Evaluating Behaviour Change Tools to Encourage Food Scraps Recycling in Auckland City

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Abstract

Food waste is a global issue with serious economic, social, and environmental consequences. In Auckland, New Zealand, around 45% of household refuse that goes to landfill is composed of food waste. The aims of this study were to evaluate a range of behavioural science interventions to increase usage of Auckland Council's kerbside food scraps collection service in pilot areas, prior to a city-wide roll out in 2023. Two separate randomised field experiments were run, in the pilot areas of Papakura and North Shore, measuring food scrap bin set out rates prior to interventions being delivered, immediately post-intervention, and seven weeks post-intervention. In the Papakura trial we randomly assigned 2,459 households, clustered into streets, to one of five treatment conditions – a proscriptive sticker prompt, attached to households' refuse bins; a postcard with appeals to social norms; free liners for kitchen food scraps caddies; household canvassing; and a no-treatment control. In the North Shore trial 1,513 households, clustered into streets, were randomly assigned to one of three treatment conditions – free caddy liners plus a postcard with appeals to social norms; free caddy liners plus household canvassing; and free caddy liners alone. In the Papakura trial, we found evidence that the sticker prompt and postcard treatments increased set out compared to a control condition. They also showed a positive effect of the sticker prompt treatment on participation (set out at least once over 3 weeks). In the North Shore trial, results showed an overall pre versus post increase of the interventions on set out and participation, and a positive effect of the postcard treatment on set out, when compared to free caddy liners alone. These findings indicate sticker prompts and social norms messaging are two promising low-cost and scalable solutions to increase use of food scraps collection services and divert food from landfills, nationwide and abroad.

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1. Introduction

1.1. Setting the context: why food waste is a problem

Food waste is a pressing and multifaceted global problem with far reaching economic, social and environmental consequences. Each year, up to one third of all food produced globally (the equivalent of 1.3 billion tons) ends up lost or wasted[1] (United Nations [UN], 2022; World Bank, n.d.). At the same time, a significant proportion of the world's population are undernourished; in 2022 this figure was between 702–828 million people (Food and Agriculture Organization of the United Nations [FAO], International Fund for Agricultural Development, UNICEF, World Food Programme & WHO, 2022). In addition to a host of environmental issues to which food waste contributes, global food demand is projected to increase by between 35%–56% between 2010 and 2050 (van Dijk et al., 2021), due to both population increases and the effects of climate change (Kummu et al., 2012). Reducing food waste is, therefore, increasingly urgent. Losses are found across the entire food supply chain: agriculture, postharvest, processing, distribution, and consumption (Kummu et al., 2012; Gustavsson et al., 2011) and the cost attached is an estimated $1 trillion (FAO, n.d.-a). Some estimates suggest that by 2030, losses are expected to increase to more than two billion tons per year (Hegnsholt et al., 2018). Whereas in lower-income countries, the primary area of food loss occurs in the early- and mid-stages of the food supply chain (Gustavsson et al., 2011), in more affluent societies a significant amount of food is lost both early in the food supply chain, and at the consumption stage when a disproportionate amount of food is wasted even though it is still suitable for consumption. On a per capita basis, food waste occurs to a far greater extent in industrialised countries, with estimates for North America and Europe ranging from 95–115 kg/year, compared with 6–11 kg/year in sub-Saharan Africa and South/Southeast Asia (Gustavsson et al., 2011).

[1] Food loss refers to food that is lost after harvest on farms and at the transport, storage and processing stages of the food supply chain, up to and excluding retail, whereas food waste refers to food that is removed from the human supply chain in the retail, food service and household food sectors (United Nations Environment Programme [UNEP], 2021, p.19).

1.2. Environmental effects of food waste

Beyond the cost to producers and consumers, food waste is one of the most significant environmental issues of our time. The food supply chain is responsible for a significant amount of environmental damage, including deforestation, pressure on already scarce water supplies in many parts of the world, soil degradation, and air and water pollution. Perhaps its most significant effect is greenhouse gas emissions (Waste Resources Action Programme [WRAP], 2019). When biodegradable material such as food waste ends up in landfills it does not compost, as landfills lack the oxygen required for this process to occur. Food waste that decomposes in landfills, instead degrades anaerobically, releasing methane, a greenhouse gas 28–36 times more potent than carbon dioxide. Methane is of particular concern since it is especially good at trapping heat in the earth's atmosphere, thus contributing to climate change (United States Environmental Protection Agency [US EPA], 2022). Diverting food waste from landfills is therefore a key way to mitigate climate change. It is estimated that globally, greenhouse gas (GHG) emissions from food loss and waste, account for around 8%–10% of total anthropogenic GHG emissions annually (Rosenzweig et al., 2020). If food waste is compared to the biggest global economies, it would follow China and the USA as the third largest emitter in the world. Moreover, the consumption phase of the food supply chain is responsible for the highest carbon footprint of wastage – 37% of total (FAO, n.d.-b).

In addition to its carbon footprint, the environmental impact of food waste extends to other facets of the food supply chain, such as water and land footprints. In a 2012 paper, Kummu et al., estimated the losses in the water, cropland and fertiliser resources involved in producing wasted food, and concluded that almost one quarter of freshwater, fertiliser and cropland used in food production is used to produce food that is subsequently wasted. This is especially problematic given that water is scarce in many parts of the world and water scarcity is a significant global issue itself (Kummu et al., 2012). Use of freshwater from groundwater or surface water for irrigation can lead to severe environmental problems including salinity,

stream depletion, waterlogging or soil degradation (Trade and Agriculture Directorate, 2016; FAO, 2013). Globally, most food production makes use of synthetic fertilisers which contain the non-renewable natural resources potassium and phosphorus. Fertiliser use is also associated with water degradation and decreased biodiversity (Kummu et al., 2012). Productive land is also a scarce resource. Most of the earth's total global cropland is already established in the areas that are most suitable for growing crops. Pressure on food systems due to population growth means that expansion of new cropland is often into areas that are marginal for cropping and is associated with environmental degradation, deforestation, and loss of biodiversity (Kummu et al., 2012; FAO, 2013; Feldstein, 2017). If the total land area used to grow food that is subsequently lost or wasted were compared to the world's largest countries by land area, then land used for producing losses would be second, after Russia (FAO, 2013). Oceania along with North America, are the regions with the largest losses relative to total cropland used; they also have the highest per-capita fertiliser use (Kummu et al., 2012).

The aforementioned social, environmental, and economic impacts have led the United Nations to respond by including "Reducing Food Waste and Loss" as one of its Sustainable Development Goals – 12.3: "By 2030, halve per capita global food waste at the retail and consumer levels and reduce food losses along production and supply chains, including post-harvest losses" (UN, 2015). Given that an estimated 61% of post-harvest food waste comes from households (570 million tonnes, which equates to a global average of 74 kg per capita annually; UNEP, 2021), there is an urgent need for effective household-level solutions, which requires looking at the causes of food waste in the home.

1.3. Causes of and solutions to the food waste problem

Common reasons that households waste food include lack of meal planning or (mental or physical) shopping lists; retailer practices, leading to impulse buying in the grocery store; lack of understanding of food labels; improper storage of purchased food, leading to spoilage;

cooking too much food; not saving and appropriately storing leftovers; and poor cooking skills (van Geffen et al., 2020; Gaiani et al., 2018). Expiration date labels are a particular problem, as they are confusing and arbitrarily restrictive, leading to food still suitable for consumption being discarded (Hegnsholt et al., 2018; Reynolds et al., 2020).

There are many solutions directed at preventing food waste at the household level. Initiatives such as the Waste and Resources Action Programme's (WRAP's) Love Food Hate Waste initiative, which began in the UK in 2007 and has since been adopted in Australia, New Zealand, and Canada, have implemented several successful campaigns (WRAP, n.d.-a). Some strategies used by Love Food Hate Waste include Food Waste Action Week: a week of action dedicated "to raising awareness of the environmental consequences of wasting food, and promoting activities that help to reduce the amount of food we waste" (WRAP, n.d.-b); and a post-Covid-19 lockdown campaign aimed at encouraging citizens to continue, or commence smart food behaviours (such as planning ahead before shopping, freezing food, and inventive cooking) that were observed during the lockdown period. Useful tools, such as a *portion planner* that calculates the exact amount of each ingredient needed for family meals, helped facilitate adoption of smart food behaviour (WRAP, n.d.-a). A recent review of the behaviour change literature on interventions targeting food waste reduction identified the following strategies used in the context of household food waste: informational or educational campaigns aimed at increasing abilities or knowledge (such as information on how to avoid impulse buying, optimally store food, or create a delicious meal with one's leftovers, or warnings about the consequences of food waste); helping people commit to food waste reduction, for example, by signing a pledge; modelling social norms, for example, a video portraying food practices that reduce food waste; feedback such as statistics or information regarding personal food waste behaviour; rewards – either monetary or praise; and penalties, for example, social shaming and monetary consequences for high levels of food waste (Stöckli et al., 2018). Focusing on reducing household food waste at the retail level by targeting purchasing, food-preparation, and storage behaviour, is undoubtedly a valuable aim, since the

earlier in the food supply chain food waste prevention occurs, the greater the environmental benefits.

1.4. Downstream mitigation

Recognising however, that some food waste is inevitable, actions to divert food waste from landfill are also important. Consequently, focusing on the downstream effects of household food waste is another important element in food waste management and is part of the food waste management hierarchy advocated by the United States Environmental Protection Agency (EPA), WRAP, the European Parliament, as well as many local authorities and private organisations (US EPA, n.d.-a; EUR-Lex, 2008; Herszenhorn, et al., 2014; Redlingshöfer et al., 2020; Mourad, 2016). In fact, wasted food is increasingly viewed not as a useless byproduct, but as a useful and underutilised manufactured product (Albizzati et al., 2021). Despite encouraging and/or subsidising home or community composting, by some governments and local authorities (see for example, Ricci-Jürgensen et al., 2020; Compost Collective, n.d.; Sherman, 2020; Compost Connect, 2022), household composting prevalence remains low in most countries. For example, estimates of home composting in the European Union indicated that this practice only accounted for 8% of food waste produced by households (UNEP, 2021) and for the United States only around 4% of wasted food is composted (US EPA, n.d.-b). To complement home composting, an increasing number of local authorities around the world have implemented organics or food waste collection schemes. When food waste is collected for recycling, the most common route it takes is anaerobic digestion (AD). Another method is commercial composting. Both methods are considered an environmentally-friendly alternative to landfill or incineration (Cerda et al., 2018). AD is the most common method of processing food waste globally and is used extensively in North America and Europe and to a much smaller extent in other regions of the world (Ricci-Jürgensen et al., 2020). The process of AD breaks down organic material without the need for oxygen, which results in digestate – a nutrient-rich material that can be used as fertiliser, soil

enricher and livestock bedding – and methane-rich biogas that can be used as a source of renewable energy (US EPA, 2022; Xu et al., 2018).

1.5. Beyond providing information

One of the primary tools available for reducing food waste is behaviour change strategies targeted at the general public. Traditional behaviour change interventions often focus on providing people with information, knowledge and/or skills to increase uptake of the desired behaviour (von Kameke & Fischer, 2018). However, information-based behaviour change campaigns have had limited success (McKenzie-Mohr, 2000). Evaluations of pro-environmental information campaigns show mixed results and only a weak relationship between information provision and behaviour change (Linder et al., 2018). Sometimes information can indeed be useful in overcoming barriers when they are centred around lack of knowledge and ability, when some baseline motivation for doing the action is present and when barriers are low (Brain & Thomson, n.d.; WRAP, 2021b). One study found that while providing information about environmental consequences of consuming bottled water increased people's knowledge, it only produced behaviour change among those who already had strong pro-environmental values (Bolderdijk et al., 2013). When barriers are high and baseline motivation is lacking, information provision on its own is unlikely to work, and additional tools are required (WRAP, 2021b; McKenzie-Mohr & Schultz, 2014; von Kameke & Fischer, 2018).

There are additional reasons why information provision alone is usually not enough to change behaviour. For many years it was assumed that human decision making was the result of conscious, deliberative thinking. The assumptions of the classical model of rationality are that humans are fully rational creatures with unlimited cognitive capabilities and willpower, who are primarily guided by self-interest to always make optimal choices (Kahneman, 2003; Kahneman, 2011). However, evidence from behavioural economics has shown that our willpower, self-interest, and rationality are bounded, we are beset by cognitive biases, and

make decisions based on the limited information we have available to us at the time, using heuristics and habits to guide us (Kahneman, 2003). For most people, most of the time, there is simply not the time, effort or motivation to always think consciously, rationally and reflectively (Marchiori et al., 2017). In particular, we are subject to the following constraints on our decision making:

- We have limited self-control. Exercising self-control requires significant physiological effort. It is helpful to think of self-control as a limited "commodity" and when we use some up on one task, we have less available for other tasks (Datta & Mullainathan, 2014). This can result in one's good intentions not being acted upon; the aptly named intention-behaviour gap. The intention-behaviour gap helps to explain why progress that individuals make towards environmental change is often limited (Barr, 2006; Vermeir & Verbeke, 2006; Flygansvær et al., 2021; Sheeran & Webb, 2016). It also explains why information provision alone will often fail in changing behaviour. Information can help create good intentions but fails to address self-control problems.
- We suffer from inattention. Limited attention is a well-established psychological phenomenon. People do not pay attention to all aspects of their environment; it is impossible to do so since we are constantly assailed by stimuli. Instead, we unconsciously filter out a great deal of information, paying selective attention to certain aspects while ignoring others (Dolan et al., 2012). The information that does get processed and considered in consciousness is that which is salient to us (Dolan et al., 2012). Important but non-salient information may simply be missed. Inattention is another reason why simply providing people with more information, often, does not by itself change behaviour; people are unlikely to pay attention to all aspects of a topic and may miss those aspects that are most important (Datta & Mullainathan, 2014).
- We possess limited cognitive capacity to process complex information. It has traditionally been typical of behaviour change programmes to focus on increasing

the amount and/or complexity of information provision, with the idea that the more knowledge humans have, the more likely they will be to change their behaviour. Such an approach assumes that humans are capable of easily and rapidly processing large quantities of complex information. The research is clear however, that this is not the case. We only have a limited amount of mental resources available for processing complex information at any given moment, which is easily depleted when spent on other tasks. Increasing the cognitive demands on a person by giving them more complex information in order to change behaviour can result in a failure to succeed in the aims (Datta & Mullainathan, 2014).

- We have a scarcity of understanding. Even when information is attended to, plentiful and not overly complex, people may still suffer from scarcity of understanding. This involves our underlying mental model of the world being flawed, so that no matter the information in the community, it is unlikely to have any effect. A health-related example helps to illustrate this constraint. Uptake of oral rehydration solutions (ORS) as a treatment for paediatric diarrhoea in many developing countries remains low, despite its affordability and accessibility, and the widespread advertising and awareness-raising campaigns about its benefits. It is often assumed that more information about ORS and how it works, is the solution. However, a flawed mental model of causation is likely to blame. Many parents erroneously believe that increasing liquid intake will exacerbate their child's diarrhoea; that it is better to keep the child "dry". As a result, ORS will not be considered by parents with such an understanding of causation (Datta & Mullainathan, 2014).

As a result of these constraints on our decision-making capabilities, we often end up making decisions that are suboptimal or even irrational.

To help us navigate life, and in particular, to deal with the constraints just outlined, we use fast, intuitive thinking; we unconsciously employ heuristics – mental shortcuts (Datta & Mullainathan, 2014). There is now a large body of evidence showing that much human

behaviour and decision making is based on heuristics, habits, unconscious associations and automatic responses (Marchiori et al., 2017). The mechanisms our brains have evolved to deal with information-processing constraints can lead to cognitive biases: unconscious systematic deviations from rationality. There are numerous cognitive biases that have been identified, but some of the most well-known include: confirmation bias (Mynatt et al., 1977), hindsight bias (Fischhoff & Beyth, 1975), inattentional blindness (Rock et al., 1992), loss aversion (Kahneman & Tversky, 1979), and present bias (O'Donoghue & Rabin, 1999). A large body of evidence for the many constraints on people's decision making has led to the general acceptance of bounded rationality, and to the development of alternative approaches to behaviour change that go beyond awareness raising and information provision which target our rational selves.

1.6. Behavioural Insights and "nudge" interventions

The Behavioural Insights (BI) approach applies findings from psychology, economics, and other social sciences about how humans behave and make decisions. Its aim is to use this knowledge to design interventions that can help people make better choices and live better lives. BI involves three elements: drawing on evidence about human biases, heuristics, and errors; applying that evidence to real-world choices and behaviour change interventions; and evaluating the effectiveness of these interventions through field experiments (Behavioural Insights Team [BIT], 2020). BI starts from the assumption that much of our behaviour and decision making is based on *fast thinking*, such as heuristics and unconscious associations. BI-informed solutions, primarily simple *nudges*, are designed to help people overcome attentional and cognitive limitations. A nudge is some aspect of the choice architecture (decision-making context) that steers behaviour in a predictable direction without forbidding alternative options (Thaler & Sunstein, 2021). Policymakers (who are choice architects) can use BI to design better policy. There is ongoing debate about the extent to which nudging works on fast thinking versus slow thinking (reflection and deliberation), but evidence is growing that being aware of the presence and purpose of a nudge does not diminish its

effectiveness (de Ridder et al., 2021). Status quo bias (Samuelson & Zeckhauser, 1988) and present bias (O'Donoghue & Rabin, 1999) are two predictable biases that choice architects can address using nudges. Status quo bias is the tendency to stick with what one has always done and can be countered using defaults (where one option has been pre-selected as the option to be used if no active choice is made), such as in the case of green energy defaults to promote energy conservation. A large, randomised trial showed the effectiveness of green energy defaults in promoting energy conservation: 69.1% of households defaulted into the green energy treatment condition stuck with the plan, compared to only 7.2% who chose green energy when required to opt-in (Ebeling & Lotz, 2015). Present bias is the tendency to give more weight to immediate decisions, while discounting decisions whose consequences will be born in the future. This bias can be countered by using defaults, as well as commitment strategies, for example to help people to save more (Thaler & Benartzi, 2004; Ashraf et al., 2006), and framing effects, for example, framing health messages as gains rather than losses (Gallagher & Updegraff, 2011).

Various frameworks have been developed which summarise strategies for developing behaviour-change interventions. One popular example, developed by the UK Government's Behavioural Insights Team, is the EAST framework. EAST consists of four simple principles for applying behavioural insights: if you want to change behaviour, then making it Easy, Attractive, Social and Timely can help (Service et al., n.d.). By making it easy, you reduce friction by removing the psychological and/or physical barriers to engaging in the behaviour. Tools for "making it easy" can include the use of defaults, simplifying processes or the messages that people see, (for example, highlighting allergens on food labels) and removing physical barriers. For example, during the Covid-19 pandemic, mobile vaccination clinics made it easier for people with mobility issues, limited access to public transport and medical providers and family or work commitments, to get vaccinated (Centres for Disease Control and Prevention, 2021). Making the behaviour attractive can include using colour, images, or personalisation to draw attention to it, offering financial incentives such as lotteries, or framing

messages. For example, changing the wording of vegetarian food labels from “meat-free” to “field grown” can make vegetarian food more attractive (Bacon et al, 2018). “Making it social” involves using social norms, networks and public commitments to utilise the power of social influence in behaviour-change. Finally, “making it timely” involves strategic use of prompts and reminders, helping people plan, and engaging people when they are most receptive. For example, a number of studies have demonstrated that motivational point-of-decision visual prompts can effectively be used to increase stair use (Soler et al., 2010). Typically, a motivational sign is placed in a prominent location at the bottom of a flight of stairs, where it will be seen by people who otherwise, may habitually have taken the elevator.

Another useful schema was introduced by Mertens et al. (2022) in their recent meta-analysis evaluating over 450 effect sizes from more than 200 studies in different domains. Mertens et al. use a framework based on the type of psychological mechanism at play, to classify types of choice architecture interventions. The researchers separated interventions into three broad categories based on their mechanism of action:

(1) decision information: the provision of decision-relevant information to increase the availability, improve comprehensibility and/or personal relevance of information. Examples include, translating, making information more visible or providing a social reference point. Nudges of this type target the psychological barrier of limited access to decision-relevant information. Translating forms full of technical jargon into everyday language can facilitate understanding of the existing information. Providing social normative information can minimise situational ambiguity and behavioural uncertainty, by providing people with clear cues about how others behave or expect people to behave. The personal relevance of decision information is thereby enhanced.

(2) decision structure: altering the arrangement of choice alternatives or conditions of decision making. Examples include, strategically using defaults, removing physical or financial effort to remove friction from a desirable choice option, changing the range or composition of choice options, or altering option consequences. Targeting the decision

structure makes the desired behaviour easier to perform, by addressing the psychological barrier of limited capacity to evaluate and compare choice options. Contrary to the assumption of the classical model of rationality, people do not engage in a cost-benefit analysis of choice options for every, or even important decisions; rather they utilise information about how the options available to them are arranged and structured within the decision-making environment, to guide their actions.

(3) decision assistance: facilitating the translation of good intentions into action, when people have made deliberate decisions to change their behaviour. Examples include, providing reminders, and encouraging public commitment to the desired behaviour. These types of interventions target the psychological barrier of limited attention and self-control. Providing reminders can help combat information overload, by making the desired behaviour more salient and thereby capturing attention, while making commitments can help address procrastination and overcome limited self-control (Mertens et al., 2022).

How effective are the nudge type interventions advocated in the BI approach to behaviour change? In their meta-analysis, Mertens et al. (2022) evaluate the efficacy of nudges across each of the three mechanisms in six different domains – Health, Food, Environment, Finance, Prosocial, Other. On the basis of their review, they claim that choice architecture "is an effective and widely applicable behaviour change tool that facilitates personally and socially desirable choices across behavioural domains, geographical locations, and populations" (p.8). Of the three mechanisms, nudges targeting decision structure were the most effective, with effect sizes ranging from Cohen's d=0.43–0.62 and a statistically significant difference over the other two conditions in a pairwise comparison. Furthermore, when comparing the effectiveness of the three intervention categories across the six different domains, decision structure interventions consistently had the largest effect on behaviour (Mertens et al., 2022). Defaults were found to be the most effective type of nudge, while altering the range or composition of options, changes to increase ease/reduce effort, and changes to the

consequences attached to options (all examples of decision structure alterations) produced the next largest effect sizes (Mertens et al, 2022).

Recently the claims of Mertens et al. (2022) about the overall effectiveness of nudging have been challenged, with critics pointing to possible publication bias (Maier et al., 2022). However, it is worth noting that even proponents of nudging do not claim that nudging always works or is a panacea for all of society's problems (Thaler & Sunstein, 2021). Effect sizes vary substantially across the literature with some studies showing very large effects (Khern-am-nuai et al., 2022; Johnson & Goldstein, 2003; Madrian & Shea, 2001). Rather than impugning the whole field, this suggests more research is needed into whether and under what conditions nudges are effective.

