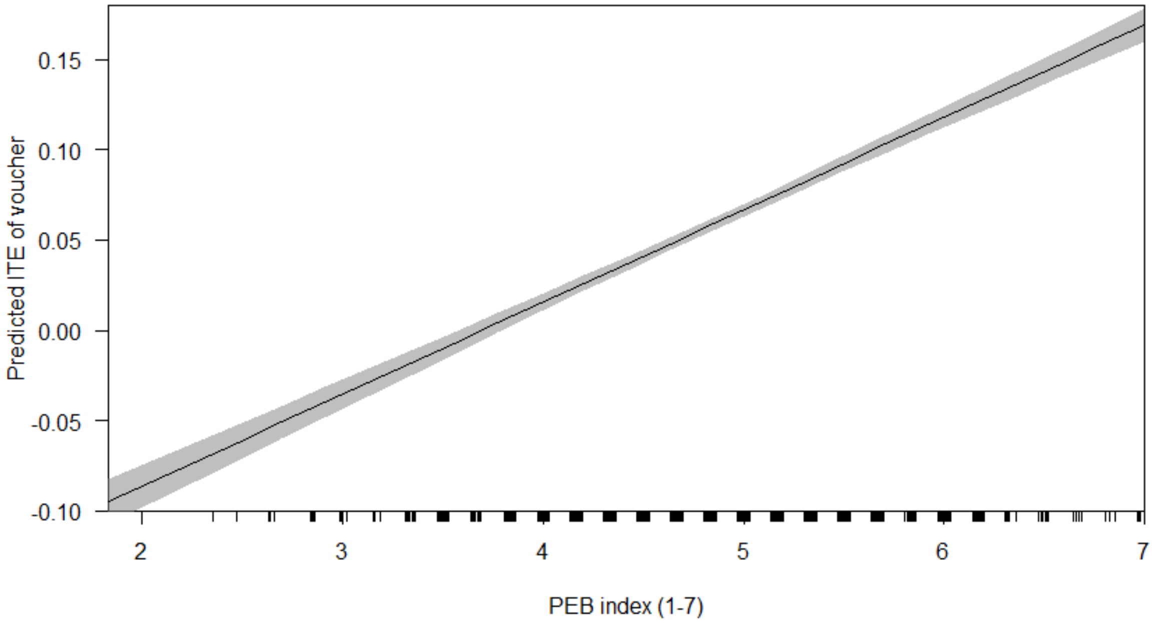

Figure A 5. Predicted voucher ITEs on attendance at different levels of the PEB index variable.

Appendix M. Balancing table for optimisation weights in stage two

Table A 5. Balancing table for optimisation weights in stage two.

Variable	Type	Mean adjusted difference (M)	M Threshold	Adjusted variance ratio (V)	V Threshold
Voucher	Binary	0.1	Balanced, <0.1	-	-
Days available	Contin.	0.1	Balanced, <0.1	1.1457565	Balanced, <2
LOC index	Contin.	-0.064319467	Balanced, <0.1	0.9466092	Balanced, <2
PEB index	Contin.	0.035850603	Balanced, <0.1	0.9503098	Balanced, <2
Who5 score	Contin.	0.1	Balanced, <0.1	0.8748612	Balanced, <2
Other volunteering	Binary	-0.040128122	Balanced, <0.1	-	-
Male	Binary	0.014427241	Balanced, <0.1	-	-
Low income	Binary	0.055072229	Balanced, <0.1	-	-
Middle income	Binary	0.044191337	Balanced, <0.1	-	-
High income	Binary	-0.099263565	Balanced, <0.1	-	-
Maori/Pacific	Binary	-0.016011977	Balanced, <0.1	-	-
Full time	Binary	-0.024445067	Balanced, <0.1	-	-
Student	Binary	0.075522801	Balanced, <0.1	-	-
Retired	Binary	0.072221792	Balanced, <0.1	-	-
Part time	Binary	-0.1	Balanced, <0.1	-	-
Bachelors or higher	Binary	-0.075777544	Balanced, <0.1	-	-
Age	Contin.	0.078164052	Balanced, <0.1	1.2674894	Balanced, <2
Outside Hamilton City	Binary	-0.080194276	Balanced, <0.1	-	-
Near Fairfield	Binary	-0.059638314	Balanced, <0.1	-	-
Children dummy	Binary	-0.06089457	Balanced, <0.1	-	-

Note: These are the balance statistics from the Cobalt package using optimisation weighting with thresholds of M = [-0.1, 0.1] and V = [0.5, 2] (Greifer, 2023). *The balance constraint for a variable is binding in the convex optimisation problem if M = 0.1 in column 3.*