Advocates of nudge interventions also argue that even if effects can be small or marginal, they are easy to implement (Benartzi et al., 2017), easily adapted to different contexts (Barker et al., 2021) and often inexpensive, offering potentially high impact per dollar spent. Common nudge-interventions such as default-choices or changing the physical environment to make a desirable choice more attractive, cost the choice architect very little, if anything, to implement. Other common nudge interventions such as social norm messaging or framing can be implemented relatively easily and inexpensively, for example, by including such messaging in letters that are already being sent to customers. In comparison, large-scale information and advertising campaigns – a traditional behaviour change policy tool – are often very expensive to run (McKenzie-Mohr, 2000).

Questions about the efficacy and cost effectiveness of nudging aside, there are ongoing debates about the desirability of an approach to social policy that focuses on nudging. In their seminal book, *Nudge*, Thaler and Sunstein (2021) call this approach to social policy *libertarian paternalism.* Nudging is libertarian in the sense that it is choice-preserving; the same set of choices are available as they would be without the nudge. If one wants to, they are free to opt

out of automatic enrolment into their organisation's pension scheme, to choose a burger rather than a salad, and to not get vaccinated. It is also paternalistic, in the sense that choice architects use nudges to influence people in ways that will improve their lives. This is a "soft", as opposed to "hard" paternalism, however. As Thaler and Sunstein (2021) write in *Nudge*:

> The paternalistic policies that we favor aim to influence choices in a way that will make choosers better off, *as judged by the choosers themselves.* This is a paternalism of means, not of ends; those policies help people reach their own preferred destination. (p.7)

Despite repeated assurances that nudging is a choice preserving, non-invasive approach to improve people's lives, libertarian paternalism has faced criticism from both the political left and right. One main criticism is that nudges are unethical since they manipulate people without their knowledge. However, this criticism is based on several flawed assumptions about the psychological mechanism of nudges: that they are able to easily influence behaviour and have a clear and predictable impact on decision-making because they target automatic, cognitive processes; that their efficacy is based on non-transparency; and that they can affect a person's choice regardless of their preexisting preferences for a particular option (de Ridder et al., 2021). A recent study found that these assumptions are not well supported. The researchers found that nudges are not necessarily effective when targeted at fast thinking and in many cases, they do not solely target fast thinking. Furthermore, transparency about nudges does not affect their efficacy, and nudges that conflict with preexisting preferences are generally ineffective. Nudges are most effective when people's choices are not well-developed. These findings suggest that concerns about the legitimacy of nudging should be reduced (de Ridder et al., 2021).

A second criticism is that nudging is not enough, and worse, detracts from structural reform by providing a "quick fix" solution that addresses symptoms rather than underlying problems. Nudges may also be seen as a way for governments or organisations to avoid taking more

comprehensive and potentially unpopular actions, such as passing new laws implementing bans or mandates, instead, shifting responsibility to the individual (Chater & Loewenstein, 2022). Chater and Loewenstein (2022) argue that the effects of nudges are modest at best and that they are insufficient to deal with many of the problems that currently face humanity (a point on which proponents of nudging agree; see Thaler & Sunstein, 2021, p.328). However, they also contend that the focus on nudging (which they call *i-frame* or individual-level interventions, in contrast to *s-frame* or system-level change), is harmful since it draws attention away from and reduces support for effective s-frame legislation. In response, nudging has never been advocated as a replacement for s-frame policy, but rather a complement to the latter (Thaler & Sunstein, 2021). Secondly, policymakers often face situations in which large-scale institutional changes are not practical or politically viable. In these non-ideal circumstances, nudging can still be a pragmatic and effective approach for addressing issues that would ideally require more comprehensive solutions (Schmidt & Engelen, 2020). It is evident that, while not guaranteed to succeed and not a panacea for the many problems our societies face, BI offers a variety of theory-driven, cost-effective behaviour change interventions and the empirical tools to evaluate their efficacy. Here, the approach is being explored in the context of pro-environmental behaviour (PEB), acknowledging that it can only ever be part of the solution. Despite its limitations, BI provides a valuable approach to behaviour change and should be considered as a complementary tool for promoting PEB.

1.7. Behavioural science interventions for pro-environmental behaviour

We have seen that nudging can work and that there is some evidence of its efficacy when applied to PEBs. Several published papers support this claim. A 2019 meta-analysis evaluating behavioural interventions promoting household action to mitigate climate change, found that although effect sizes were small, nudge-based interventions produced the largest average effect size (d=0.35) and that those targeting recycling behaviour had the largest effect (Nisa et al., 2019). A 2021 systematic review of recent studies involving nudge interventions to promote PEB, also found that the majority had effective outcomes – only 5.41% of studies

included in the review had ineffective outcomes (Wee et al., 2021). Mertens et al.'s (2022) meta-analysis (discussed in Section 1.6) found that choice architecture interventions applied in the environmental domain had an average effect size of d=0.43. A 2018 systematic review of 160 interventions aimed at PEB changes found that the most promising intervention types were commitments, defaults and norms (Byerly, et al., 2018). However, the researchers also conclude that there remain large gaps in our knowledge about how certain interventions affect choices and that future research is needed. Finally, a 2012 meta-analysis of 253 treatments to promote PEB, that measured observed behavioural outcomes, found that the largest overall effect sizes were from treatments that included cognitive dissonance, goal setting, social modelling, and prompts (Hedge's g>0.60; Osbaldiston & Schott, 2012). They also found that there is no one treatment that is most effective across all types of PEB; some treatments were more effective for specific types of PEBs. As the current study is concerned with behavioural interventions to encourage a PEB (food scraps recycling), these findings are encouraging. The types of behavioural interventions that seem most promising for application in the context of this study include: changes to increase ease (decision structure); messaging that appeals to social norms (decision information); and visual prompts, which can simplify information and act as timely reminders or provide cues to help decision making (decision information and decision assistance). In the following section we will consider these intervention-types in more detail.

1.7.1. "Make it easy"

One of the most commonly used types of choice architecture are those that make it easier to perform the desired behaviour. "Make it easy" interventions are an example of altering the decision structure (see Section 1.6). By removing physical or financial effort they make the desired option easier to choose (Mertens et al., 2022). Nudges which make it easy, are popular, because they are simple to implement and require the least level of engagement of participants (Osbaldiston & Schott, 2012).

In their 2012 meta-analysis of experimental interventions for promoting PEB, Osbaldiston and Schott (2012) found that interventions which focused on changing situational conditions to make performing behaviours easier, such as relocating recycling bins to more convenient locations or providing low-flow shower heads for water conservation, had medium effect sizes. There were 19 studies of this type, and the average effect size was g=0.46. The analysis also found that when the type of intervention (for example “make it easy”, prompts, cognitive dissonance, feedback, social modelling, and so on) was broken down by type of PEB (for example, kerbside recycling, energy conservation, water conservation), some interventions were more effective at encouraging certain types of behaviour. For example, intervention-types that were most effective for promoting kerbside recycling were “make it easy” and rewards.

1.7.2. Visual prompts

Another promising nudge for encouraging PEB is the use of visual prompts, which can combine both decision assistance and decision information (Mertens et al., 2022). Visual prompts such as signs, posters, stickers, or digital visual imagery can give viewers useful information, influence their opinions, and guide their actions in making decisions (Sussman et al., 2013). Visual prompts also function as timely reminders to prompt people to engage in a behaviour at the point-of-decision, when they may otherwise be acting habitually or are inattentive. If well designed, visual prompts can increase the salience of a message. They may also be used to simplify information, making it easier for people to engage in the target behaviour. If they incorporate normative messaging, they are also *social* nudges.

The literature identifies several key aspects of effective visual prompts. They work best when engaging in the target behaviour is relatively easy to do; the behaviour being encouraged or discouraged is clearly defined; when trying to avoid an undesired behaviour, alternative desirable behaviours that are easy to do, are provided; the message is presented near opportunities to perform the desired action (such as advertising at the point-of-purchase); and

the language used in the message is polite and does not limit the individual's perceived freedom (Sussman et al., 2013). Effective visual prompts may also include pictures alongside text, which can aid with communication of the message – as long as the picture is congruent with the text. Finally, text and images should be clear, simple and unambiguous (Sussman & Gifford, 2012).

Visual prompts have been found to be effective at encouraging a range of behaviours in a variety of different domains (Sussman & Gifford, 2012). In the domain of PEB, visual prompts have been used successfully to increase polystyrene, plastic, and paper recycling, composting, energy conservation, and decrease littering in cafeterias and football stadiums (Sussman et al., 2013; Bergquist & Nilsson, 2016; Essl et al., 2021). One study found that a simple, well-placed sign encouraging people to switch the light off when exiting office meeting rooms produced a statistically significant increase in the likelihood of lights being turned off after meetings. The visual prompt increased the salience, timeliness and specificity compared to the old sign (Tetlow et al., 2014). A recent study found that using either stickers or flyers as timely visual prompts was effective in increasing plastic reuse. The field experiment aimed to increase the return rate of plastic bags in food boxes delivered to customers of a Swiss agricultural association. The researchers found that both a sticker-prompt and a flyer reminder produced statistically significant positive effects, increasing the return rate by 83% compared to a control group (Essl et al., 2021).

1.7.3. Social norms

Interventions that use social normative information are examples of decision information (Mertens et al., 2022). By signalling how others behave and expect people to behave they enhance decision-relevant information. Social norms are generally accepted values or standards shared by members of a social group, pertaining to the appropriate way of thinking, feeling, or acting. They can have a significant impact on people's behaviour and decision-making, as people seek to conform to social norms to maintain their membership in a group

and avoid rejection (Turner, 1996). Research in social psychology has shown that people are influenced by others, both positively and negatively (Asch, 1956), and that social norms can be a powerful motivator for behaviour change (Cialdini et al., 1991). The formation of social norms can occur through various mechanisms, such as group interactions, top-down leadership, and comparison with other groups (Smith, 2020). Since the pioneering work by Asch (1956) on social influence and conformity, much further work has been done. There is a distinction between two types of social norms: descriptive and injunctive. Descriptive norms describe how people actually behave. They work because people (a) use the behaviour of others as a standard by which to guide their own behaviour, and (b) tend to grossly over- or underestimate the prevalence of behaviours in society (Schultz et al., 2007). Injunctive norms reflect society's approval or disapproval of behaviour, and have connotations regarding how one should behave (Cialdini et al., 1991). Evidence suggests that social norms only influence behaviour directly when they are made salient and focal in attention (Cialdini et al., 2006), that they work best when they are aligned with the target behaviour and therefore draw attention to what most people do (descriptive), or believe is right (injunctive), respectively (Mertens & Schultz, 2020), and that people often underestimate the effect of norms on their own behaviour (Cialdini, 2007; Nolan et al., 2008). There is also a growing body of research on “conspicuous conservation”, which shows that people care about their “green” status and how it compares to others (Carlsson et al., 2021). This suggests that messaging that conveys comparative social norms can be particularly effective in promoting PEB-change.

The research findings about social norms have been applied to a variety of health, financial and environmental contexts. Many studies have shown that incorporating social norms into efforts to change behaviour can be effective in promoting PEB (Farrow et al., 2017). Descriptive social norms have often been used in the form of written feedback, for example, a utilities bill highlighting one’s own consumption and comparing it with that of a relevant reference group. One well-known area of application is power consumption. Studies by Schultz et al. (2007), Nolan et al. (2008), Allcott (2011) and Ayres et al. (2012) for example,

have shown that when an individual's focus is drawn to the behaviour of others like them, they reduce their energy consumption. Ayres et al. (2012) demonstrated that when feedback on consumption of household electricity and natural gas was provided to customers, while also drawing their attention to peer comparisons, consumption levels decreased by 1% to 2%. The highest consumers reduced their consumption while those below average did not increase theirs. Moreover, the effect was sustained over time. Other studies have demonstrated similar findings with respect to reducing water use (Ferraro et al., 2011, Ferraro & Price, 2013), hotel guests' reuse of towels (Goldstein et al., 2008), sustainable transportation (Kormos et al., 2014) and improving recycling performance (Schultz, 1999; Cialdini, 2003; Kip Viscusi et al., 2014; Milford, et al., 2015; Dupré & Meineri, 2016; Cosic et al., 2018; Czajkowski et al., 2019).

Social norm-based interventions are not always successful. One phenomenon that often undermines the effectiveness of such interventions is the *boomerang effect*. This occurs when communicating descriptive norms leads to a counterintuitive result and a rise in the opposite behaviour to what was intended (Schultz, et al., 2007). It is caused by alerting people that the undesired behaviour is common, thereby normalising that behaviour. For example, in an energy consumption study, communicating the average household consumption as a descriptive norm caused low consuming households to increase their usage (Cialdini et al., 1991). The boomerang effect is a particular problem for many PEBs, as they are often not yet widespread. Combining an injunctive norm, which communicates social approval or disapproval, with a descriptive norm can be effective in combating the boomerang effect. In another energy consumption study, simply adding an injunctive norm removed the boomerang effect (Schultz et al., 2007). In a study on organ donation, the combination of messaging involving high injunctive and low descriptive norms was found to increase initiating donor registrations by almost 70% compared to using either norm alone (Habib et al., 2021). A third study compared different combinations of positively and negatively framed descriptive and injunctive norms to promote a PEB and found that negatively framed injunctive norm messages had the largest effect (Cialdini et al., 2006). The finding is interesting; it could be

explained by evidence suggesting that negative stimuli are more salient than positive stimuli, leading people to focus more on the content of a negatively worded message. The findings of these studies suggest that in situations where the desirable behaviour is low, care must be taken in how and when descriptive norms are conveyed, and that negatively framed injunctive norms can be an effective alternative.

Studies investigating how reference group specificity mediates the effects of social norms have had mixed results. Evidence of a local norm effect (where descriptive norms are most effective when the immediate situational circumstances of the participants are similar to those in which the group norm is found) has been found in studies by Goldstein et al. (2008) on improving hotel guests' reuse of towels, Schultz (1999), and Nomura et al. (2011) on improving household recycling, and Agerström et al. (2016) on increasing charitable giving. However, Mertens and Schultz (2020) and Czajkowski et al. (2019) failed to find evidence that a local norm effect was larger than the effects of other descriptive norm messages, for encouraging residential recycling. Studies that attempted to partially or fully replicate Goldstein et al.'s (2008) hotel towel study, failed to find any support (Bohner & Schlüter, 2014).

Other studies have highlighted the role of ideology and socio-cultural factors in mediating the effectiveness of social norms as a behaviour change tool. A 2017 German replication of the Alcott (2011) study found a smaller descriptive norm effect on energy consumption (Andor et al., 2017). German households have lower energy consumption than the US, where the original study was conducted, suggesting high baseline consumption is needed for an effect to be seen. Another study found that using social comparisons in home energy reports was more effective for liberals than conservatives – who were more likely to opt out and dislike the report (Costa & Khan, 2013). The findings indicate that to maximise efficacy, energy conservation social norm nudges need to be targeted. Finally, some studies have not found any evidence that social norms change behaviour (for example, Silva & John, 2017; Bohner & Schlüter, 2014). Overall, the mixed results suggest that responses to social norm

interventions vary greatly. Schultz (2014) notes that as with other nudges, social norm nudges to promote PEB are successful “at least in some contexts, for some behaviors, and for some individuals” (p.107). It can therefore be expected that how a specific behavioural intervention will affect people, is context dependent, and will differ between countries and cultures (Czajkowski et al., 2019).

1.8. BI approaches to food scraps recycling

1.8.1. Barriers to overcome

Even when people are aware of food waste recycling and its purpose, there are many barriers that prevent people engaging in the behaviour, making the area ripe for the application of Behavioural Insights approaches. Notably, there is clear evidence of an intention-behaviour gap with respect to food waste recycling. This means that while consumers may have good intentions to use their local authority-provided food waste collection service, their actions do not reflect this. For example, in a Swedish study, participation in food waste recycling was low, despite pre-intervention surveys which revealed that the majority of residents supported it (Linder et al., 2018). Another study, in Oslo, Norway, reported that in 2019, 97% of people had positive or neutral recycling intentions, but the collection rate was only 38% (Flygansvær et al., 2021). This intention-behaviour gap suggests that there are often significant barriers to action. Potential barriers include: being time-poor, which means that often recycling is not prioritised (Flygansvær et al., 2021); relatedly, the associated inconvenience, for example, of having to clean bins (Allison et al., 2022; WRAP, 2021a; Brook Lyndhurst, 2009; BIT, 2018); the belief that one’s household does not produce enough food waste (Allison et al., 2022; BIT, 2018; WRAP, 2021a; Brook Lyndhurst, 2009); the associated financial costs (Allison et al., 2022; Brook Lyndhurst, 2009); lack of space for food waste sorting equipment (Bernstad, 2014; Allison et al., 2022; WRAP, 2021a; Brook Lyndhurst, 2009); odours, pests, associated mess and concerns about hygiene (Allison et al., 2022; WRAP, 2021a; Brook Lyndhurst, 2009; BIT, 2018); forgetfulness if it is not an established habit; and for some people there genuinely

is a lack of awareness, such as not understanding the point of recycling food waste (Allison et al., 2022; BIT, 2018).

1.8.2. Literature review

Barriers such as these can and have been targeted by BI interventions. A systematic review of the literature on nudge techniques targeting food waste behaviour change, found that "there is reliable information on the effectiveness of nudge for food waste recycling interventions" (Barker et al, 2021, p1). Using Google Scholar and the Web of Science database, with key terms searches involving "nudge*" and/or "choice architect*", "food waste", "recycl*", "pro-environmental", "pro environmental", "compost*", our own review identified several promising examples of social norm, timely reminder and "make it easy" nudges that have been used in food waste recycling. Other studies have tested more traditional behaviour change tools (information provision and canvassing). In what follows, we review the literature regarding what has been tried and what works, in the food waste recycling context (summarised in Table 1).

Table 1

Review table summarising main characteristics of studies aimed at encouraging food scraps recycling

Study	Sample characteris-tics	Methods - data collection and analysis	Type of intervention	Outcome variable(s)	Results
[1]	Sweden *n*=465	Field experiment, with pre-post measures and a control group. Regression.	Social norms, disclosure, free sorting bags.	Weight of food waste.	Significant increase compared to baseline and the control group, which persisted up to 8 months post-intervention.
[2]	Norway *n*=176	Quasi-experimental field study, with pre-post measures and a control group.	Social norms, free sorting bags.	Weight-percentage (wt.%), i.e., weight of sorted food waste as a percentage of	Increase from baseline in food waste wt.%; reduced contamination of food waste bags compared to baseline.

		No statistical testing done.		total waste weight; observation of food waste in residual and food waste bags; number of residual bags used, and contaminated food waste and plastic recycling bags.	
[3]	USA *n*=370	Field experiment. Phase 1 pre-post measures for the new bin/service intervention. Phase 2 Randomisation with treatment & control groups for the social norms intervention. Wilcoxon Sign Rank test, Wilcoxon-Mann Whitney rank-sum test.	New bin/service, social norms.	Reported separation of food waste.	Phase 1: Significant increase in reported food waste separation behaviour between pre and post. Phase 2: Significant increase in reported food scrap separation compared to the control group.
[4]	Norway *n*=8,989	Randomised field experiment, pre-post measures and a control group. Regression	Social norms (four variations).	Number of times waste bins (paper, organic, residual) collected; weight for each emptied waste bin (paper, organic, residual waste bins).	All four treatments had a significantly positive effect on the amount of organic waste recycled, compared to a control group.
[5]	UK *n*=9,082	Cluster randomised field experiment, pre-post measures and a control group. Regression.	Social norms.	Participation.	Statistically significant positive effect on food waste participation compared to the control group.
[6]	Sweden *n*=1,632	Case study with pre-post measures. *t*-test.	Written information, installed sorting equipment, free sorting bags	Weight of food waste; source-separation ratio.	Written information had no effect on either outcome measure; installation of kitchen equipment increased source-separation ratio, and had a statistically significant and long-term increase on the amount of food waste.

[7]	Sweden *n*=630	Case study with pre-post measures. *t*-test.	Oral information, written information.	Weight of food waste; waste composition analyses; questionnaire.	The difference between the two treatment groups was not statistically significant.
[8]	UK *n*=110,589	Pilot involving 19 separate projects across 11 local authority areas. No statistical testing done.	Free sorting bags, visual prompts, written Information, oral information, kitchen caddy, communal bin alteration.	Participation; weight of food waste.	Highest increases in participation and weight were associated with a combination that included refuse bin stickers, free liners, info. leaflet.
[9]	Ireland *n*=8,000	Pilot involving 3 areas - each assigned a combination of interventions. No statistical testing done.	Oral information, written information, kitchen caddy, free sorting bags.	Participation; weight of food waste; level of contamination.	All areas saw increases in participation, capture of food waste, decreased contamination; areas that received all four interventions saw the largest increases.
[10]	UK *n*=64,284	Randomised field experiment with control group. Regression.	Visual prompt .	Weight of food waste.	Statistically significant positive increase in average weight of food waste in treatment group compared to control, between pre and post.
[11]	China *n*=~1,300	Randomised field experiment with a control group ANOVA with Tukey Pairwise Comparisons Test.	Visual prompt, volunteer modelling.	Effective capture rate.	Both interventions had a positive statistically significant effect compared to the control group but were not statistically different from each other.
[12]	Canada	Observational study.	Visual prompt, volunteer modelling.	Percentage of ideal composters; percentage of non-composters.	Both interventions were associated with improved 'ideal' composting behaviour.
[13]	China *n*=986	Pre-post measures with no control group, Interviews, and focus groups for qualitative analysis. *t*-test, Mann-Whittney *u*-test	Oral information.	Capture rate of food waste.	Quantitative: statistically significant increase in the recycling capture rate. Qualitative: social norms and emotion are important determinants; prompts minor.
[14]	Netherlands *n*=~8,000	Field experiment with quasi-control group.	Kitchen caddy, written information [Control], kitchen	Average frequency with which households	Significant effects for: kitchen waste separation equipment, reducing distance,

			sorting equipment, reducing distance, goal setting, feedback, influencing attitudes, strengthening social standards, social modelling, acknowledging and reducing resistance, pre-emptive gift, promising reward.	separate their organic waste.	group goal setting & feedback, influencing attitudes, social modelling, pre-emptive gift, promising reward.
[15]	Canada Study 1: *n*=113 Study 2: *n*=1510	Randomised field experiment with no control group.	Reducing distance	Weight of food waste.	Both studies found statistically significant increases in composting for 'the most convenient bin locations compared to all other bin locations.

[1] Linder et al., 2018; [2] Flygansvær et al., 2021; [3] Geislar, 2017; [4] Milford et al., 2015; [5] Nomura et al., 2011; [6] Bernstad, 2014; [7] Bernstad et al., 2013; [8] WRAP, 2021b; [9] Composting & Anaerobic Digestion Association of Ireland [CRE], 2019; Shearer et al., 2017; [11] Lin et al., 2016; [12] Sussman et al., 2013; [13] Dai et al., 2015; [14] Langeveld et al., 2020; [15] DiGiacomo et al., 2018.

1.8.3. Evidence that social norms work

The studies in Table 1 provide some evidence that social norms work. According to the schema introduced by Mertens et al. (2022), social norms alter the decision information in the decision-making context by enhancing the personal relevance of the information. Social norms have been used in different ways to improve household food waste collection behaviour. A common way was to give comparative feedback. Two studies found evidence that providing households with descriptive feedback about their own behaviour and how it compares with a relevant referent group, was effective at increasing the frequency of reported food waste separation (Geislar, 2017), and the weight of recycled food waste (Milford et al., 2015). In the latter study, the effect of descriptive social norm feedback conveyed via a letter, was enhanced

when the letter included practical advice on how to sort recyclables effectively.[2] Even providing descriptive normative information about one's own and other's *waste reduction* behaviour had a statistically significant effect on the weight of recycled food waste, compared to a control group (Milford et al., 2015). Other studies found that providing feedback at the group (rather than household) level, was also effective (Nomura et al., 2011, Langeveld et al., 2020). For example, Nomura et al. (2011) found that providing households with injunctive norm messaging alongside descriptive norm feedback about how a household's street compared to other streets, increased participation in the local food waste collection service by 2.6% compared to a control group. They also found evidence for a local norm effect: the street on which a household lived had a greater impact on their food waste recycling, than the area in which they lived. The implications of these findings are that comparative feedback is effective, especially when using a local referent group that people identify closely with.

Studies have also tested and found support for the effect of messaging that combines injunctive and descriptive norms, but does not provide personalised feedback (Flygansvær et al., 2021, Linder et al., 2018). For example, a Swedish study found that an information leaflet which used descriptive and injunctive social norms appeals alongside disclosure, to increase separation of food waste, was effective (Linder et al., 2018).[3] The study, conducted over a period of almost two years, found a statistically significant effect of the leaflet on the weight of separately collected food waste. Compared to the control group, the treatment group deposited 13 kg more food waste on average, per communal food waste bin. However, in another study, an intervention that used descriptive and injunctive social norm messaging printed on compostable food waste bags, was not found to be effective (Langeveld et al., 2020). Studies have also evaluated the effects of social norms conveyed through social

[2] This study had broader aims than simply increasing food waste weight. It included food waste as part of a wider study testing the effect of personalised feedback and social norms comparisons on all forms of household recycling as well as residual waste reduction.

[3] They also provided households with two free compostable bags to help get started, however the primary intervention was the social norms leaflet.

modelling. This type of intervention is based on the expectation that people are unconsciously motivated to separate their waste if they are shown how, and made to feel that separating waste is normal – in other words, when they are given a good example. Social modelling was found to be an effective tool for increasing the frequency of correct food waste disposal behaviour amongst cafeteria patrons (Sussman et al., 2013) and households' depositing food waste (Langeveld et al., 2020), and for improving effective capture rates,[4] and reducing contamination of collected food waste (Lin et al., 2016), when compared to a control group.

There is evidence supporting the efficacy of social norm messaging at increasing performance of both high- and low-performing households (Milford et al., 2015) and streets (Nomura et al., 2011) at baseline, although there is stronger evidence for the impact of this type of intervention on the behaviour of those below the norm (Milford et al., 2015; Langeveld et al., 2020; Mertens & Schultz, 2020). One study found that although both high- and low-performing streets at baseline improved their participation after receiving a social norms intervention, individual baseline high-performing households living on low-performing streets were more likely to stop or decrease their recycling, when their street was given feedback alerting them that they were below the norm (Nomura et al., 2011). This provides evidence that street-level feedback can cause a boomerang effect among individual high-performers.

While not all studies tested for, or found, evidence for sustained effects, several studies did (Linder et al., 2018, Milford et al., 2015, Langeveld et al., 2020). For example, a large-scale study involving a series of separate field experiments in the Netherlands tested a variety of different BI interventions to increase the frequency of households' depositing food waste, three of which appealed to social norms. They found that two of the three social norm-based

[4] Effective capture rate is a measure defined as βCR. CR = 100 × Food Waste[FW] (in recycling bins) / (total FW in all waste). β = the proportion of non-FW - contamination level)/(proportion of non-FW), where (proportion of non-FW) =
[non-FW/(non-FW + FW)] all bins, and contamination level = the percentage of non-FW in the FW recycling bin.

interventions (“setting group goals and feedback” and “social modelling”) produced statistically significant increases in the frequency of households’ recycling their food waste, and that the effect of “setting group goals and feedback” persisted after three months (Langeveld et al., 2020). It is also likely that social norm messaging is more effective when reiterated over time. Nomura et al. (2011) found that it was only after a second feedback postcard was received that a statistically significant effect on participation was seen, and Langeveld et al. (2020) found that the effect of social modelling became stronger when the intervention had been delivered multiple times.

1.8.4. Evidence that visual prompts work

There is also evidence that sticker (Shearer et al., 2017; WRAP, 2021b), sign (Sussman et al., 2013), and bin cover (Lin et al., 2016) visual prompts are effective at improving food waste recycling behaviour. Proscriptive stickers attached to a household’s refuse bin can improve household food waste recycling. A large randomised controlled trial (RCT) in the UK, involving over 64,000 households, found that a proscriptive sticker prompt discouraging food waste in refuse bins and encouraging use of food waste bins, resulted in a statistically significant increase of 20.74% in average weight of food waste, compared to a control group. The effect also persisted longer term (Shearer et al., 2017). Also in the UK, WRAP ran a large, but non-peer reviewed, research study where they evaluated the effectiveness of a number of intervention-combinations, at improving food waste recycling behaviour. The findings showed that the greatest percentage increases in participation and weight of collected food waste were seen in areas where proscriptive refuse bin stickers were used. Food waste caddy stickers, on the other hand, were associated with poorer outcomes compared to most other measures (WRAP, 2021b). The difference in outcomes due to sticker placement (refuse versus food waste caddy) and messaging (“don’t put food waste in refuse” versus “do put these things in your food waste bin”) is interesting; only the former was associated with significant increases in participation and tonnage. This could be because placing stickers on the refuse bin warning people *not* to place food in the bin, acts as a prompt or reminder at the point-of-decision about

what to dispose of in the refuse bin, whereas a sticker on the caddy will not be seen if the caddy is not being used to begin with. Covering communal food waste bins in housing units with attractive, colourful covers also proved effective, resulting in a statistically significant increase of 32% in the effective capture rate of food waste (Lin et al., 2016).

1.8.5. Evidence that increasing ease works

Increasing ease can be an effective way to improve food waste recycling behaviour, and evidence supports a number of different ways that this can be done. These examples alter how the options are structured in the decision-making environment. People utilise this information to guide their actions. One way of doing this is to reduce the distance residents have to walk to the nearest food waste collection point. Studies found that this had a positive, statistically significant effect on the weight (DiGiacomo et al., 2018) and frequency of households' depositing (Langeveld et al., 2020) food waste. Interestingly, increasing the distance to the nearest residual waste collection point was just as effective (Langeveld et al., 2020). Another method is providing food waste sorting containers and/or equipment for kitchens, to increase convenience. Installation of equipment on the inside of kitchen cupboard doors to hold compostable food waste bags, increased capture and reduced contamination of food waste compared to baseline (Bernstad, 2014),[5] while space-saving kitchen caddies for small kitchens were associated with increased tonnage (WRAP, 2021b).[6] Providing kitchen caddies and/or large separation bins for the different waste streams also resulted in improvements in food waste outcome measures in the Netherlands (Langeveld et al., 2020) and Ireland[7] (CRE, 2019). However, an intervention that used built-in waste separation bins had no effect (Langeveld et al., 2020).

[5] They also provided households with free compostable bags, however the primary intervention seems to be the installation of the kitchen equipment to facilitate food waste sorting in small kitchens.
[6] In the WRAP research study, no interventions were trialled alone, so it is not possible to distinguish the effects of specific interventions. However, the kitchen caddies were included as part of an intervention combination associated with a 35% increase in tonnage, compared to baseline.
[7] No interventions were trialled alone in this study, making it impossible to distinguish the effects of specific interventions. However, the kitchen caddies were included as part of intervention

A number of studies provide evidence that free bin liners to increase ease when managing food waste, can be effective at improving outcomes. Free caddy/bin liners can help reduce the financial, psychological and physical burden of sorting food waste. Free bin liners have been part of intervention-combinations associated with increases in participation and/or weight, and decreased contamination of collected food waste (WRAP, 2021b; CRE, 2019; Flygansvær et al., 2021). The large WRAP study in the UK concluded free caddy liners should be essential parts of a package targeted at raising participation (WRAP, 2021b), although bin liners were part of intervention-combinations associated with a range of outcomes spanning the highest to the lowest increases in tonnages and participation. Finally, free caddy-plus-liner-combinations were found to be more effective at increasing participation and improving quantity and quality of collected food waste than canvassing and an information leaflet alone (CRE, 2019).

1.8.6 Evidence for other types of intervention

Other types of interventions to improve household food waste capture have also been tried, with mixed success. In a Chinese study which tested the efficacy of canvassing[8] in isolation, it resulted in a statistically significant increase of 12.5% in the capture of household food waste, from pre-intervention (Dai et al., 2015). In another study, canvassing was included as part of a package of interventions that was found to be an effective way to increase participation and decrease contamination (CRE, 2019). However, WRAP (2021b) found that canvassing was not associated with notable increases in tonnage and may be associated with decreases in participation.[9]

combinations associated with a doubling of participation and capture of food waste, compared to a control area.

[8] Also known as “door-stepping”.

[9] As no statistical testing was carried out, it is difficult to draw any firm conclusions. However, descriptive statistics show that participation decreases in intervention combinations of which canvassing is a part. There are small (4%−5%) increases in tonnage in these same combinations.

Written information can take many different forms. Focusing on environmental benefits was not effective at improving household food waste capture in one study (Bernstad, 2014), and not statistically different from a written information intervention in another study (Bernstad et al., 2013),[10] but focusing on *how* to use the food waste collection may be effective (Milford et al., 2015; CRE, 2019; WRAP, 2021b). Langeveld et al. (2020) used letters explaining what happens to separated food waste and emphasising specific useful products it can produce. This intervention aimed to stimulate a positive attitude towards waste separation, and resulted in a 23% increase in the frequency of depositing organic waste, compared to a control group, which was sustained at three months post-intervention. These studies provide evidence that the content of the message is the most important factor for written information interventions. This is probably also true for the content included in canvassing scripts. Since canvassing is neither well defined nor consistent across studies, Dai et al. (2015) investigated which elements of canvassing have an effect on behaviour change and which are not important. They found that of eleven clusters of potential behaviour-change determinants, only social norms and emotion were important influencers, while prompts played a minor role. Focusing on knowledge of the collection service, or environmental consequences (that one's actions make a difference) played no role.

Setting group goals and receiving feedback was found to be highly effective in a Dutch study, with both immediate and sustained increases in food waste recycling frequency (Langeveld et al., 2020), however the same study found that setting personal goals was not effective. Interventions that offered rewards and pre-emptive gifts were also trialled and resulted in 15%–16% increases in food waste depositing frequency, but these effects were not sustained over time (Langeveld et al., 2020). The former aimed to activate feelings of reciprocity in the gift-recipient while the latter acted as an extrinsic motivation (incentive). It is unsurprising that

[10] There was no control group and no comparison with baseline measurements of household food waste capture in this study, hence no conclusions can be drawn regarding overall efficacy of either intervention.

effects of the promising reward were not sustained longer term. Short-term effectiveness is commonly observed in interventions that involve rewards, as people tend to link their behaviour to the external motivation of receiving a reward rather than to their own values. Once the rewards are removed, people tend to exhibit less of the desired behaviour (Langeveld et al., 2020).

1.8.7. Limitations of prior work

Although the insights from these studies are useful in highlighting what may work in the context of the current study, there are a number of limitations of this prior work. Many studies involved households with communal waste collection facilities (Lin et al., 2016; Linder et al., 2018; Dai et al., 2015; Flygansvær et al., 2021; Bernstad, 2014; Bernstad et al., 2013; Langeveld et al., 2020; DiGiacomo et al., 2018) which is very different to the context in which the current study is set (individual household kerbside collection). Where the aim is to increase household participation (an individual level measure), as is the case for the current study, findings from studies that measure communal waste behaviour may not generalise. One study, which used average weight per collection round (Shearer et al., 2017) noted that because it was not possible for them to measure food waste weight at the individual household level, it is unclear to what extent the effect of the intervention had on new participants to the scheme compared to existing users who were reminded to use the collection service more often.

In some studies there was ambiguity in study design, making it unclear whether multiple interventions were applied simultaneously or at different times (Bernstad, 2014) and how randomisation took place (Milford et al., 2015), for example, whether it was an RCT or cluster randomised trial – the latter would have been more appropriate due to the possibility of *contamination* (that is, the unintended sharing of treatments caused by participants in different treatment groups interacting with one other [Cotterill et al., 2009]). Control groups were not used in several studies, therefore we cannot identify whether the effect of the intervention(s) was due to other factors (Bernstad, 2014; Bernstad et al., 2013; WRAP, 2021b; CRE, 2019;

Dai et al., 2015; DiGiacomo et al., 2018), while one used an intervention given to all households, as a quasi-control (Langeveld et al., 2020). Some studies lacked true experimental design (Flygansvær et al., 2021; Bernstad, 2014; Bernstad et al., 2013; WRAP, 2021b; CRE, 2019). The WRAP (2021b) pilots, for example, had several important methodological limitations. They lacked randomisation and controls. Methodologies for data analysis also varied between the local authorities, hence results could not be directly compared between local authorities. Due to small sample sizes of the unit of statistical analysis (the "collection round" weights of food waste) in each pilot, statistical analysis of the results could not be carried out. Ultimately, we cannot draw conclusions about which set of interventions were effective, due to the non-experimental nature of the study and the results of the pilots should be interpreted with caution (Shearer et al., 2017). Randomisation was also lacking in Bernstad (2014), Bernstad et al., (2013), Dai et al. (2015). Other times, convenient characteristics of location were used to assign treatments to groups (Flygansvær et al., 2021; Linder et al., 2018) or areas (CRE, 2019). In three studies, statistical analysis of the results was not carried out (WRAP, 2021b; CRE, 2019; Flygansvær et al., 2021), meaning claims about the findings are not robust. In a number of studies, multiple interventions were applied to the same participants, making it impossible to isolate the effect of each, on the outcome(s) (Flygansvær et al., 2021; Linder et al., 2018; WRAP, 2021b; CRE, 2019; Bernstad, 2014). These limitations related to study design raise questions about whether other variables were confounding the reported results, whether causality can be established, and to what extent we can conclude that specific interventions are effective for increasing capture of household food waste.

One study recruited participants via mailed out surveys, making non-response bias an issue. It also used self-reported food scrap separation behaviour rather than observed behaviour as the outcome measure (Geislar, 2017). This is problematic because a number of studies have found that many people exaggerate their recycling behaviour, perhaps because they know that this is the "right" behaviour and feel implicit pressure to give the "correct answer" when

asked (Timlett & Williams, 2008). In multiple studies, households involved did not have existing kitchen food waste sorting equipment prior to the study period. This makes it difficult to know whether increases seen in the respective outcome variables were due to the introduction of the new system, or to the other interventions being trialled (Bernstad et al., 2013; Flygansvær et al., 2021; CRE, 2019; Geislar, 2017). Some studies covered in the literature review used relatively small sample sizes (<500). Finally, there are relatively few studies evaluating attempts to improve food waste recycling behaviour specifically, fewer that have tested interventions for a service similar to the context in which the current study takes place (individual household kerbside collection) and all of the prior studies were carried out in the Northern Hemisphere, whereas the current study setting is New Zealand.

This short review has revealed that each of the intervention types identified in 1.8.3 – 1.8.6 has some support. It has also shown that these studies have limitations, and we have no prior evidence of what works in New Zealand, meaning we cannot say with confidence what the best approach for our context is. There is, therefore, a clear need for work in a New Zealand context.

1.9. The current study

The current study is set in Auckland, New Zealand, a city of 1.72 million inhabitants. The overarching aim of the current study is to develop, test and evaluate behavioural interventions to increase use of and participation in the existing kerbside food scraps collection service in parts of Auckland. Being a developed, industrialised nation, New Zealand has a large food waste footprint: recent estimates indicate that around 157,300 tonnes of avoidable household food waste is disposed of through domestic kerbside refuse collections annually (Sunshine Yates Consulting [SYC], 2018), which equates to around 60 kg of household food waste per capita (SYC, 2018; UNEP, 2021) and costs consumers $1.17 billion per annum, (around $644 per average New Zealand household; SYC, 2018). In terms of environmental impact, avoidable food waste in New Zealand is responsible for between 325,000 and 410,000 tonnes

of CO_2 equivalent emissions annually (LoveFoodHateWaste, n.d.; Reynolds, et al., 2016, p.8), which is equivalent to that produced by over 150,000 cars per year. In Auckland specifically, it was estimated that around 3.3% of total greenhouse gas emissions came from landfill waste in 2015 (Auckland Council, 2018). Given that the average household refuse bin in Auckland contains around 45% food waste, there is huge scope for a well-functioning kerbside food scraps collection service to have an impact on the reduction of greenhouse gas emissions. As part of Auckland Council's Zero Waste vision that "Auckland aspires to be Zero Waste by 2040, taking care of people and the environment and turning waste into resources" (Auckland Council, 2018, p.8), Auckland Council plans to roll out a kerbside food scraps collection service, in stages, to all of mainland urban Auckland in 2023. This will be the largest roll-out in the Southern Hemisphere and the anchor project for Auckland Council achieving its waste reduction target for residential waste to landfill (from 110 kg/cap/annum to 88 kg/cap/annum) by 2028 (Auckland Council, 2018). In preparation for this roll-out, Auckland Council ran a food scraps collection trial on Auckland's North Shore from 2014–2019 as well as piloting a collection service in Papakura, South Auckland, which commenced in March 2018. When the North Shore trial officially ended, provision of the service to existing households remained, and continues to the present, charged at a targeted rate.

Auckland Council's experience from the roll-out of the North Shore trial and Papakura pilot food scraps collections, shows a trend of declining usage since 2018. According to a participation survey of Papakura kerbside rubbish and food scrap collections in January 2021, participation in the collection service in Papakura was only 35.2%, down from 49% in 2018 (personal communication, June 8, 2022), despite over 90% awareness of the service being reported in surveys (Auckland Council, personal communication, June 8, 2022). A participation rate of 49% is average according to WRAP UK, with poor being less than 35% and good being above 55% (WRAP, 2021c). There is clear evidence of the intention-behaviour gap affecting households participating in Auckland's food scraps collection: self-report surveys on participation repeatedly show rates of participation around the 60% mark (Auckland Council,

personal communication, June 8, 2022) – significantly higher than the figures that observational participation surveys reveal. In addition to the direct benefits of reducing waste and emissions, there is a real concern that there needs to be enough food scraps collected to make the AD processing viable (K. Buller, Community WasteWise Manager at Auckland Council, personal communication, 31 October, 2022). This, along with the goal of meeting Auckland's waste reduction target for residential waste to landfill, is a pressing reason why finding solutions to increase participation is so important. Surveys have highlighted that many of the same barriers to food scraps recycling that are found overseas, are also applicable in a New Zealand context (Auckland Council, personal communication, June 8, 2022). The most prominent amongst these include, odours, inconvenience, a belief that the household does not produce enough food waste, and the cost of bin liners.

Auckland Council has tried several initiatives to increase food scraps recycling participation. They launched an engagement campaign in Papakura in 2019–2020 aimed at increasing participation in the food scraps program from 49% to 55% by December 2020. The campaign involved public place engagement at local events, partnerships with stakeholders and potential influencers, and different bin designs. Due to the Covid-19 pandemic, only four out of eight planned public engagement events were carried out, and attendance was low. As part of the engagement campaign, a trial was carried out involving 1,200 households across different participation areas (low, medium, high prior participation). Three combinations of interventions were trialled, including a refuse bin sticker, a brochure with information, and canvassing by a WasteWise advisor. Unfortunately, there were problems with data collection which meant that the results were not valid.

In 2021, a refresh campaign was launched in response to further-declining participation in the food scraps service in Papakura. The campaign involved various elements, including sending residents communications materials such as testimonials and endorsements, buy-one-get-one-free vouchers for caddy liners, and brochures with practical tips on how to use the service.

The campaign also advertised through various channels, and there were four key messages of the campaign: celebrating use of the service, any amount of food scraps makes a difference, unusual items that can be recycled, and saving money by reusing old newspapers as bin liners. The campaign saw positive results in terms of bin requests and a slight increase in tonnage but did not result in a significant increase in participation (Auckland Council, personal communication, 10 January, 2022).

As discussed in Section 1.8, there are a limited number of studies specifically evaluating attempts to improve household food waste recycling behaviour, and fewer still, that have tested interventions for food waste collection services similar to Auckland's (a regular kerbside collection where each household has its own food scraps bin). There are, to our knowledge, no published studies that have been done in New Zealand on this specific topic. The Dutch study discussed in Section 1.8 emphasised that "the intervention(s) that are best suited to a specific area depends on local circumstances, such as the attitude of residents…'The devil is in the detail'" (Langeveld et al., 2020). This study will therefore be valid for Auckland's unique context. Our study will benefit Auckland Council as it will be the first time a rigorous experimental design has been applied to evaluate initiatives to increase the use of the food scraps service; it will allow the Council to systematically compare a range of behaviour change tools under controlled conditions. This study will therefore contribute to new knowledge by confirming whether behavioural interventions that have been successful in other countries, are also successful in a New Zealand context. Moreover, if successful, they have the potential to become useful tools that can be scaled up and implemented by Auckland Council, as well as other local authorities in New Zealand who already have kerbside food scraps collections and are facing similar problems with low participation, or who will be implementing their own food scraps collections in the near future. The latter is likely, since the Ministry for the Environment is proposing that all urban households in New Zealand be provided with a food scraps collection service (Ministry for the Environment [MFE], 2022b) to increase the amount of

organic waste diverted from landfill, as part of the New Zealand Government's Emissions Reduction Plan (MFE, 2022a).

The study will also be of interest beyond Auckland, New Zealand. We aim to address the limitations of prior work, outlined in Section 1.8 in the following ways: (a) by running a randomised controlled field experiment we will be able to determine if any of our interventions have a causal effect on participation – many of the prior studies did not, or were unable to, apply scientifically rigorous experimental design; (b) we have chosen a cluster randomised design for the experiment, to avoid the (otherwise) high possibility of contamination – for example if households receiving one intervention talk about it with their neighbours in the control group; (c) by measuring the participation of individual households, we can infer whether any of the interventions cause *new users* to participate in the service; (d) we are using relatively large sample sizes, to minimise the risk of incorrectly rejecting true hypotheses (getting a false negative); (e) where possible, care has been taken to keep interventions separate in order to isolate any potential effects of a given intervention on behaviour; (f) by *observing* food scraps bins we are using an objective measure of participation as opposed to self-report, which can be unreliable; (g) we are conducting a second post-intervention follow-up, seven weeks after the intervention delivery, to examine whether behaviour change is sustained; (h) our samples have been selected in an objective manner, as far as practical considerations allow, and demographically, they are approximately representative of the diversity of the population of Auckland; and (i) our study has deliberately excluded multi-unit dwellings (MUDs) since it is difficult to measure individual household participation behaviour when MUDs are included. It focuses on individual household waste behaviour, meaning that any findings will be applicable to the majority of Auckland households, which have (or will have) individual household waste collection services.

In light of the relevant literature reviewed in this chapter and Auckland's unique context, the current study will test four interventions. Our aims are to implement and evaluate three BI-

informed interventions aimed at maximising participation in the existing kerbside food scraps collection service in Auckland – a visual prompt in the form of a sticker, placed on the lid of each household's refuse bin (Papakura trial only); a postcard, using descriptive and injunctive social norms messaging around food scraps recycling behaviour; the provision of free food scraps caddy liners; and household canvassing by Auckland Council Waste Solutions WasteWise advisors. The latter intervention is being compared to the other three as it is Auckland Council's most commonly used behaviour-change tool, and they would like to test its efficacy in an experimental setting. The outcomes being measured in the study are globally recognised measures of participation and set out (WRAP, 2010). The interventions will be tested across two separate cluster randomised trials – in Auckland's North Shore, and in Papakura, South Auckland. The Papakura trial will test four interventions and will involve approximately 2,500 households while the North Shore trial will test three interventions and will involve approximately 1,500 households due to constraints on the delivery of treatments and sample size.

For transparency, we pre-registered the study on the Open Science Framework prior to data collection and included an analysis plan which outlined the type of analysis that we intended https://osf.io/dvwep. The hypotheses we pre-registered were:

(1) In Papakura, participation in the food scraps collection service will show a greater increase post-intervention (relative to pre-intervention) in the bin liners (H1a), postcard (H1b), canvassing (H1c) and sticker prompt (H1d) conditions than in the control condition.

(2) In Papakura, set out in the food scraps collection service will show a greater increase post-intervention (relative to pre-intervention) in the bin liners (H2a), postcard (H2b), canvassing (H2c) and sticker prompt (H2d) conditions than in the control condition.

(3) In the North Shore, participation in the food scraps collection service will be greater in the post-intervention period than in the baseline period (H3a), and will be greater

in the canvassing plus bin liners (H3b) and postcard plus bin liners (H3c) condition than in the bin liners-only condition.

(4) In the North Shore, set out in the food scraps collection service will be greater in the post-intervention period than in the baseline period (H4a), and will be greater in the canvassing plus bin liners (H4b) and postcard plus bin liners (H4c) condition than in the bin liners-only condition.

2. Experimental Design and Methods

2.1. Study setting

To evaluate the effect of the various interventions being trialled, we designed two separate field experiments for the two areas that receive Auckland Council's food scraps service. The two field experiments were run simultaneously. We chose to run two concurrent field experiments since the settings are home to relatively different populations. The setting for one was Auckland's North Shore, approximately 10 km from the CBD, where just over 2,000 households on selected streets in the suburbs of Takapuna, Milford, and Northcote, receive the service. As of June 2021, the total population of these three suburbs was estimated to be around 20,000 (Statistics New Zealand [Stats NZ], n.d.-a), however only a subset of streets in each area receive the food scraps service. The Auckland Council food scraps service trial was introduced in these selected North Shore streets in May 2014. The streets were chosen to be part of the trial as they were, at the time, roughly demographically representative of Auckland's population (B. Osbourne, Senior Statistical Analyst at Auckland Council, personal communication through email, July 22, 2022). Although the 2014 trial was initially only intended to last 14 weeks, its popularity among residents led to it being extended. The trial officially ended in June 2019, however the service remains in place, paid for by residents at a targeted rate. The demographic makeup of households in the North Shore field experiment varies significantly depending on the suburb and is shown in Table 2 (see also Figure A1 in Appendix F).

Table 2

Background statistics for suburbs included in North Shore and Papakura Trials

Suburb	Ethnic group						Born in NZ	Average NZDep (2018) score**
	European descent	Māori	Pacific	Asian	MELAA	Other		
Takapuna*	82.8%	4.1%	1.1%	13.8%	3.4%	0.8%	63.8%	1.3
Milford*	55.5%	3.3%	2.1%	41.5%	2.0%	1.2%	48.2%	5.5
Northcote*	38.4%	14.4%	21.2%	32.6%	4.3%	1.0%	52.9%	8.2
Red Hill	50.6%	38.8%	25.1%	8.4%	1.1%	0.8%	78.5%	7.5
Ōpaheke	71.0%	24.5%	9.6%	11.3%	1.9%	0.9%	76.8%	3.8
Papakura Central	56.1%	26.8%	14.5%	17.3%	1.8%	0.7%	73.4%	9.1
Rosehill	47.8%	31.0%	17.1%	22.3%	1.3%	0.8%	71.9%	4.6
Pahurehure	70.2%	16.3%	9.3%	17.4%	1.8%	1.4%	74.3%	3.8
Hingaia	66.9%	8.2%	3.8%	27.2%	1.0%	1.4%	67.9%	1.1
Karaka Lakes	49.9%	6.9%	4.3%	45.3%	2.2%	1.4%	52.4%	1.4

Note:

* The data for these suburbs comes from Stats NZ's Statistical Area 2 (SA2) data. SA2 is an output geography defined by Stats NZ, that comprises between 1000–4000 residents. The suburb of Takapuna falls in Takapuna Central SA2, Milford in Westlake SA2, and Northcote in Northcote Central SA2. "Suburb" is a term that is not clearly defined; while suburbs and SA2 areas are sometimes identical, this is not always the case.

** The New Zealand Index of Deprivation for 2018 (NZDep[2018]) is used to measure socio-economic deprivation. It is displayed as deciles, from 1 (most deprived areas) to 10 (least deprived areas). The Deprivation Index values in Table 2 are the average values for the study households, across SA2 areas.

Takapuna is a relatively affluent suburb of mostly European descent (Stats NZ, n.d.-b). Just under two-thirds of residents are New Zealand born. Milford is less affluent and has a large Asian minority. Of the three North Shore suburbs it has the lowest proportion of residents who are New Zealand born (Stats NZ, n.d.-c). Northcote is the most deprived of the three North Shore suburbs. It is a rapidly changing area, characterised by a mixture of government-provided housing and private ownership. It is the most ethnically diverse of the three North Shore areas in our study, with a large Māori and Pacific Island population and just over half of residents born in New Zealand (Stats NZ, n.d.-d). Between the three suburbs there is a wide range of ethnic and socio-economic diversity. One measure of socio-economic deprivation in New Zealand is the New Zealand Index of Deprivation (NZDep) for 2018. NZDep(2018) is displayed as deciles, where deciles 1–2 represent areas with the least deprived score while deciles 9–10 represent areas with the most deprived score.

The setting for the other field experiment was the South Auckland Local Board of Papakura, approximately 35 km from the CBD, and which encompasses households across seven suburbs: Hingaia, Karaka Lakes, Pahurehure, Papakura Central, Rosehill, Ōpaheke, and Redhill. There are a total of 19,743 households that receive the Papakura food scraps service, but our sample includes just under 2,500 of these. Papakura has received the food scraps service since it was introduced in 2018 as a pilot. As with the North Shore, the sample is demographically diverse, and is shown in Table 2 (also Figure A2 in Appendix F). The sample includes a range of ethnic diversity. Pahurehure, Ōpaheke and Hingaia are predominantly European, Karaka Lakes has a significant Asian minority, while the largest groupings of Māori and Pacific Islanders are found in Rosehill, Papakura Central and Red Hill - the latter being almost two-thirds Māori and Pacific Island. In all suburbs, a majority of residents are New Zealand born, however the proportion varies, and ranges from 52%–79%. There is also a range of socio-economic diversity, with households from deciles 1–10.

2.2. Design

We used a repeated-measures cluster randomised design, stratifying by level of deprivation, with a baseline monitoring period and two post-intervention monitoring periods, where we recorded bins that were set out by individual households. The North Shore and Papakura trials ran simultaneously for a period of 16 weeks. They began with a baseline monitoring period that ran for three weeks from August 1–19, 2022. This was immediately followed by the intervention delivery phase which lasted for three weeks. Immediately following this, the first post-intervention monitoring period ran for three weeks from 12–30 September. There was then a gap of four weeks which included the October school holidays, where no data collection took place. The second post-intervention monitoring period began on 31 October and ended on 18 November; this three-week monitoring period took place seven weeks after the end of the intervention-delivery period and its purpose was to test if there was a sustained effect of the interventions. Recycling tends to increase immediately post-intervention, but then decline over time. To assess if the behaviour-change has become a habit that is maintained, it is advisable to take follow-up measurements after a period of time, which takes into account the decline in recycling and determines if the habit has been established and sustained (Cotterill et al., 2009). We designed the study timeline to avoid any study activity – food scraps bin monitoring or delivery of interventions – that would coincide with school holidays or public holidays, given that holidays do not represent typical household waste behaviour.

Papakura food scraps collection days are Monday through to Friday, however for practical and logistical reasons we chose to select the streets for our sample from those that have Monday, Thursday and Friday collection days. From each of the three collection days, a sub-sample of streets was chosen. These sub-sampled streets tended to be in contiguous areas and were chosen to ensure we ended up with a demographically diverse sample. North Shore food scraps collection days are Monday, Tuesday, Wednesday; because all houses who are provided with the food scraps collection service (who also met our inclusion criteria) were included in the North Shore trial, all collection days were included. We chose to use a cluster

design, where households were grouped into *street clusters*, which were either streets, or similar sized, usually contiguous groupings of households over which treatment conditions were clustered. We clustered households in this way to limit the possibility of contamination. This could occur when, for example, neighbours receiving the canvassing intervention discuss this with a neighbour receiving one of the other intervention types, or with a neighbour in the control group, such that a treatment could end up inadvertently influencing the food scraps participation behaviour of households assigned to a different condition. Where single streets were required to be split, as far as possible this was done in one of two ways to minimise potential contamination: (1) all odd numbers (on one side of the street) were clustered into a street cluster while all even numbers (on the other side of the street) were clustered into a different street cluster; (2) the street was divided in half – a hypothetical line was drawn halfway down the street and all odd and even numbers on the left side of the line were clustered together, while all odd and even numbers on the right side of the line were assigned to a different cluster.

The Papakura trial had four treatment conditions and a control condition that received no intervention, while the North Shore trial had three treatment conditions: bin liners plus canvassing, bin liners plus a postcard, and bin liners-only; the latter acted as a quasi-control condition by which we could compare the effects of the other two treatment combinations. There was no true control condition in the North Shore for practical reasons; due to our study coinciding with the roll-out of a new type of food scraps bin liner which was to supersede the existing liners in the area (discussed in Section 2.3 below), it was necessary for all households in the North Shore trial to receive a sample roll of the new bin liners.

This study was approved by UAHPEC (#24598). Households were not aware that they were taking part in a study. This was deemed acceptable on the grounds that we would not be collecting any individual-level data – only data about the food scraps behaviour of households. We sought a waiver of informed consent of individuals living in the households in our study on

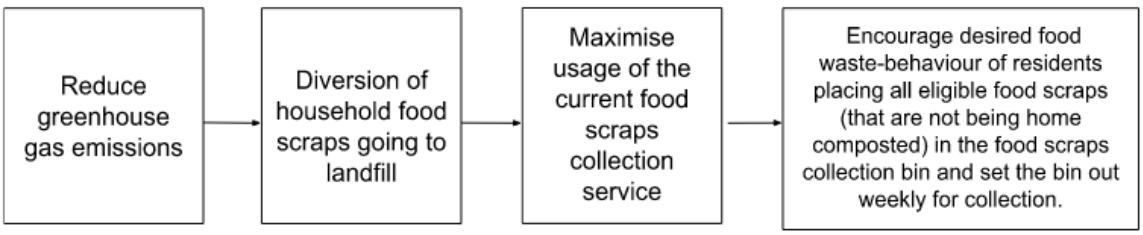
the grounds that the field experiments would involve no more than minimal risk to human subjects and informing individuals would undermine the validity of our findings. This is in line with US Food and Drug Administration guidance around studies of this kind (Food and Drug Administration, 2017). As a further precaution, before analysis of the data, household identifying information (address numbers) was de-identified using a coding system, which ensured that the food waste behaviour of households would remain confidential.

2.3. Designing the interventions

Our aim was to encourage residents in Auckland's food scraps recycling service trial areas to better utilise the food scraps collection service. The study was developed in collaboration with Auckland Council and in support of their broader aim of diverting waste from landfill in order to reduce greenhouse gas emissions. The ultimate goal of the study was therefore the reduction of greenhouse gas emissions. This was broken down into steps leading to a specific behaviour to target: encouraging the desired food waste behaviour, that is, residents placing all eligible food scraps (that are not already being home composted) into their food scraps bin and setting the bin out weekly for kerbside collection. The process is outlined in Figure 1.

Figure 1

Process of Identifying a Specific Behaviour to Target



We began designing solutions by first identifying key barriers to the desired behaviour. This step involved gaining a thorough understanding of the context and identifying behavioural barriers and enablers. For the current study, first an extensive literature review was conducted

to identify the barriers to food waste recycling, and behavioural interventions that have been trialled in the context of food waste recycling and other related pro-environmental behaviours, such as dry kerbside recycling. We also made use of the extensive prior work that had been conducted by Auckland Council, surveying and interviewing residents of Papakura and the North Shore on their experiences with the food scraps collection service, barriers to use, and suggested improvements. In addition, potential barriers were identified in a workshop, involving the University of Auckland researchers and key Auckland Council stakeholders. Many of the barriers that were identified through this workshop closely matched barriers identified in the academic literature review as well as earlier Auckland Council reports from surveys and focus groups involving the target population. This created a rich and broad picture of the context in which we would be working.

The identified barriers fed into the design of the interventions. A collaborative workshop was held involving relevant parties from Auckland Council and the University of Auckland researchers, with the aim of brainstorming intervention ideas. The most promising interventions from the literature review were outlined, and input was sought from workshop members to develop and expand this list. By the close of the workshop, fifteen potential intervention ideas had been suggested, ranging from low-cost nudges to more traditional behaviour change tools. Work was then done amongst the key researchers to narrow down the list to the most promising four ideas, with several back-up options. Of the fifteen potential ideas, a shortlist of five was arrived at. They included: a reminder sticker placed on households' refuse bins; a postcard, leaflet or brochure designed to activate social norms; provision of free liners for the food scraps kitchen caddy; and a new food scraps bin to replace the approximately eight-year-old bins on the North Shore. Household canvassing was also included in the shortlist, despite not having been identified in the literature review, because it is currently Auckland Council's most widely used behaviour change tool in the area of waste solutions. The final set of interventions are outlined below.

2.3.1. Intervention 1 - Postcard with social norms appeal

We developed a postcard encouraging people to use the food scraps service, with messaging that uses both an injunctive and descriptive social norm. Both types of norms were included in order to maximise impact, in line with the recommendations of Cialdini et al. (2003). On the front of the postcard is an animated image of a talking eggshell saying: “Hey! Food isn’t rubbish. Food scraps belong in your food scraps bin”. We borrowed and adapted the image and text from Metro Vancouver’s “Food Scraps Aren’t Garbage” campaign. All intervention material and communications use the terminology “food scraps” or “para kai” as opposed to “food waste”. The choice of wording was a direct result of prior consultation undertaken with Auckland Council’s Māori Responsiveness Unit in the lead up to the implementation of *Te Mahere Whakahaere me te Whakaiti Tukunga Para i Tāmaki Makaurau 2018 – Auckland Waste Management and Minimisation Plan 2018* ([Waste Plan 2018], Auckland Council, 2018). The choice of terminology is based on Māori tikanga that there is no term for or concept of food waste in Te Ao Māori (Auckland Council, 2018). Therefore, in line with tikanga, a more appropriate name is food scraps/para kai. Accordingly, all communications (words and/or images) used in the study did not depict food in a wasted way; rather, that food scraps are a resource. It is for this reason that we chose to use an image of an eggshell (rather than, for example, using a piece of meat, fruit or bread) since eggshells are unavoidable food waste.

A descriptive norm appears near the bottom of the postcard. There is slightly different wording on the North Shore and Papakura postcards. The North Shore and Papakura postcards are found in Appendix A. The descriptive norm message used for the Papakura postcard is: “Join thousands of your neighbours in Papakura who already recycle their food scraps!” This is a true descriptive norm and is based on the most recent participation figures which showed a participation rate of approximately 35% for Papakura. As there are 19,743 households who receive the food scraps service in Papakura, just under 7,000 households regularly use the service. On the North Shore, however, there are only around 2,000 households that receive the food scraps service; therefore we could not use the wording “thousands”. Since “hundreds”

(a true descriptive norm) does not carry the same impact as “thousands”, we opted to use the wording: “Join your neighbours on the Shore – recycle your food scraps!”[11] On the reverse of the postcard we used three key features which are the same on both versions of the postcard. First, we included an image of a seedling, showing a use for the processed food scraps – as compost. Second, an injunctive norm appears at the top of the postcard, in prominent bold lettering and reads: “Most Aucklanders believe that reducing the amount of waste we throw in landfills is important for Auckland. Get on board and use your food scraps bin!” This is a true injunctive norm and is based on findings from the consultation period for the Waste Plan 2018, where Auckland residents were asked what they believe is the most important waste outcome for Auckland. Approximately 65% of people chose “reducing waste to landfill and carbon emissions”. Third, underneath the injunctive norm there is some information about what happens to the food scraps that are collected. It states: “Did you know? Using your food scraps bin means your food scraps get turned into compost that is used to grow more plants and vegetables. Recycling food scraps means less rubbish goes into the landfill.” This links to the injunctive norm, presenting readers with the connection between what most Aucklanders believe is the right behaviour, and how that behaviour can be achieved.

2.3.2. Intervention 2 - Sticker prompt

A second intervention was a sticker prompt, placed on the lid of households’ refuse bins, with proscriptive messaging about using the refuse bin for disposing of food scraps. The general idea for the sticker came from the sticker used in a UK study on food waste recycling (Shearer et al., 2017). Sussman and Gifford (2012) note that sometimes visual prompts are effective, may be less effective than other types of behavioural interventions or may result in the opposite effect than that intended, so care must be made in their design and placement in order to achieve maximum impact. The content and design of the sticker prompt were decided on with reference to the literature on effective visual prompts (see Section 1.7.2). The central

[11] Auckland’s North Shore is colloquially known as “the Shore”.

message of our sticker clearly specifies the undesirable behaviour in large text which takes up just under half of the sticker area. It reads: “No food scraps please”. To the right of this, an easy alternative behaviour is provided, with the message: “Remember to use your food scraps bin”. On the far right there is an image of an Auckland Council food scraps bin with a tick on it, indicating that this is the correct behaviour. The sticker also includes information on how to find out more, by going to Auckland Council’s waste website. The sticker is not cluttered, and the message is simple and unambiguous. The language used is polite and non-threatening.

The choice of location for the sticker was the top of the refuse bin lid. The purpose of the bin sticker is to act as a point-of-decision visual prompt. Since evidence indicates that behavioural compliance is increased when a visual prompt is read in the moments leading up to the opportunity to perform the behaviour in question, point-of-decision visual prompts should be placed in the immediate vicinity of where the behaviour takes place (Sussman & Gifford, 2012). Accordingly, the top of the bin lid was chosen since it is the most salient location. We chose to use a dual-injunctive prompt with a proscriptive main message (“No food scraps please”), accompanied by prescriptive text (“Remember to use your food scraps bin”) and images (a picture of the food scraps bin with a tick on it). A study testing the effectiveness of different variations of single-injunctive versus dual-injunctive and proscriptive versus prescriptive visual prompts, on energy conservation, found that dual-injunctive visual prompts (proscriptive or prescriptive) significantly increased compliance with the target behaviour (switching lights off when leaving an office) compared to single-injunctive prompts (Bergquist & Nilsson, 2016). The colour scheme of the sticker was chosen to be primarily green. Green was chosen as it provides a contrast to the red background of the refuse bin, helping to increase salience. The sticker prompt is found in Appendix B.

2.3.3. Intervention 3 - Free kitchen caddy liners (bin liners)

We also tested the effect of giving households free food scraps caddy liners. Auckland Council accepts approved compostable bin liners only. Their communications materials make this

clear to households. They also encourage people to line their food scraps kitchen caddy and bin with newspaper or paper towels as an alternative to Council-approved liners. Qualitative evidence from surveys, focus groups and interviews of residents in the food scraps service area, that have been previously conducted by Auckland Council, indicates that people's use of the food scraps service is strongly linked to their use of liners; liners are seen to be a necessary part of the food scraps recycling process. In a 2019 survey of Papakura residents, 91% of respondents reported using caddy liners (Auckland Council, personal communication, June 8, 2022). It was therefore decided to trial an intervention providing households with a free roll of kitchen caddy liners.

By chance, our trials coincided with the timeline for a planned campaign to introduce a new type of caddy liner to replace the existing one. This meant that our trials had to accommodate the changeover. Essentially, the two trials involved not merely giving residents in the free bin liners condition a free roll of liners, but also giving these households a new product. The new liners look different and are more affordable. The liners, which were delivered to letterboxes of addresses in the free liner treatment conditions in both trials, were accompanied by an A5 double-sided flyer. We wanted to keep the amount of information in the flyer to a minimum and in particular, to avoid any sort of social norm messaging, or other form of communications that could cause people to increase their participation in the service for reasons other than having received free liners, since we wanted to isolate the effect of *free* liners on our outcome variables. Due to reasons outside of our control, the free liners given to North Shore residents needed to include potentially confounding communications about the liners being more affordable. Directly advertising the affordability of the new liners to these households could incentivise people to better use the food scraps service during the trial period, confounding the effect of free liners (see Appendix F for more information). An image of the liners and accompanying North Shore flyer is found in Appendix C. The Papakura trial flyer is identical to the North Shore flyer with the exception of the wording about affordability and can also be found in Appendix C.

2.3.4. Intervention 4 - Canvassing and commitment

Canvassing and commitment was trialled as a fourth intervention. Auckland Council routinely uses household canvassing as a behaviour-change intervention, including using canvassing to address recycling contamination issues and to encourage participation in the food scraps collection service. However, its efficacy in relation to increasing participation in the food scraps service has never been evaluated in a methodologically rigorous way by Auckland Council.

In both Papakura and North Shore trials, households in the canvassing treatment group were visited by a pair of Auckland Council Waste Solutions WasteWise Advisors (WWAs) who chatted to an adult in the household for approximately five minutes about the food scraps collection service. If residents declined to chat, the WWAs offered them the standard Auckland Council food scraps service brochure. This can be found in Appendix D. At households where no one was home, a calling card was left in the letterbox stating that an Auckland Council WWA had visited. WWAs used questions to engage residents and record their responses, such as whether the resident had heard of the Council's food scraps service.

The canvassing script can be found in Appendix E. The purpose of the canvassing was primarily educational, to ensure that residents know the purpose of the food scraps service and how it works and to provide tips for dealing with common complaints about smell, pests, and hygiene. Residents who engaged with the WWAs had the opportunity to ask questions and give comments or feedback to the WWAs. Canvassers did their best to adapt to the situation, speaking to non-English speakers in their first language where possible, showing a video about the service on iPads, and referring to the Auckland Council food scraps service brochure. Finally, the script includes the opportunity for residents to make a commitment to using the food scraps service (or use it more often) in the future – another BI tool that has shown success in promoting pro-environmental behaviour (Lokhorst et al., 2013).

2.4. Inclusion and exclusion criteria

During the first week of set out monitoring, the monitors also recorded each address that was to be included in the study. This was done by walking the streets that were included and recording individual addresses that met our inclusion criteria (as well as recording whether the address had set out a food scraps bin). The following dwellings were excluded from our sample: multi-unit dwellings (MUDs), for example, apartment buildings; houses on shared driveways where more than four houses share a driveway; businesses, schools, commercial venues and any dwelling that is not a residential dwelling; houses under construction where it is obvious that the house is unoccupied; houses where it is known that the house will be demolished during the study period; and any other house where it is clear that the house is unoccupied or was consistently unoccupied during the course of the study. We also excluded 37 addresses of newly built houses in Northcote in our North Shore trial area, where it was known that the household did not and had never had a food scraps bin.

The reasoning behind the rule of excluding households with more than four houses sharing a driveway, was practical: because some existing food scraps bins no longer have an address sticker affixed and have no other means of identification, this makes it difficult to identify with certainty, that a specific food scraps bin belongs to a specific house when there are a large number of refuse, recycling and food scraps bins sharing berm space. By restricting our inclusion number to four houses or less per shared driveway, it was simpler. MUDs were excluded for the same reason. Houses with four or less properties sharing a driveway were included since there are a large number of households that fit into this category in both of our trial areas, and we needed to ensure that our sample sizes were large enough. Including these types of houses did pose a problem though. Where a food scraps bin belonging to one of the houses down a shared driveway was missing an address sticker and was adjacent to other bins (refuse and recycling) belonging to households that share the same driveway, it is difficult to unambiguously assign the food scraps bin to a specific address. This is a necessary part of measuring participation – one of our outcome measures. We resolved this problem in the

following way: for driveways with multiple bins that could not be unambiguously assigned to an address, unassigned bins were marked with a letter code, allowing us to track whether the bin was set out in subsequent weeks without having to know the precise address to which the bin belonged. At the end of the study there were six unidentified bins in the Papakura trial and 10 in the North Shore trial.

2.5. Sample size and randomisation

The North Shore trial involved a sample of 1,513 households clustered into 27 street clusters spread across the three North Shore suburbs of Northcote, Milford, and Takapuna. The sample size gives us at least 500 households per condition. The sample size was determined by conducting an a priori power analysis to give us a figure that would be sufficient to detect even small effect sizes (d<0.2). This sample included all of the households that receive the Council's food scraps collection service and who also met our inclusion criteria. Households were first stratified into four socio-economic sub-groups, based on their NZDep(2018) score. This resulted in four different sized sub-groups: red (decile 9-10, most deprived), orange (decile 7–8), yellow (decile 5–6), green (deciles 1–4, least deprived). Within each sub-group, households were then clustered into similarly sized street clusters; each sub-group ended up with either six or nine clusters. Clusters within each sub-group were then randomly assigned to one of the three treatment conditions – bin liners-only; bin liners plus postcard; bin liners plus canvassing – ensuring the three different treatments were approximately evenly distributed across the different socio-economic sub-groups (see Table A1 in Appendix F for Deprivation Index score across conditions). Final sample sizes in each condition were: n=530 (bin liners-only), n=499 (bin liners + postcard), n=484 (bin liners + canvassing).

The Papakura trial involved a sample size of 2,459 households clustered into 50 street clusters. With five conditions, this gave us approximately 500 per condition. We had targeted a sample of at least 400 per condition to be large enough to detect even small effect sizes (d<0.2). Households were first stratified into seven sub-groups. These were based on their

collection days and/or geographic proximity which in turn, is associated with level of deprivation. Within each of the seven sub-groups, households were then clustered into similarly sized street clusters; each sub-groups ended up with either five or ten clusters. Clusters within a given sub-group were randomly assigned one of the five treatment conditions – sticker prompt; postcard; bin liners; canvassing; no-treatment control – ensuring that treatments were approximately evenly represented across the different levels of deprivation in the trial area (see Table A2 in Appendix F for Deprivation Index score across conditions). Final sample sizes for each condition were: *n*=496 (sticker prompt), *n*=492 (postcard), *n*=479 (bin liners), *n*=482 (canvassing), *n*=510 (control).

2.6. Intervention delivery

Delivery of the interventions was carried out by team members from Auckland Council Waste Solutions together with P.J. and ancillary staff contracted to the Council to help with projects. The intervention delivery took place between 22 August and 9 September.[12] No blinding was applied to those who delivered the interventions. All personnel involved in delivering the postcards and caddy liners, applying the refuse bin stickers and canvassing households, were made fully aware that they were part of an RCT and of the importance of not contaminating any households in either the control group, or other intervention groups outside of the intervention they were delivering. Checklists were created which listed the specific streets in a given condition, and there was a requirement that these be completed daily during the intervention period. Only the specific addresses on each individual helper's checklist were to be given the specified treatment. If a treatment could not be delivered, this was noted down on the checklist, along with the reason. Scans of these checklists were sent daily to the WasteWise Coordinator. In this way, we ensured that contamination was minimised, as well as retaining a record of which addresses did and did not receive an intervention. A number of the Council and ancillary staff who were involved in delivering the interventions were also

[12] With one exception that is discussed in Section 2.8.

involved in set out monitoring, both before and after the intervention period. It is possible, therefore, that these staff were aware of whether a street that they were monitoring for set out was assigned a particular condition, although they were not aware of the specific predictions we were aiming to test.

For some treatments, not every household in each treatment condition received the intervention. While postcards and bin liners were successfully delivered to almost 100% of addresses in their respective treatment conditions, the canvassing success rate (defined as the proportion of households where a WasteWise advisor had a conversation with a householder about the food scraps service, out of all households randomly assigned to the canvassing condition) was 28% and 27% in the Papakura and North Shore trials, respectively. Two-thirds of the households in the sticker prompt condition had a sticker successfully affixed to their refuse bin. We dealt with this partial delivery of some treatments in our statistical analyses (see Section 2.9).

2.7. Outcome variables and measurement

To measure changes in food scraps recycling behaviour and compare the control and treatment conditions, two binary outcome measures were used: *set out* and *participation*. Set out refers to whether a household, in a given week, sets out a food scraps bin on the day it is usually collected, where 1 = did set out a bin, and 0 = did not. Participation refers to whether a household sets out a food scraps bin for collection *at least once* in a three-consecutive-week period, where 1 = participated and 0 = did not participate. Not all households set out food scraps weekly, due to low levels of food scraps; by setting out a bin at least once during the three consecutive weeks, however, they count as participating in the food scraps service. We monitored set out and participation over three time periods: three consecutive weeks for the baseline period (T1), three consecutive weeks in the first post-intervention period (T2) and three consecutive weeks in the second post-intervention period (T3).

We measured food scraps set out and participation through observation of food scraps recycling behaviour. In the North Shore trial, set out was monitored on Monday, Tuesday, and Wednesday, which are the three collection days for the North Shore service area. In Papakura, set out was monitored on Monday, Thursday, and Friday for streets in our study whose usual collection day is either Monday, Thursday or Friday. The monitors began, on foot, at 7.50 a.m. and continued for as long as it took to observe all addresses on their list. Bin address labels were observed by the monitors to ensure that a bin belonged to a specific address and to keep the accuracy of data collection high.

Food scraps bins are supposed to be set out for collection by 7 a.m. on the morning of collection day. However, in reality, many people set their bin out when they leave for work or school in the morning, which means that bins are still being set out after 7 a.m. The food scraps truck did not always take the same route each week and there were variations in the timing of when a street's bins were collected. It was not always possible therefore, to get to a street before the food scraps truck had collected that street's bins, meaning that sometimes bins were taken back in from the kerbside by their owner before the monitor could record the bin. However, the majority of the time the monitor finished a street prior to the truck arriving, and there is no reason to think the effects of the truck route timing varied across conditions.

All staff were made aware of the importance of accurate data collection. To minimise between-monitor variation in data collection, a standardised recording system was designed, and a training session held before each round of monitoring. Where possible, the same people were used on the same streets each week (See Appendix F for more information about data collection and a copy of the data sheet). One obstacle we faced was that bin address labels were often difficult to read; many bins were several years old, and the labels were partially peeled off or missing completely. As previously mentioned, to ensure that the same bins were being recorded each week, wherever there was doubt about which address a bin belonged to, a letter was marked on the bin so that this bin could be tracked over the course of the study.

Bin marking was also encouraged in other cases, where bin labels were missing or difficult to read, but where it was more obvious that a bin belonged to a specific house. This was done to minimise between-monitor variation. In cases like this, a letter was marked on the bin and a note written in the comments, such as “Bin marked with W on 28/9 – belongs to number 42”. Alternatively, if the bin had a distinguishing feature, such as that it was missing a handle, a note about this was recorded in the comments to help identify the bin in subsequent weeks. In the post-intervention monitoring period, a “previous notes” column was added to the data sheet, where these important notes were kept, alerting monitors to look out for certain things. Besides bin labels, details such as a household setting out two bins, letterboxes with different numbers to the food scraps bin, and houses that looked unoccupied, were recorded. In the latter case, monitors were asked to record whether the house still looked unoccupied the following week. At the end of each week, the comments from the current week were transferred to the “previous notes” column for the subsequent week.

2.8. Other factors

Two unforeseen events occurred during the study period which may have had an effect on set out and/or participation. The New Zealand Government declared a public holiday on September 26 (during week 3 of the first post-intervention monitoring period [T2]), causing changes in the collection days for rubbish, recycling, and food scraps. We had planned to avoid public holidays and school holidays, since people’s waste behaviour changes from their typical behaviour during such times. The change in collection days resulted in some households in Milford with a Wednesday usual collection day setting out their bins on the wrong day. Unfortunately, approximately 40 food scraps bins were emptied by the contractor a day early (bin monitoring was to take place the following day, so some of these bins will have been missed in the set out count). A follow-up analysis was conducted, excluding households with a Milford Wednesday collection day, to compare results with the main analysis. The implications of this discrepancy will be discussed in the Discussion chapter.

Second, there was one exception to the interventions being delivered in the three-week period between 22 August and 9 September. A small subset of households in Papakura who were in the sticker prompt condition and have a Monday collection day, received the treatment on Monday 12 September because of delays in printing the stickers. This catch-up intervention-delivery day overlapped with the first day of monitoring for the first post-intervention period (T2). A follow-up analysis was conducted, excluding these households from set out and participation data. The implications of this will be covered in the Discussion.

2.9. Data analysis

All analyses were conducted using R v4.1.1 (R Core Team, 2022). We used Bayesian multilevel modelling to test our key hypotheses. Throughout all analyses, prior probabilities were determined using prior predictive checks.

2.9.1. ITT and ToT analyses

We conducted two types of analysis for both participation and set out: Intention-to-Treat (ITT), where all households in each condition are included in the final analysis; and Treatment-on-the-Treated (ToT) where only the outcomes for those who received the treatments, and those in the control condition, are considered. The ITT analysis is concerned with the average behaviour of those in the treatment conditions and recognises that not everyone in each condition actually receives the treatment. It gives an unbiased estimate of the causal effect of the treatments across the different conditions. This allows us to make causal claims since randomisation is the only factor affecting which households received a treatment. A ToT analysis is potentially more powerful because it ignores households known to have not received the treatment. However, it is vulnerable to potential confounding from other factors, such as a willingness to talk to a canvasser or type and location of employment (which is a determinant of whether someone is home during the day). This leads to a potentially biased sample of households who ended up receiving the treatment. Poor weather conditions and how much refuse a household produces (a determinant of how often a household puts their

bin out for collection) are variables affecting whether or not a household in the sticker prompt condition had a sticker affixed to their bin. Any findings about treatments in the ToT analysis need to be interpreted in light of this.

2.9.2. Papakura analyses

To test Hypotheses H1a–1d and H2a–2d (Section 1.9) we used a hierarchical repeated measures logistic regression, which included random effects for household and street cluster, that predicted the outcome variable – participation for H1a–1d and set out for H2a–2d – from the interaction between a categorical variable representing our treatment conditions (control, sticker prompt, postcard, bin Liners, canvassing) and a categorical variable representing timepoints (pre-intervention, first post-intervention, and second post-intervention). We used a hierarchical model to account for the two levels in our data: household level and street level. The model formulae for set out and participation respectively, are as follows:

$$
\begin{aligned}
\text{Set out}_i &\sim \text{Bernoulli}(p_i) \\
\text{logit}(p_i) &= \gamma_{\text{cluster}[i]} + \kappa_{\text{cluster:id}[i]} + \beta_j(1 + \text{Timepoint}_{T2,i} + \text{Timepoint}_{T3,i})(1 + \text{Condition}_{2,i} + \ldots + \text{Condition}_{n,i}) \\
\gamma_{\text{cluster}} &\sim \text{Normal}(0, \sigma_\gamma) \\
\kappa_{\text{cluster:id}} &\sim \text{Normal}(0, \sigma_\kappa) \\
\beta_j &\sim \text{Normal}(0, 1) \\
\sigma_\gamma &\sim \text{Exponential}(2) \\
\sigma_\kappa &\sim \text{Exponential}(2)
\end{aligned}
$$

$$
\begin{aligned}
\text{Participation}_i &\sim \text{Bernoulli}(p_i) \\
\text{logit}(p_i) &= \gamma_{\text{cluster}[i]} + \kappa_{\text{cluster:id}[i]} + \beta_j(1 + \text{Timepoint}_{T2,i} + \text{Timepoint}_{T3,i})(1 + \text{Condition}_{2,i} + \ldots + \text{Condition}_{n,i}) \\
\gamma_{\text{cluster}} &\sim \text{Normal}(0, \sigma_\gamma) \\
\kappa_{\text{cluster:id}} &\sim \text{Normal}(0, \sigma_\kappa) \\
\beta_j &\sim \text{Normal}(0, 1) \\
\sigma_\gamma &\sim \text{Exponential}(2) \\
\sigma_\kappa &\sim \text{Exponential}(2)
\end{aligned}
$$

Here, $\kappa_{cluster:id[i]}$ refers to individual households nested within street clusters. $Timepoint_i$ is a factor with three levels (T1, T2, T3) where T1 is the baseline, and $Condition_i$ is a factor with five levels (sticker, postcard, bin liners, canvassing, control) where control is the baseline. In both models, for the Intercept and Beta coefficients, a Normal Distribution with μ=0, σ=1, and for the Standard Deviation, an Exponential Distribution (rate = 2), was used.

2.9.3. North Shore analyses

To test H3a and H4a (Section 1.9), we estimated the change from pre- to post-intervention in the North Shore trial where all households received the free bin liners intervention and there was no control group. We used a hierarchical repeated measures logistic regression with random effects for household and street cluster, that predicted the outcome variable – participation for H3a and set out for H4a – from a categorical variable representing the three time points (pre-intervention, first post-intervention, and second post-intervention). This allowed us to identify if any of the intervention combinations had an effect on the change in set out and/or participation. The model formulae for set out and participation follow:

$$
\begin{aligned}
\text{Set out}_i &\sim \text{Bernoulli}(p_i) \\
\text{logit}(p_i) &= \gamma_{\text{cluster}[i]} + \kappa_{\text{cluster:id}[i]} + \beta_j(1 + \text{Timepoint}_{T2,i} + \text{Timepoint}_{T3,i}) \\
\gamma_{\text{cluster}} &\sim \text{Normal}(0, \sigma_\gamma) \\
\kappa_{\text{cluster:id}} &\sim \text{Normal}(0, \sigma_\kappa) \\
\beta_j &\sim \text{Normal}(0, 1) \\
\sigma_\gamma &\sim \text{Exponential}(2) \\
\sigma_\kappa &\sim \text{Exponential}(2)
\end{aligned}
$$

$$
\begin{aligned}
\text{Participation}_i &\sim \text{Bernoulli}(p_i) \\
\text{logit}(p_i) &= \gamma_{\text{cluster}[i]} + \kappa_{\text{cluster:id}[i]} + \beta_j(1 + \text{Timepoint}_{T2,i} + \text{Timepoint}_{T3,i}) \\
\gamma_{\text{cluster}} &\sim \text{Normal}(0, \sigma_\gamma) \\
\kappa_{\text{cluster:id}} &\sim \text{Normal}(0, \sigma_\kappa) \\
\beta_j &\sim \text{Normal}(0, 1) \\
\sigma_\gamma &\sim \text{Exponential}(2) \\
\sigma_\kappa &\sim \text{Exponential}(2)
\end{aligned}
$$

Here, $\kappa_{cluster:id[i]}$ refers to individual households nested within street clusters. $Timepoint_i$ is a factor with three levels (T1, T2, T3) where T1 is the baseline.

To test H3b–3c and H4b–4c (Section 1.9), we estimated the effect of receiving either a postcard or canvassing in addition to free bin liners. We used the same type of model described in Section 2.9.2, which predicted the outcome variable – participation for H3a–3d and set out for H4a–4d – from the interaction between a categorical variable representing our three treatment conditions (postcard plus bin liners, canvassing plus bin liners, bin liners-only) and a categorical variable representing the three timepoints (pre-intervention, first post-

intervention, and second post-intervention). The model formulae for set out and participation respectively, are as follows:

$$
\begin{aligned}
\text{Set out}_i &\sim \text{Bernoulli}(p_i) \\
\text{logit}(p_i) &= \gamma_{\text{cluster}[i]} + \kappa_{\text{cluster:id}[i]} + \beta_j(1 + \text{Timepoint}_{T2,i} + \text{Timepoint}_{T3,i})(1 + \text{Condition}_{2,i} + \ldots + \text{Condition}_{n,i}) \\
\gamma_{\text{cluster}} &\sim \text{Normal}(0, \sigma_\gamma) \\
\kappa_{\text{cluster:id}} &\sim \text{Normal}(0, \sigma_\kappa) \\
\beta_j &\sim \text{Normal}(0, 1) \\
\sigma_\gamma &\sim \text{Exponential}(2) \\
\sigma_\kappa &\sim \text{Exponential}(2)
\end{aligned}
$$

$$
\begin{aligned}
\text{Participation}_i &\sim \text{Bernoulli}(p_i) \\
\text{logit}(p_i) &= \gamma_{\text{cluster}[i]} + \kappa_{\text{cluster:id}[i]} + \beta_j(1 + \text{Timepoint}_{T2,i} + \text{Timepoint}_{T3,i})(1 + \text{Condition}_{2,i} + \ldots + \text{Condition}_{n,i}) \\
\gamma_{\text{cluster}} &\sim \text{Normal}(0, \sigma_\gamma) \\
\kappa_{\text{cluster:id}} &\sim \text{Normal}(0, \sigma_\kappa) \\
\beta_j &\sim \text{Normal}(0, 1) \\
\sigma_\gamma &\sim \text{Exponential}(2) \\
\sigma_\kappa &\sim \text{Exponential}(2)
\end{aligned}
$$

Here, $\kappa_{cluster:id[i]}$ refers to individual households nested within street clusters. $Timepoint_i$ is a factor with three levels (T1, T2, T3) where T1 is the baseline, and $Condition_i$ is a factor with three levels (postcard plus bin liners, canvassing plus bin liners, bin liners-only), where bin liners-only is the baseline. In all four North Shore models, for the Intercept and Beta coefficients, a Normal Distribution with μ=0, σ=1, and for the Standard Deviation, an Exponential Distribution (rate = 2), was used. All models described above were used for both the ITT and ToT analyses.[13]

2.9.4. Follow-up analyses

In the Papakura trial, to account for the overlapping day in the first post-intervention participation monitoring period (T2), we did follow-up analyses for set out and participation. For set out, our follow-up models omitted the set out data for week 1 of T2, for the 25 households that received their refuse bin sticker on Monday 12 September. For participation, our follow-up models excluded the 25 households entirely. To account for the collection discrepancy that occurred in Milford on Wednesday 28 September, we ran follow-up analyses

[13] We decided to conduct a ToT analysis in addition to an ITT analysis, only after submitting the pre-registration.

that tested models for set out, that omitted data from week 3 of T2 for households in Milford that normally put their bin out for collection on Wednesday. For participation, we omitted Milford Wednesday households from the T2 participation monitoring round entirely.

3. Results

3.1. Descriptive statistics for Papakura set out

We first explored whether the interventions had an effect on set out, which refers to whether a household, in a given week, sets out a food scraps bin on the day it is usually collected. The final sample size for the Papakura trial Intention-to-Treat (ITT) set out analysis was *n*=2459 households (*n*=7377 observations). After households that did not receive a treatment had been removed, the final sample size for the Papakura Treatment-on-the-Treated (ToT) analysis was *n*=1941 households (*n*=5823 observations). Tables 3 and 4 and Figures 2 and 3 display descriptive statistics for set out in the Papakura trial for the ITT analysis and ToT analysis, respectively.

Table 3

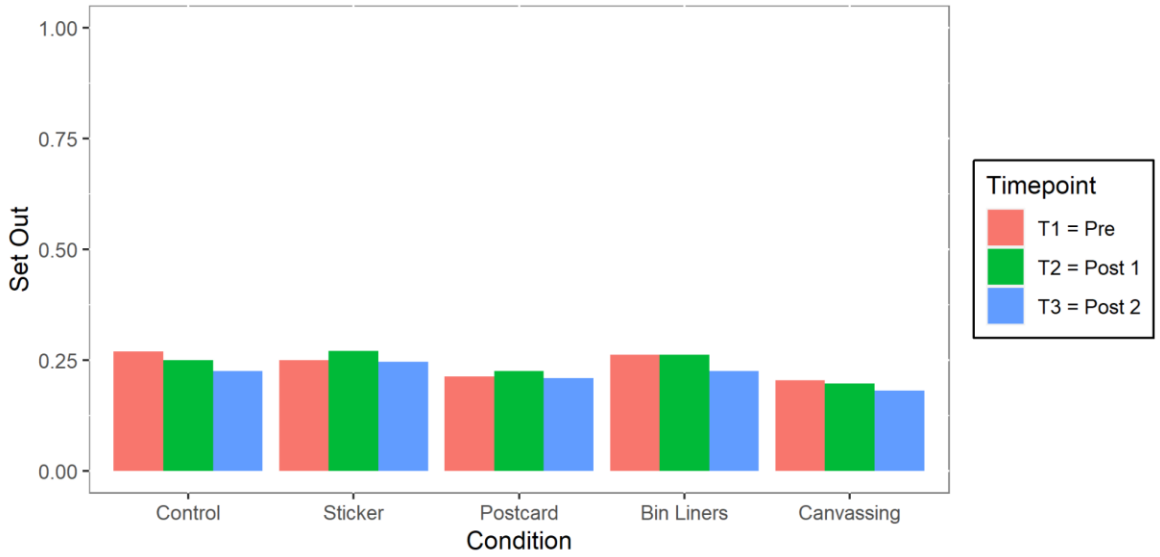
Papakura Set Out Proportions (ITT) by Condition, Pre- and Post-intervention.

			T1 (Pre)		T2 (Post 1)		T3 (Post 2)	
Condition	Households	Obs	*n* (obs)	Set out proportion	*n* (obs)	Set out proportion	*n* (obs)	Set out proportion
Control	510	1530	412	0.27	383	0.25	345	0.23
Sticker	496	1488	371	0.25	402	0.27	367	0.25
Postcard	492	1476	315	0.21	333	0.23	309	0.21
Bin Liners	479	1437	377	0.26	377	0.26	324	0.23
Canvassing	482	1446	295	0.20	285	0.20	262	0.18

Note: Obs = number of observations

Figure 2

Papakura Set Out Proportions (ITT) by Condition, Pre- and Post-intervention



Note: Set out proportions for the five conditions are shown for T1 (Pre), T2 (Post 1) and T3 (Post 2) time points. Set out is calculated as the proportion of households setting out their food scraps bin each week, averaged across a 3-week observation period.

Table 4

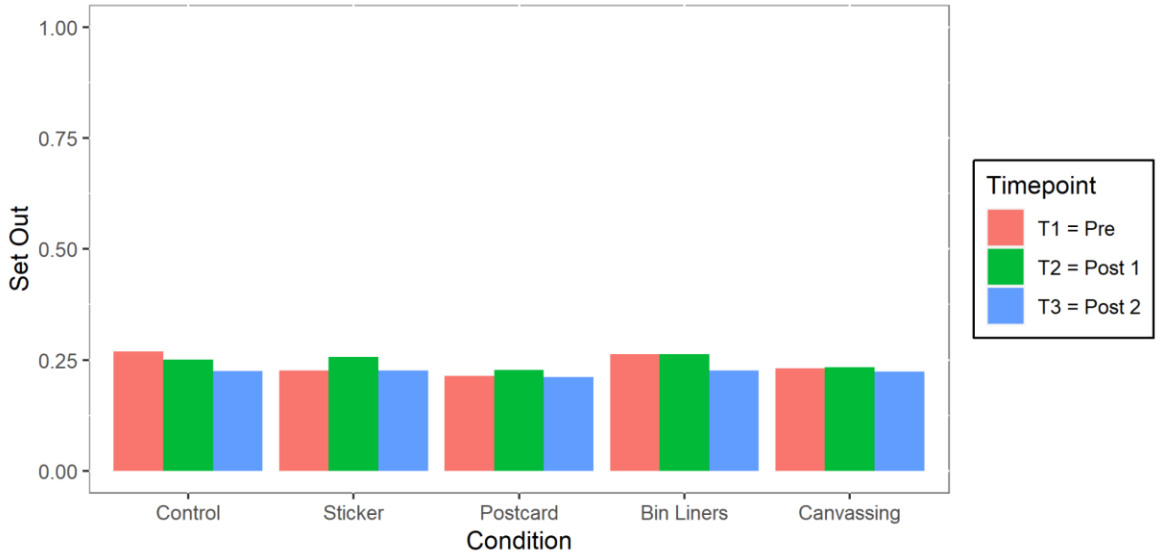
Papakura Set Out Proportions (ToT) by Condition, Pre- and Post-intervention

			T1 (Pre)		T2 (Post 1)		T3 (Post 2)	
Condition	Households	Obs	*n* (obs)	Set out proportion	*n* (obs)	Set out proportion	*n* (obs)	Set out proportion
Control	510	1530	412	0.27	383	0.25	345	0.23
Sticker	332	996	226	0.23	256	0.26	226	0.23
Postcard	488	1464	314	0.21	333	0.23	309	0.21
Bin Liners	477	1431	377	0.26	377	0.26	324	0.23
Canvassing	134	402	93	0.23	94	0.23	90	0.22

Note: Obs = number of observations

Figure 3

Papakura Set Out Proportions (ToT) by Condition, Pre- and Post-intervention



Note: Set out proportions for the five conditions are shown for T1 (Pre), T2 (Post 1) and T3 (Post 2) time points. Set out is calculated as the proportion of households setting out their food scraps bin each week, averaged across a 3-week observation period.

The results from both ITT and ToT data show that while the bin liner, canvassing and control conditions show static and/or declining set out over the study duration, the sticker prompt and postcard conditions appear to increase between T1 and T2, although this is attenuated by T3. In what follows we statistically test whether any of these changes support our hypothesised predictions.

3.2. Regression analysis for Papakura set out

We hypothesised that in the Papakura field experiment, set out in the food scraps collection service would show a greater increase post-intervention (relative to pre-intervention) in the bin liners (H2a), postcard (H2b), canvassing (H2c) and sticker prompt (H2d) conditions than in the control condition. To test these hypotheses, we used a hierarchical repeated measures logistic regression, with random effects for household and street cluster, predicting set out from the interaction between a categorical variable representing our treatment conditions (control [baseline], bin liners, postcard, canvassing, and sticker prompt) and a categorical

variable indicating the time point when measurement took place (T1, T2, T3). Support for Hypotheses 1a–1d would be indicated by set out in the food scraps collection service showing a significantly greater increase post-intervention (relative to pre-intervention) in the bin liners (H2a), postcard (H2b), canvassing (H2c), and sticker prompt (H2d) conditions than in the control condition.

For each hypothesis test we fitted two (otherwise identical) models; one that included all households in the sample (ITT) and one with only households that received a treatment (ToT). We did this because some treatments were difficult to deliver to everyone in the treatment condition and we wanted to investigate whether there were any differences in outcome measures when considering the direct effect of the treatments on those that actually received them, as opposed to the average effect of the treatments, as seen in the treatment condition as a whole. Because various factors may influence whether a household actually receives treatment, ToT analyses do not provide the rigorous test of causation that ITT analyses do – we discuss this further in the limitations section of the Discussion chapter. The results for both the ITT and ToT regressions are presented in Table 5.

Table 5

Regression Findings for Papakura Food Scraps Set Out

Parameter	Intention to Treat				Treatment on Treated			
	Estimate	Est.Error	Q2.5	Q97.5	Estimate	Est.Error	Q2.5	Q97.5
Intercept	-3.19	0.36	-3.90	-2.47	-3.13	0.35	-3.83	-2.46
TimepointT2	-0.22	0.13	-0.47	0.03	-0.21	0.13	-0.46	0.05
TimepointT3	-0.56	0.13	-0.82	-0.32	-0.55	0.13	-0.79	-0.30
CONDITIONSticker	0.04	0.51	-0.95	1.03	-0.25	0.52	-1.26	0.79
CONDITIONPostcard	-0.38	0.51	-1.40	0.60	-0.40	0.53	-1.44	0.62
CONDITIONBinLiners	0.17	0.51	-0.84	1.16	0.16	0.51	-0.86	1.18
CONDITIONCanvassing	-0.68	0.52	-1.68	0.33	-0.32	0.56	-1.44	0.78
TimepointT2:CONDITIONSticker	0.47	0.18	0.13	0.83	0.60	0.21	0.20	1.01
TimepointT3:CONDITIONSticker	0.51	0.18	0.16	0.87	0.52	0.20	0.13	0.92
TimepointT2:CONDITIONPostcard	0.39	0.19	0.01	0.78	0.39	0.19	0.01	0.77
TimepointT3:CONDITIONPostcard	0.48	0.19	0.10	0.86	0.47	0.19	0.10	0.86
TimepointT2:CONDITIONBinLiners	0.21	0.18	-0.13	0.56	0.20	0.19	-0.15	0.58
TimepointT3:CONDITIONBinLiners	0.05	0.18	-0.29	0.43	0.04	0.18	-0.32	0.39
TimepointT2:CONDITIONCanvassing	0.09	0.20	-0.28	0.48	0.19	0.28	-0.35	0.73
TimepointT3:CONDITIONCanvassing	0.15	0.20	-0.23	0.55	0.39	0.28	-0.16	0.93

Note:
Table presents regression findings from repeated measures logistic regression models, with random effects for household and street cluster. Coefficients are on the log odds scale for ITT (orange) and ToT (blue).
Q2.5 and Q97.5 refers to Bayesian 95% Credible Intervals.

The ITT analysis shows there are interaction effects for T2 and sticker (log odds = 0.47, 95% Credible Interval (CI) [0.13, 0.83]), T3 and sticker (log odds = 0.51, 95% CI [0.16, 0.87]), T2 and postcard (log odds = 0.39, 95% CI [0.01, 0.78]) and T3 and postcard (log odds = 0.48, 95% CI [0.10, 0.86]), but no support for an interaction with time point in the canvassing or bin liners conditions. Between T1 and T2 in the control condition, there was a 20% decrease in the odds of set out (posterior mean Odds Ratio [OR] = exp(-0.22) = 0.8). In contrast, there was a 28% increase in the odds of set out in the sticker prompt condition (OR = exp (-0.22 + 0.47) = 1.28), and an 19% increase for the postcard condition (OR = exp (-0.22 + 0.39) = 1.19). Between T1 and T3, the control condition saw a 43% decrease in the odds of set out (OR = exp (-0.56) = 0.57), compared to a 5% decrease in the sticker prompt condition (OR = exp (-0.56 + 0.51) = 0.95) and a 8% decrease in the postcard condition (OR = exp (-0.56 +

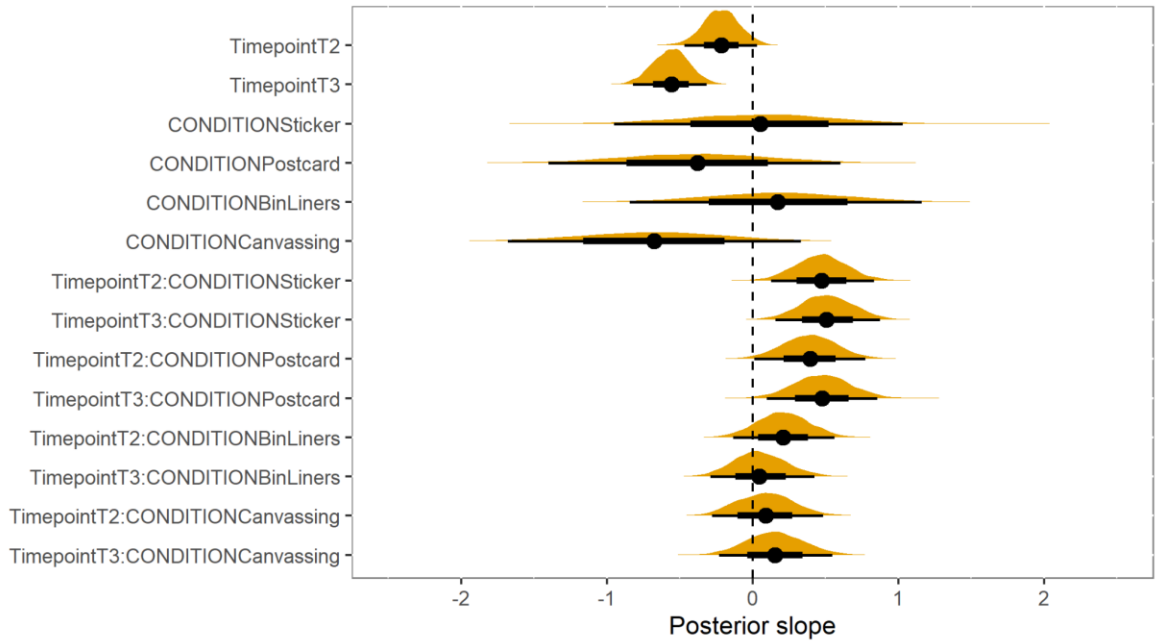
0.48) = 0.92). These results support the hypotheses that, compared to the control condition, set out rates pre- versus post-intervention were greater in both sticker prompt (H1a) and postcard (H1b) conditions. We find no support for an effect of providing free bin liners (H1c) or canvassing (H1d) on set out rates post-intervention.

These results are mirrored in the ToT analysis, although parameter estimates for interaction effects differ slightly: for T2 and sticker (log odds = 0.60, 95% CI [0.20, 1.01]), T3 and sticker (log odds = 0.52, 95% CI [0.13, 0.92]), T2 and postcard (log odds = 0.39, 95% CI [0.01, 0.77]) and T3 and postcard (log odds = 0.47, 95% CI [0.10, 0.86]). In terms of percentage change in the odds of set out, between T1 and T2 in the control condition there was a 19% decrease (posterior mean OR = exp (-0.21) = 0.81). In contrast, there was a 48% increase in the odds of set out in the sticker prompt condition (OR = exp (-0.21 + 0.60) = 1.48), and a 20% increase for the postcard condition (OR = exp (-0.21 + 0.39) = 1.20). Between T1 and T3 the control condition saw a 42% decrease in the odds of set out (OR = exp (-0.55) = 0.58), compared to a 3% decrease in the sticker prompt condition (OR = exp (-0.55 + 0.52) = 0.97) and a 8% decrease in the postcard condition (OR = exp (-0.55 + 0.47) = 0.92). As for the ITT analyses, these results support the hypotheses that, compared to the control condition, set out rates pre- versus post-intervention were greater in both sticker prompt (H1a) and postcard (H1b) conditions, but are not affected by providing free bin liners (H1c) or canvassing (H1d).

Figures 4 and 5 show the posterior distributions of the model parameters in the ITT and ToT analyses respectively. When comparing the ITT and ToT plots, we see slightly stronger effects post-intervention for the sticker prompt condition in the latter, and a trend towards a positive effect of canvassing at T3 (though the confidence interval includes zero). Nevertheless, there is a considerable amount of overlap between the respective posterior distributions for all of the model parameters under the ITT and ToT analyses.

Figure 4

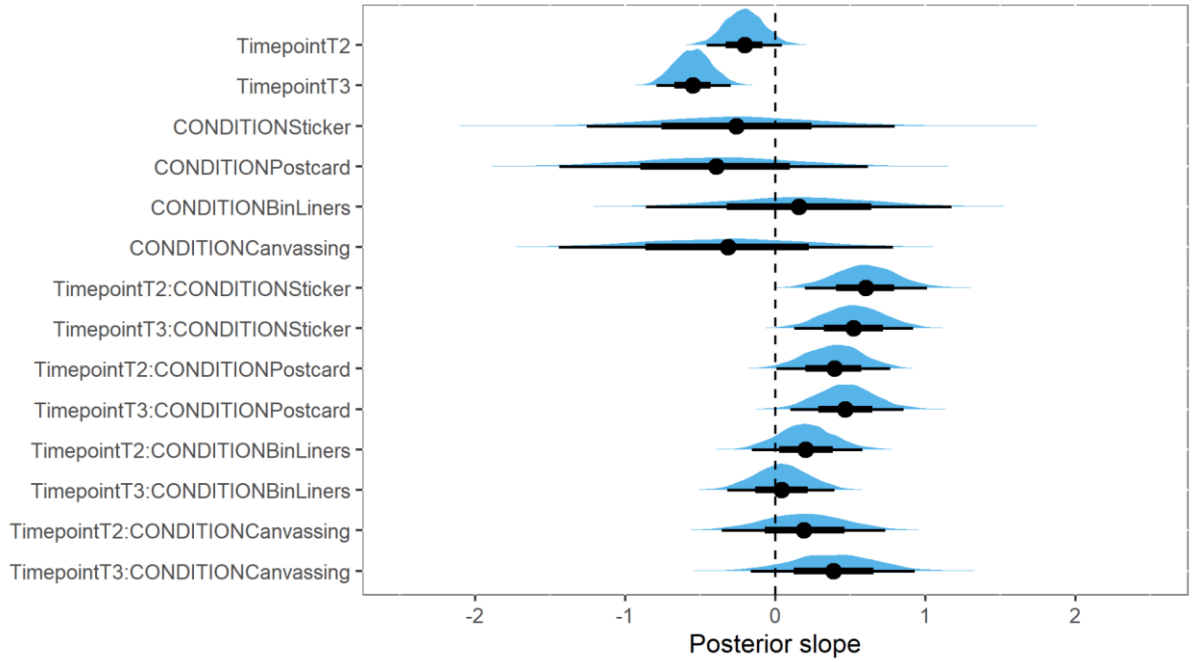
Posterior Distribution of Model Parameters for Papakura ITT Set Out Analysis



Note: Points are median posterior estimates, thick black lines are 66% CIs, and thin black lines are 95% CIs.

Figure 5

Posterior Distribution of Model Parameters for Papakura ToT Set Out



Note: Points are median posterior estimates, thick black lines are 66% CIs, and thin black lines are 95% CIs.

We also conducted a follow-up analysis which compared the results from the models in Table 5 with those from models that excluded the set out data for week 1 of T2, for the 25 households in Papakura that received the sticker prompt treatment on Monday 12 September. This was the result of printing delays, which meant that we decided to use Monday 12 September – the first day of the post-intervention monitoring period – as a catch-up day to deliver the sticker prompt treatment. The parameter estimates are almost identical to those shown in Table 5, and none of the conclusions change. See Table A3 in Appendix G for follow-up analysis results.

3.3. Descriptive statistics for North Shore set out

The final sample size for the North Shore trial ITT analysis was *n*=1513 households (*n*=4539 observations). After removing households for the ToT analysis, the final sample size was *n*=1153 households (*n*=3459 observations). Tables 6 and 7 summarise proportions for set out in the North Shore trial for the ITT analysis and ToT analysis, respectively. This information is also displayed in Figures 6 and 7. Because in the North Shore experiment we were interested in whether food scraps set out rates increased overall in the post-intervention time points (T2 and T3) relative to T1, we have also included observed statistics for total set out in the North Shore tables and figures in this section.

Table 6

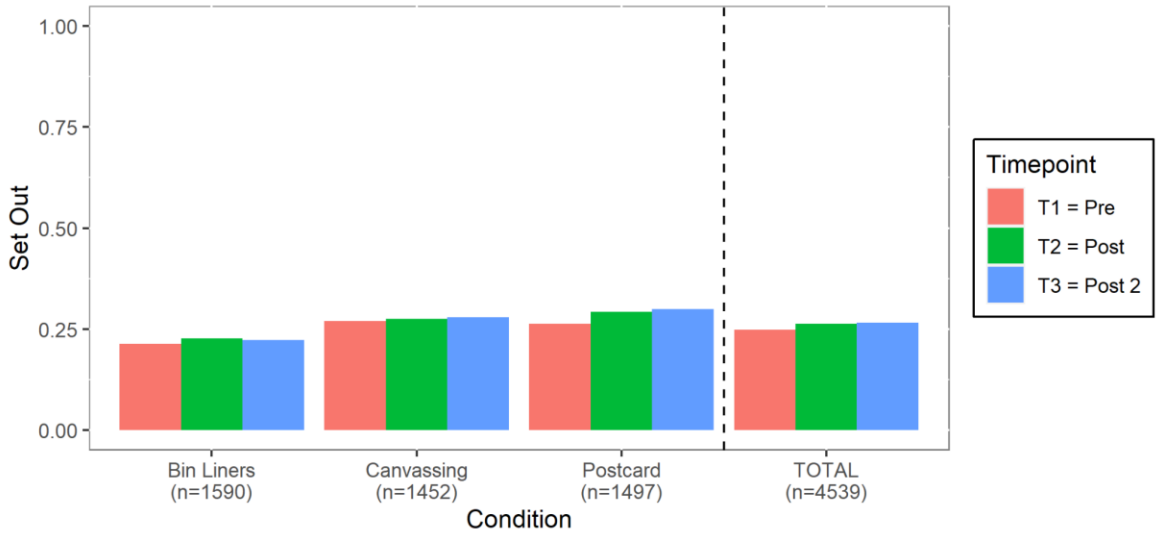
North Shore Set Out Proportions (ITT) by Condition, Pre- and Post-intervention

			T1 (Pre)		T2 (Post 1)		T3 (Post 2)	
Condition	Households	Obs	*n* (obs)	Set out proportion	*n* (obs)	Set out proportion	*n* (obs)	Set out proportion
Bin Liners	530	1590	340	0.21	360	0.23	354	0.22
Postcard	499	1497	394	0.26	439	0.29	449	0.30
Canvassing	484	1452	393	0.27	399	0.27	405	0.28
Total	1513	4539	1127	0.25	1198	0.26	1208	0.27

Note: The Bin Liners condition refers to the "bin liners-only" treatment; Postcard refers to the "postcard plus bin liners" treatment; Canvassing refers to the "canvassing plus bin liners" treatment and Total refers to all conditions combined. Obs = number of observations

Figure 6

North Shore Set Out Proportions (ITT) by Condition, Pre- and Post-intervention



Note: Set out proportions for the three conditions are shown for T1 (Pre), T2 (Post 1) and T3 (Post 2) time points. Set out is calculated as the proportion of households setting out their food scraps bin each week, averaged across a 3-week observation period. n refers to number of observations.

Table 7

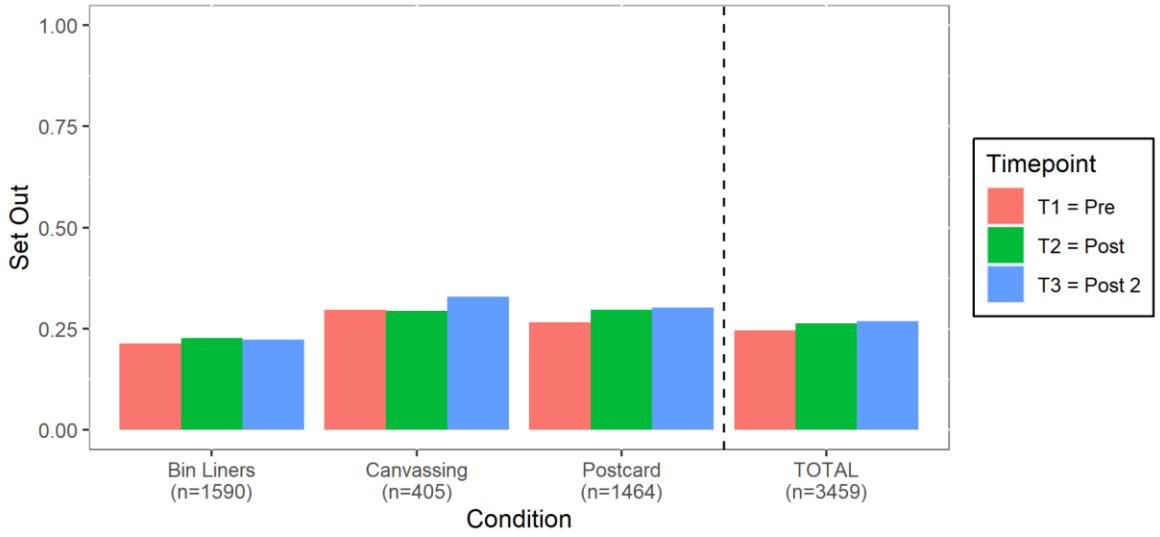
North Shore Set Out Proportions (ToT) by Condition, Pre- and Post-intervention

			T1 (Pre)		T2 (Post 1)		T3 (Post 2)	
Condition	Households	Obs	*n* (obs)	Set out proportion	*n* (obs)	Set out proportion	*n* (obs)	Set out proportion
Bin Liners	530	1590	340	0.21	360	0.23	354	0.22
Postcard	488	1464	389	0.27	434	0.30	443	0.30
Canvassing	135	405	120	0.30	119	0.29	133	0.33
Total	1153	3459	849	0.25	913	0.26	930	0.27

Note: The Bin Liners condition refers to the "bin liners-only" treatment; Postcard refers to the "postcard plus bin liners" treatment; Canvassing refers to the "canvassing plus bin liners" treatment and Total refers to all conditions combined. Obs = number of observations

Figure 7

North Shore Set Out Proportions (ToT) by Condition, Pre- and Post-intervention



Note: Set out proportions for the three conditions are shown for T1 (Pre), T2 (Post 1) and T3 (Post 2) time points. Set out is calculated as the proportion of households setting out their food scraps bin each week, averaged across a 3-week observation period. n refers to number of observations.

This pattern of results suggests that there was an overall increase in set out between T1 and T2 and T1 and T3. It also suggests that set out increased between T1 and T3 for all three conditions, but between T1 and T2 an increase is only seen in the bin liner-only and postcard

conditions. In what follows we statistically test whether any of these changes support our hypothesised predictions.

3.4. Regression analysis for North Shore set out

In our North Shore field experiment, constraints from our partners at Auckland Council meant all households received the free bin-liners between the pre-treatment (T1) and post-treatment (T2 and T3) timepoints, so that we have no no-treatment control condition against which to evaluate intervention effects over time. Instead, we first test whether food scraps set out rates increased overall in the post-intervention time points (T2 and T3) relative to T1 (H4a). To do this, we used the same modelling framework as above, with support for Hypothesis 4a indicated by a significant positive effect of the post-intervention indicators (T2 and T3). The results of both ITT and ToT analyses are presented in Table 8.

Table 8

Regression Findings for North Shore Food Scraps Set Out

Parameter	Intention to Treat				Treatment on Treated			
	Estimate	Est.Error	Q2.5	Q97.5	Estimate	Est.Error	Q2.5	Q97.5
Intercept	-3.87	0.28	-4.41	-3.33	-3.90	0.3	-4.47	-3.30
TimepointT2	0.25	0.09	0.09	0.42	0.30	0.1	0.11	0.51
TimepointT3	0.29	0.08	0.12	0.45	0.39	0.1	0.20	0.58

Note:
Table presents regression findings from repeated measures logistic regression models, with random effects for household and street cluster. Coefficients are on the log odds scale for ITT (orange) and ToT (blue).
Q2.5 and Q97.5 refers to Bayesian 95% Credible Intervals.

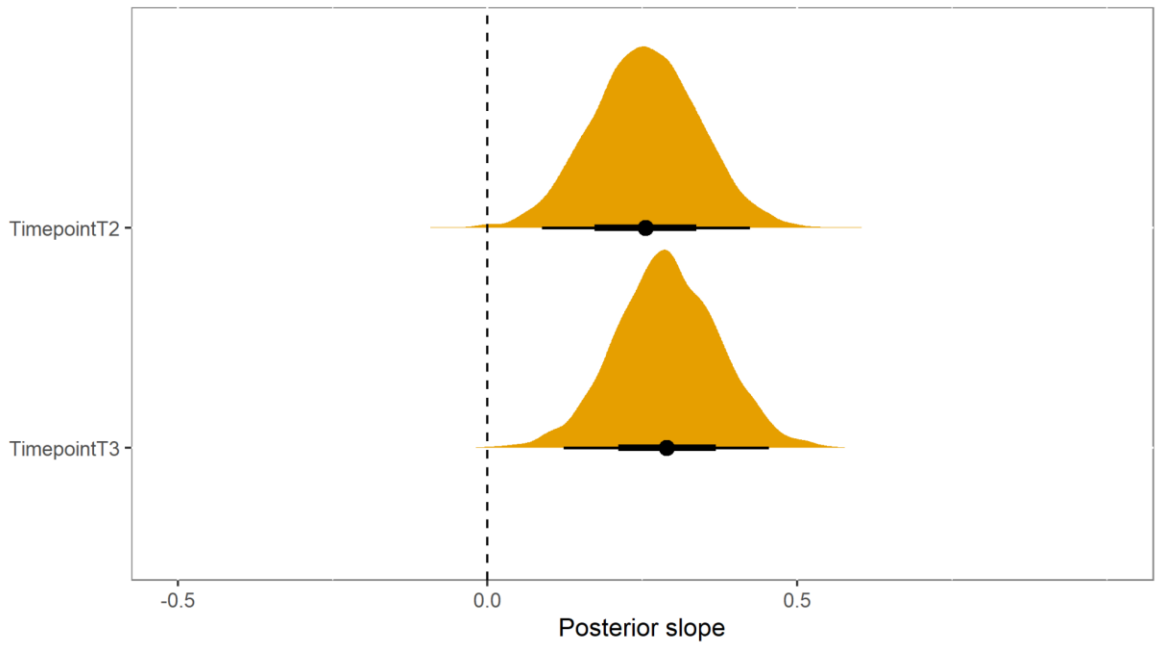
The ITT analysis shows that set out increased in the post-intervention period compared to T1 (at T2, log odds = 0.25, 95% CI [0.09, 0.42], and at T3, log odds = 0.29, 95% CI [0.08, 0.12]). From T1 to T2, the odds of set out increased by 28% (posterior mean OR = exp (0.25) = 1.28), while from T1 to T3 they increased by 34% (OR = exp (0.29) = 1.34), meaning that H4a is supported. The ToT analysis results also revealed the same post-intervention increase (at T2,

log odds = 0.30, 95% CI [0.11, 0.51], and at T3, log odds = 0.39, 95% CI [0.20, 0.58]). At T2, the odds of set out have increased by 35% (OR = exp (0.3) = 1.35) compared to T1, and at T3, there is a 48% increase (OR = exp (0.39) = 1.48) in the odds of set out compared to T1. The fact that the overall effect is larger for the ToT sample than for the ITT sample is consistent with the effect being due to treatment (and hence larger when only considering those who were actually treated), although selection bias cannot be ruled out as an alternative. The implications of this will be addressed in the Discussion chapter.

Figures 8 and 9 show the posterior distributions of the model parameters in the ITT and ToT analyses, respectively. The credible intervals are very similar in the ITT and ToT analyses and in both are clearly above zero. However, in the ToT model there are slightly stronger effects, particularly for the T3 parameter.

Figure 8

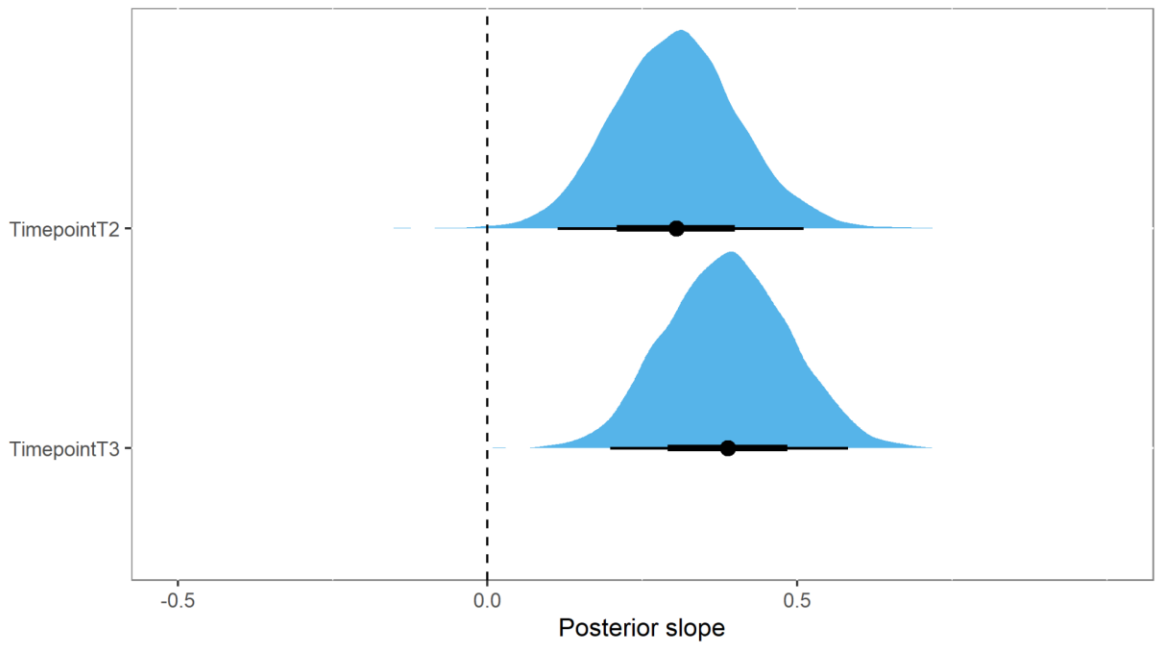
Posterior Distribution of Model Parameters for North Shore ITT Set Out Analysis



Note: Points are median posterior estimates, thick black lines are 66% CIs, and thin black lines are 95% CIs.

Figure 9

Posterior Distribution of Model Parameters for North Shore ToT Set Out Analysis



Note: Points are median posterior estimates, thick black lines are 66% CIs, and thin black lines are 95% CIs.

Next we tested whether, compared to the bin liners-only condition, set out rates are higher post- versus pre-intervention in the postcard plus bin liners (H3b) or canvassing plus bin liners conditions – that is, whether the postcard or canvassing treatments have a positive effect on set out rates over and above the effects of providing free bin liners. The findings from the ITT and ToT analyses are presented in Table 9 and the posterior distributions for the parameters are illustrated in Figures 10 and 11.

Table 9

Regression Findings for North Shore Food Scraps Set Out

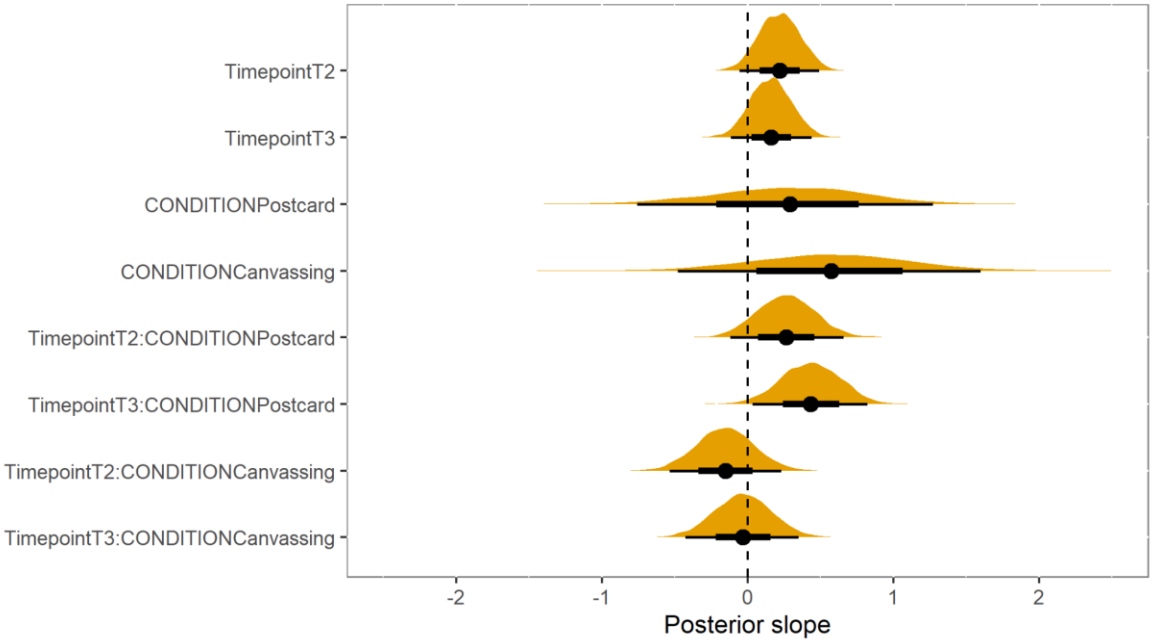
Parameter	Intention to Treat				Treatment on Treated			
	Estimate	Est.Error	Q2.5	Q97.5	Estimate	Est.Error	Q2.5	Q97.5
Intercept	-4.15	0.41	-4.91	-3.29	-4.19	0.41	-4.93	-3.32
TimepointT2	0.22	0.14	-0.06	0.49	0.22	0.14	-0.07	0.50
TimepointT3	0.16	0.14	-0.11	0.44	0.16	0.14	-0.12	0.43
CONDITIONPostcard	0.28	0.52	-0.76	1.27	0.38	0.51	-0.71	1.34
CONDITIONCanvassing	0.57	0.53	-0.48	1.60	0.62	0.59	-0.59	1.74
TimepointT2:CONDITIONPostcard	0.26	0.20	-0.12	0.66	0.28	0.20	-0.12	0.66
TimepointT3:CONDITIONPostcard	0.43	0.20	0.04	0.82	0.44	0.20	0.04	0.85
TimepointT2:CONDITIONCanvassing	-0.15	0.20	-0.53	0.23	-0.22	0.28	-0.77	0.34
TimepointT3:CONDITIONCanvassing	-0.03	0.20	-0.43	0.35	0.32	0.29	-0.23	0.88

Note:

Table presents regression findings from repeated measures logistic regression models, with random effects for household and street cluster. Coefficients are on the log odds scale for ITT (orange) and ToT (blue).
Q2.5 and Q97.5 refers to Bayesian 95% Credible Intervals.

Figure 10

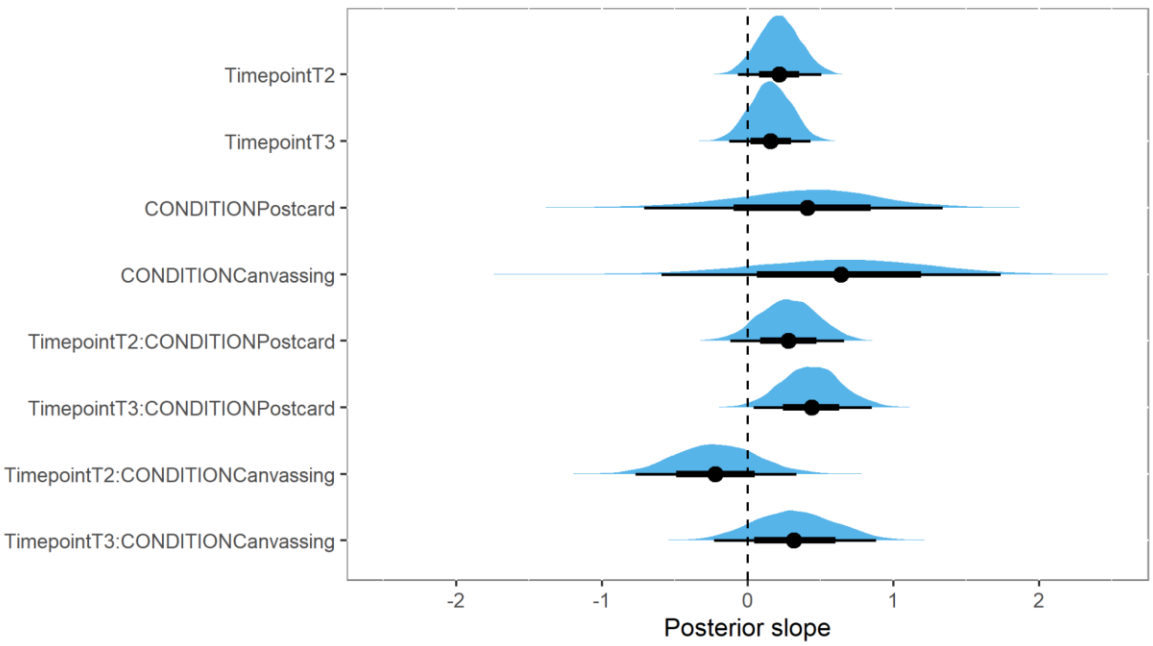
Posterior Distribution of Model Parameters for North Shore ITT Set Out Analysis



Note: Points are median posterior estimates, thick black lines are 66% CIs, and thin black lines are 95% CIs.

Figure 11

Posterior Distribution of Model Parameters for North Shore ToT Set Out Analysis



Note: Points are median posterior estimates, thick black lines are 66% CIs, and thin black lines are 95% CIs.

For the ITT analysis, we see evidence for an interaction effect for the postcard condition at T3 (log odds = 0.43, 95% CI [0.04, 0.82]), meaning that the change in set out from T1 to T3 was greater in the postcard condition than in the bin liners-only condition. This effect is also evident in the ToT regression, where the results are almost identical (log odds = 0.44, 95% CI [0.04, 0.85]). For ITT, the postcard condition saw an 80% increase (posterior mean OR = exp (0.16 + 0.43) = 1.80) in the odds of set out between T1 and T3, compared to a 17% increase (OR = exp (0.16) = 1.17) in the bin liners-only condition. For the ToT analysis, the odds of set out at T3 are increased by 82% (OR = exp (0.16 + 0.44) = 1.82) relative to T1, compared to a 17% increase (OR = exp (0.16) = 1.17) for bin liners-only. Figures 10 and 11 show that the interaction effect for the postcard at T2 is also of note, as the bulk of the credible interval is above zero.

We also conducted follow-up analyses comparing the results in Tables 8 and 9 with those from models that excluded set out data from week 3 of T2, belonging to households in Milford

that have their usual collection day on a Wednesday. Some of these households were subject to the collection day discrepancy on Wednesday 28 September, discussed in Section 2.8. Due to a public holiday on Monday 26 September, usual collection days were pushed back by one day. Some households in Milford accidentally had their food scraps bin emptied by the collection truck on Wednesday 28th, when collection (and monitoring) was not scheduled to occur until Thursday 29th. As we do not know which houses had their bins emptied early, we excluded all households with a Milford Wednesday collection day, in set out data for that week. The parameter estimates and credible intervals in the follow-up model testing H4b–4c were almost identical to those in the original model. The estimates in the follow-up model testing H4a were also similar to those in the original model. See Tables A4 and A5 in Appendix G, for follow-up analysis results.

3.5. Descriptive statistics for Papakura participation

We repeated the same analyses as reported in Section 3.2 for our other outcome variable, participation. Set out gives us information about the proportion of households using the service in any given week. The set out analyses told us whether the food scraps service is being used more in some conditions than in others and whether the service is being used more frequently in the post-intervention periods. However, it does not give us any information about whether the total number of households participating has increased post-intervention. This is why we repeated the analyses described above, for participation. To count as “participating”, a household needs to set their food scraps bin out at least once over a three-consecutive-week period. Tables 10 and 11 and Figures 12 and 13 display descriptive statistics for participation in the Papakura trial for the ITT analysis and ToT analysis, respectively.

Table 10

Papakura Participation Proportions (ITT) by Condition, Pre- and Post-intervention

		T1 (Pre)		T2 (Post 1)		T3 (Post 2)	
Condition	Households	*n*	Participation proportion	*n*	Participation proportion	*n*	Participation proportion
Control	510	185	0.36	175	0.34	168	0.33
Sticker	496	173	0.35	186	0.38	179	0.36
Postcard	492	152	0.31	150	0.30	147	0.30
Bin Liners	479	175	0.37	174	0.36	163	0.34
Canvassing	482	134	0.28	129	0.27	132	0.27

Figure 12

Papakura Participation Proportions (ITT) by Condition, Pre- and Post-intervention

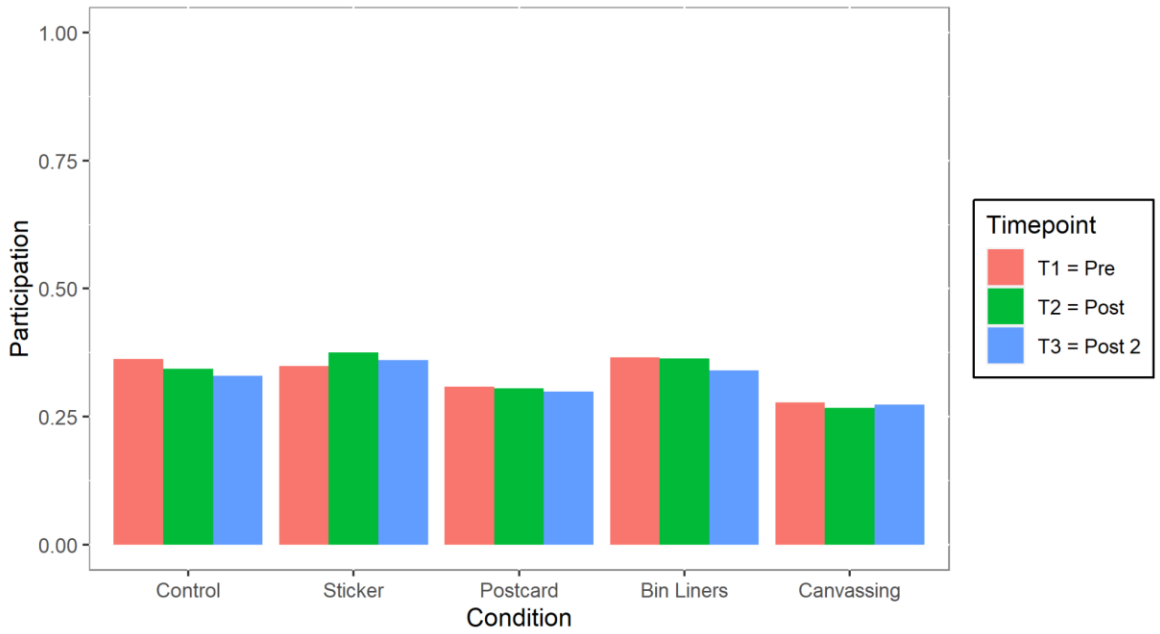


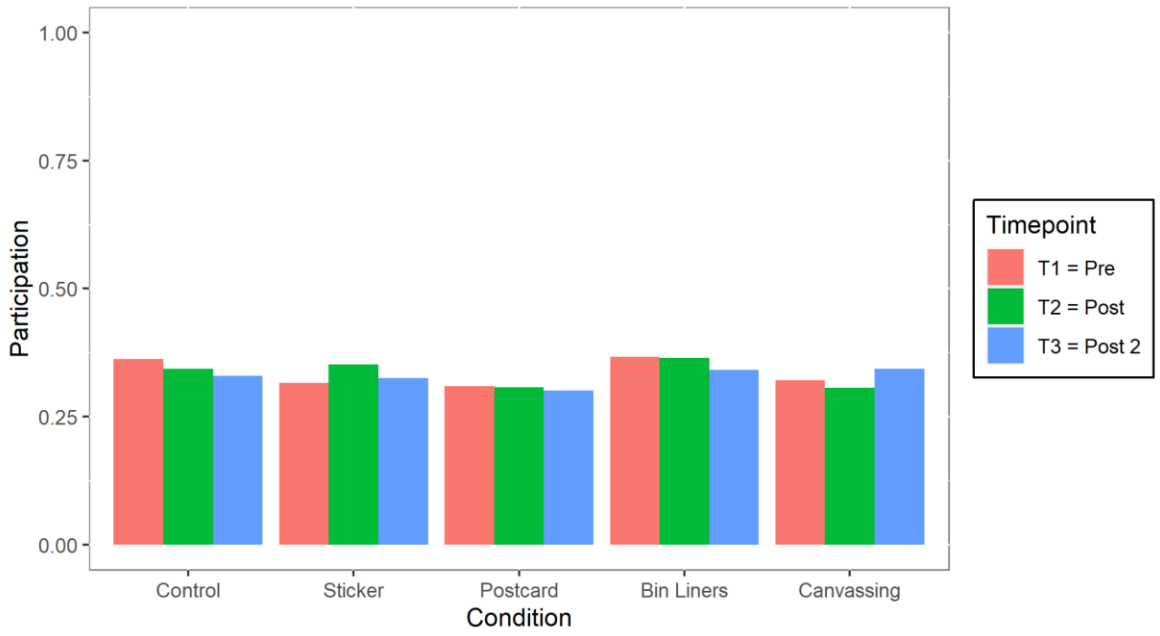
Table 11

Papakura Participation Proportions (ToT) by Condition, Pre- and Post-intervention

		T1 (Pre)		T2 (Post 1)		T3 (Post 2)	
Condition	Households	*n*	Participation proportion	*n*	Participation proportion	*n*	Participation proportion
Control	510	185	0.36	175	0.34	168	0.33
Sticker	332	105	0.32	117	0.35	108	0.33
Postcard	488	151	0.31	150	0.31	147	0.30
Bin Liners	477	175	0.37	174	0.36	163	0.34
Canvassing	134	43	0.32	41	0.31	46	0.34

Figure 13

Papakura Participation Proportions (ToT) by Condition, Pre- and Post-intervention



The results from both the ITT and ToT data show a pattern of declining participation between T1 and T2 (with the exception of the sticker prompt condition, which saw an increase). Changes between T1 and T3 show a similar pattern, however, in the ToT analysis,

participation in the canvassing condition appears to increase. We then statistically tested whether any of these changes support our hypothesised predictions.

3.6. Regression analysis for Papakura participation

We hypothesised that in the Papakura field experiment, participation in the food scraps collection service would show a greater increase post-intervention (relative to pre-intervention) in the bin liners (H1a), postcard (H1b), canvassing (H1c) and sticker prompt (H1d) conditions than in the control condition. To test Hypotheses 1a–1d we used the same model structure that we used to test Hypotheses 2a–2d (see Section 3.2), but with participation as the outcome variable. The results for both the ITT and ToT regressions for Papakura participation are presented in Table 12.

Table 12

Regression Findings for Papakura Food Scraps Participation

Parameter	Intention to Treat				Treatment on Treated			
	Estimate	Est.Error	Q2.5	Q97.5	Estimate	Est.Error	Q2.5	Q97.5
Intercept	-2.77	0.47	-3.72	-1.85	-2.61	0.52	-3.61	-1.54
TimepointT2	-0.22	0.25	-0.70	0.27	-0.24	0.25	-0.74	0.24
TimepointT3	-0.51	0.24	-0.98	-0.03	-0.49	0.24	-0.96	-0.01
CONDITIONSticker	0.14	0.66	-1.13	1.50	-0.30	0.70	-1.70	1.08
CONDITIONPostcard	-0.27	0.67	-1.61	1.03	-0.32	0.69	-1.69	1.05
CONDITIONBinLiners	0.36	0.68	-0.98	1.71	0.34	0.69	-1.05	1.67
CONDITIONCanvassing	-0.85	0.67	-2.16	0.49	-0.27	0.75	-1.72	1.23
TimepointT2:CONDITIONSticker	0.64	0.35	-0.07	1.31	0.77	0.39	0.02	1.54
TimepointT3:CONDITIONSticker	0.67	0.34	-0.01	1.35	0.53	0.38	-0.21	1.30
TimepointT2:CONDITIONPostcard	0.10	0.36	-0.59	0.80	0.16	0.36	-0.56	0.86
TimepointT3:CONDITIONPostcard	0.24	0.36	-0.46	0.94	0.27	0.36	-0.44	0.94
TimepointT2:CONDITIONBinLiners	0.17	0.35	-0.53	0.85	0.19	0.36	-0.50	0.90
TimepointT3:CONDITIONBinLiners	0.01	0.35	-0.67	0.70	-0.01	0.35	-0.68	0.69
TimepointT2:CONDITIONCanvassing	-0.07	0.37	-0.82	0.67	-0.22	0.52	-1.23	0.78
TimepointT3:CONDITIONCanvassing	0.34	0.37	-0.37	1.07	0.72	0.50	-0.22	1.72

Note:

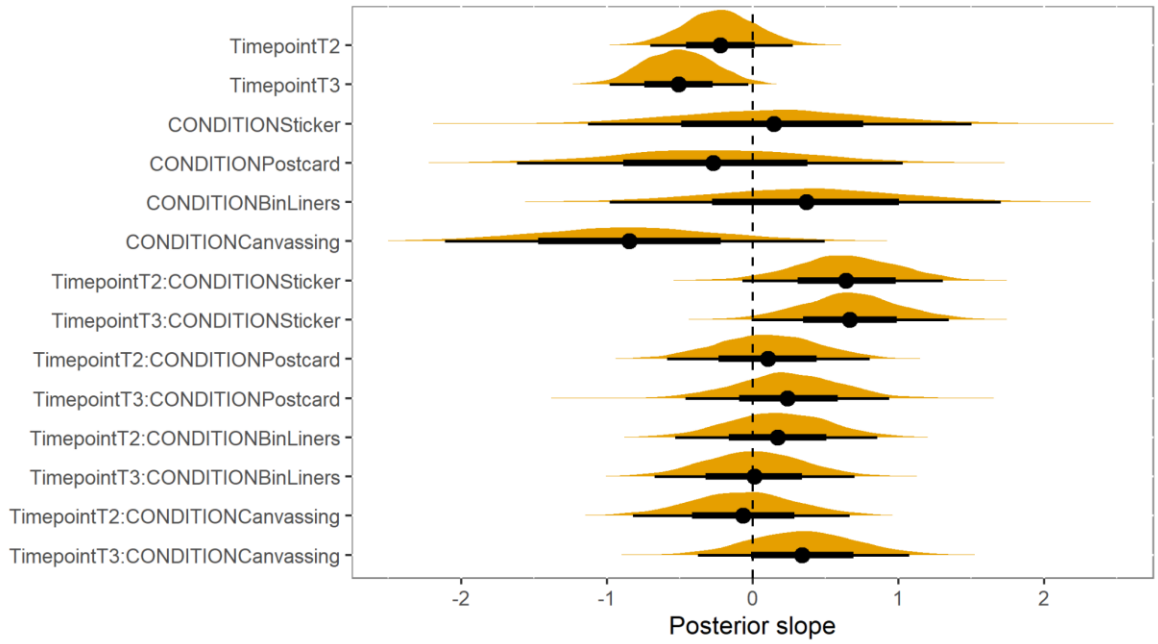
Table presents regression findings from repeated measures logistic regression models, with random effects for household and street cluster. Coefficients are on the log odds scale for ITT (orange) and ToT (blue).
Q2.5 and Q97.5 refers to Bayesian 95% Credible Intervals.

The ITT analysis found no unambiguous effects of the predictors on participation. Two of the interaction parameters showed marginal effects, since there is a slight overlap with zero in the 95% credible interval (T2 and sticker: log odds = 0.64, 95% CI [-0.07, 1.31]; T3 and sticker: log odds = 0.67, 95% CI [-0.01, 1.35]. In the ToT analysis there is clearer evidence of an interaction effect for the sticker at T2 (log odds = 0.77, 95% CI [0.02, 1.54]), though notably the effect size is very similar to the ITT analysis and the posterior distributions of the T2:sticker parameter in the two models are almost identical (see Figure A3, in Appendix G). This means that the pre- versus post-intervention change was greater in the sticker prompt condition compared to the change in the control condition, in the ToT analysis and probably also in the ITT analysis. Specifically, in the ToT analysis, between T1 and T2 in the control condition, there was a 21% decrease in the odds of participating (posterior mean OR = exp (-0.24) = 0.79). In contrast, in the sticker prompt condition there was a 70% increase in the odds of participating (OR = exp (-0.24 + 0.77) = 1.70).

Figures 14 and 15 show the posterior distributions of the parameters in the ITT and ToT models respectively, and illustrate their similarities between models. It is of note that the distributions for the T2:sticker parameter in the ITT and ToT models overlap significantly and that the former shows only a slight overlap with zero in the credible interval. Also of note is the effect for canvassing at T3 in the ToT model, which overlaps with zero but is still notable when compared to the postcard, bin liners and control parameters.

Figure 14

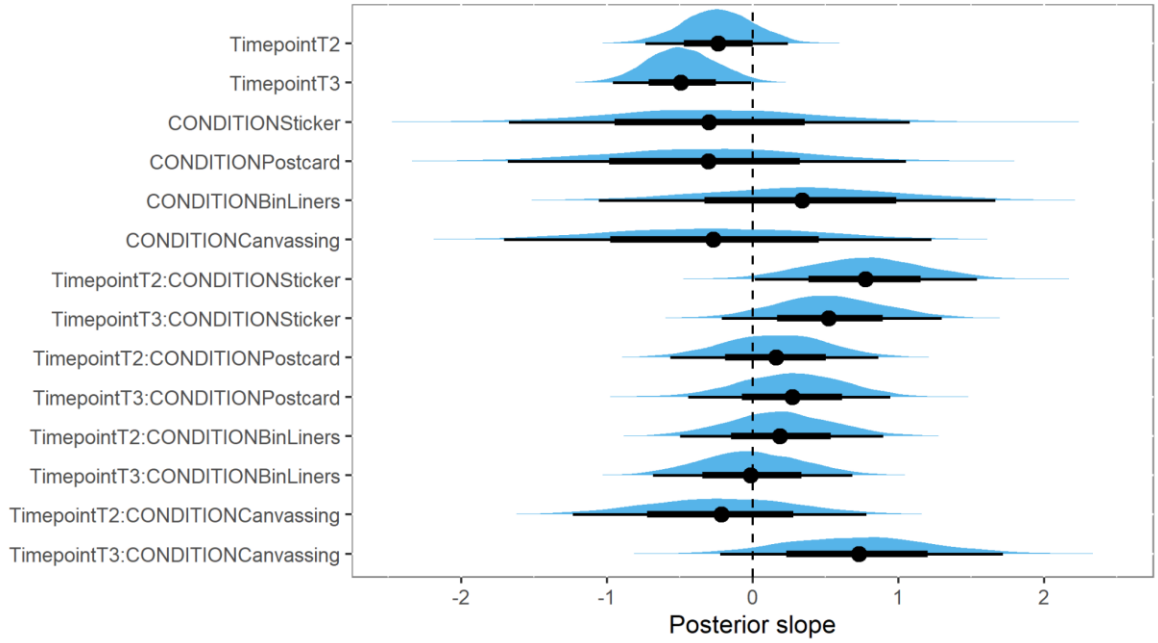
Posterior Distribution of Model Parameters for Papakura ITT Participation Analysis



Note: Points are median posterior estimates, thick black lines are 66% CIs, and thin black lines are 95% CIs.

Figure 15

Posterior Distribution of Model Parameters for Papakura ToT Participation Analysis



Note: Points are median posterior estimates, thick black lines are 66% CIs, and thin black lines are 95% CIs.

As with set out, we also conducted follow-up analyses for participation. These compared the results from the original analyses given in Table 12, with those from models that excluded the 25 households in Papakura that received the sticker prompt treatment on Monday 12 September (as discussed in Section 2.8). The results for both follow-up models were very similar to those given in Table 12, however, in the follow-up ToT analysis there is a slight overlap with zero in the credible interval for the sticker effect at T2. The implications of this will be discussed in the Discussion chapter. See Tables A6, A7, and A8 in Appendix G for follow-up analysis results.

3.7. Descriptive Statistics for North Shore Participation

We repeated the same analyses as reported in Section 3.4, for the participation outcome variable. Tables 13 and 14 present descriptive statistics for participation in the North Shore trial for the ITT analysis and ToT analysis, respectively. This information is also illustrated in Figures 16 and 17.

Table 13

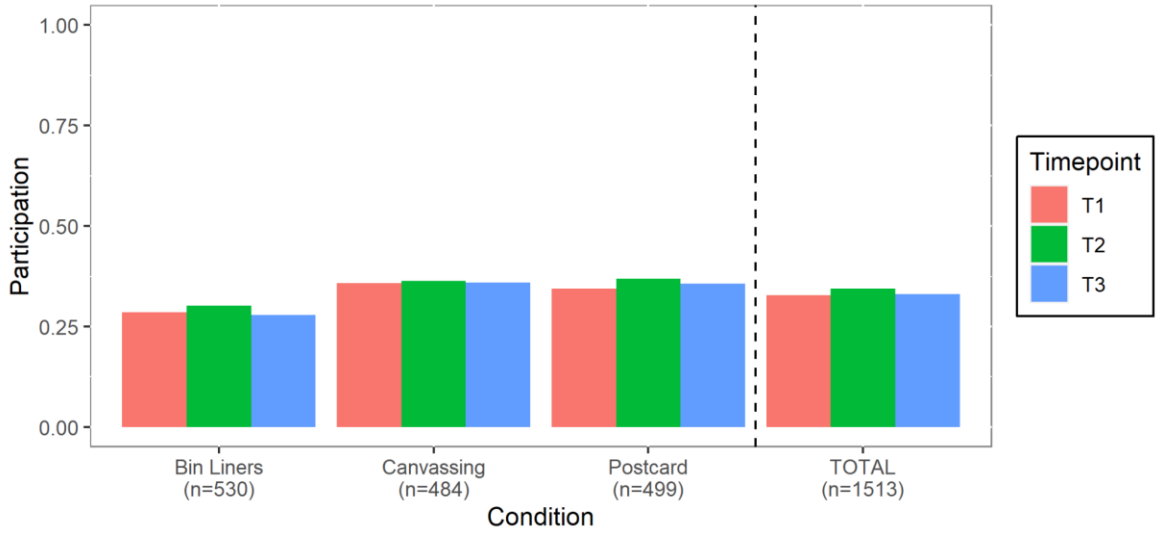
North Shore Participation Proportions (ITT) by Condition, Pre- and Post-intervention

		T1 (Pre)		T2 (Post 1)		T3 (Post 2)	
Condition	Households	*n*	Participation proportion	*n*	Participation proportion	*n*	Participation proportion
Bin Liners	530	151	0.28	160	0.30	148	0.28
Postcard	499	172	0.34	184	0.37	178	0.36
Canvassing	484	173	0.36	176	0.36	174	0.36
Total	1513	496	0.33	520	0.34	500	0.33

Note: The Bin Liners condition refers to the "bin liners-only" treatment; Postcard refers to the "postcard plus bin liners" treatment; Canvassing refers to the "canvassing plus bin liners" treatment and Total refers to all conditions combined.

Figure 16

North Shore Participation Proportions (ITT) by Condition, Pre- and Post-intervention



Note: Participation proportions for the three conditions are shown for T1 (Pre), T2 (Post 1) and T3 (Post 2) time points. Participation is calculated as the proportion of households setting out their food scraps bin at least once during a three-consecutive-week period.
n refers to number of households.

Table 14

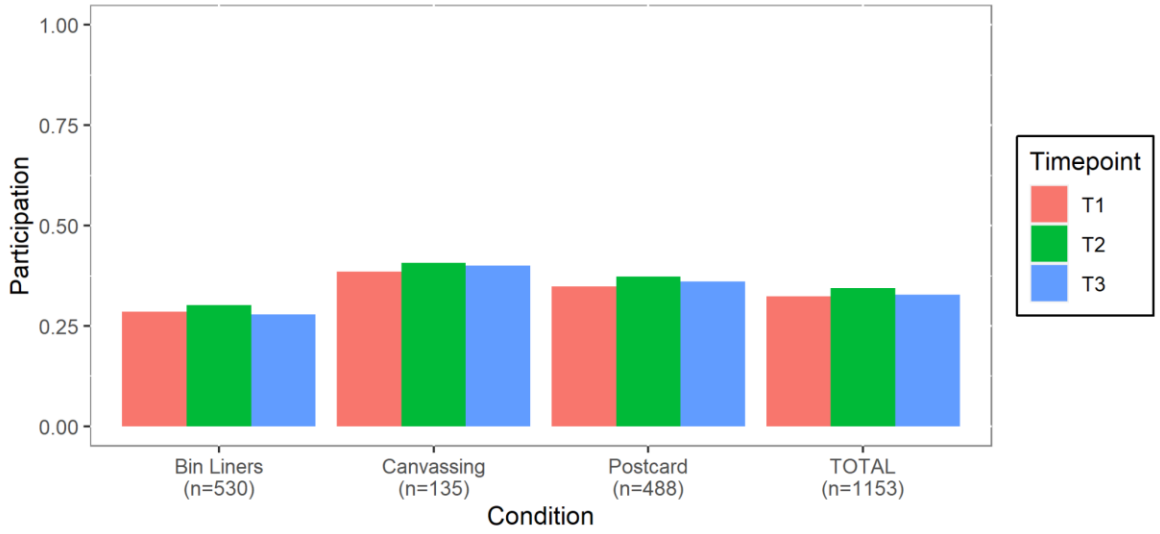
North Shore Participation Proportions (ToT) by Condition, Pre- and Post-intervention

		T1 (Pre)		T2 (Post 1)		T3 (Post 2)	
Condition	Households	*n*	Participation proportion	*n*	Participation proportion	*n*	Participation proportion
Bin Liners	530	151	0.28	160	0.30	148	0.28
Postcard	488	170	0.35	182	0.37	176	0.36
Canvassing	135	52	0.39	55	0.41	54	0.40
Total	1153	373	0.32	397	0.34	378	0.33

Note: The Bin Liners condition refers to the "bin liners-only" treatment; Postcard refers to the "postcard plus bin liners" treatment; Canvassing refers to the "canvassing plus bin liners" treatment and Total refers to all conditions combined.

Figure 17

North Shore Participation Proportions (ToT) by Condition, Pre- and Post-intervention



Note: Participation proportions for the three conditions are shown for T1 (Pre), T2 (Post 1) and T3 (Post 2) time points. Participation is calculated as the proportion of households setting out their food scraps bin at least once during a three-consecutive-week period.
n refers to number of households.

The results given above suggest that there was an overall increase in participation between T1 and T2, and possibly also between T1 and T3. They also suggest that participation in the postcard condition increased between T1 and T2 and T1 and T3, but that in the canvassing and bin liner-only conditions an increase may have only occurred between T1 and T2.

3.8. Regression analysis for North Shore participation

As with set out in the North Shore field experiment, there is no no-treatment control condition against which to evaluate intervention effects over time. We first tested whether food scraps participation increased overall in the post-intervention time points (T2 and T3) relative to T1 (H3a). To do this, we used the same modelling framework as above, with support for Hypothesis 3a indicated by a significant positive effect of the post-intervention indicators (T2 and T3). The results of both ITT and ToT analyses are presented in Table 15.

Table 15

Regression Findings for North Shore Food Scraps Participation

Parameter	Intention to Treat				Treatment on Treated			
	Estimate	Est.Error	Q2.5	Q97.5	Estimate	Est.Error	Q2.5	Q97.5
Intercept	-3.33	0.44	-4.21	-2.44	-3.30	0.47	-4.19	-2.36
TimepointT2	0.37	0.17	0.03	0.70	0.49	0.20	0.10	0.89
TimepointT3	0.06	0.17	-0.28	0.41	0.09	0.20	-0.28	0.48

Note:

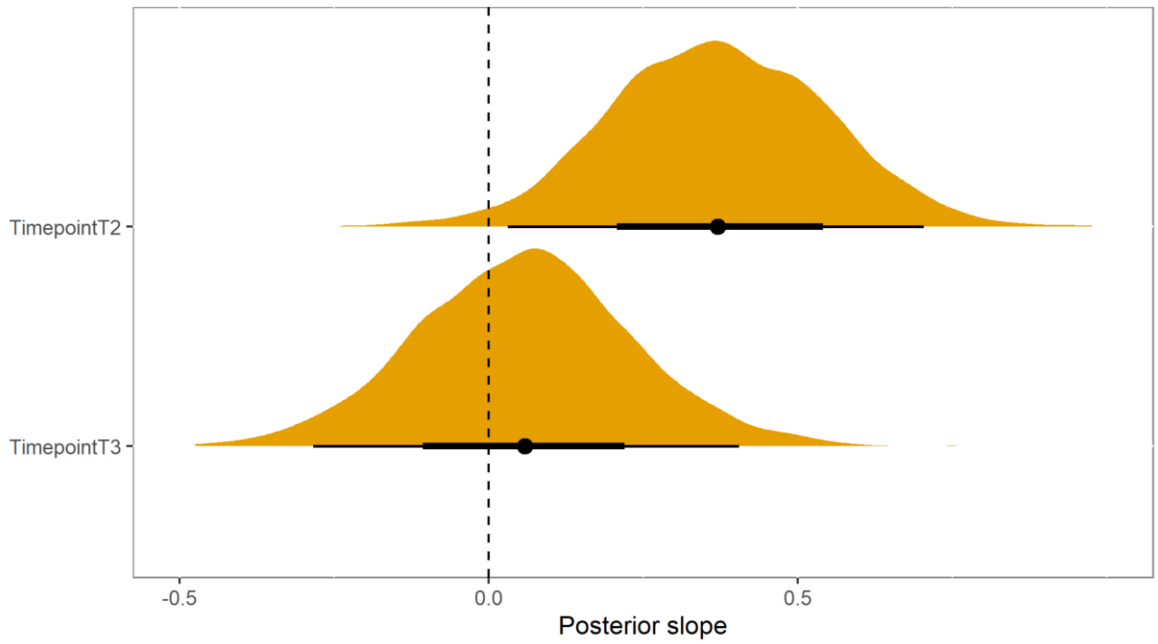
Table presents regression findings from repeated measures logistic regression models, with random effects for household and street cluster. Coefficients are on the log odds scale for ITT (orange) and ToT (blue).
Q2.5 and Q97.5 refers to Bayesian 95% Credible Intervals.

We found support for H3a from both ITT and ToT analyses; participation increased in the post-intervention period at T2 compared to T1 (for ITT, log odds = 0.37, 95% CI [0.03, 0.70]; for ToT, log odds = 0.49, 95% CI [0.10, 0.89]). However this effect was not sustained longer term, as it has weakened by T3 in both models. For the ITT analysis, the odds of participating at T2 compared to T1 were increased by 45% (posterior mean OR = exp (0.37) = 1.45), while for the ToT analysis, the odds of participating at T2 increased by 63% (OR = exp (0.49) = 1.63) compared to at T1.

Figures 18 and 19 show the posterior distributions of the model parameters in the ITT and ToT analyses, respectively. They show a slightly stronger effect for T2 in the ToT model. It is also evident that the T2 effect has disappeared by T3, as the credible interval is much more evenly distributed around zero in both models.

Figure 18

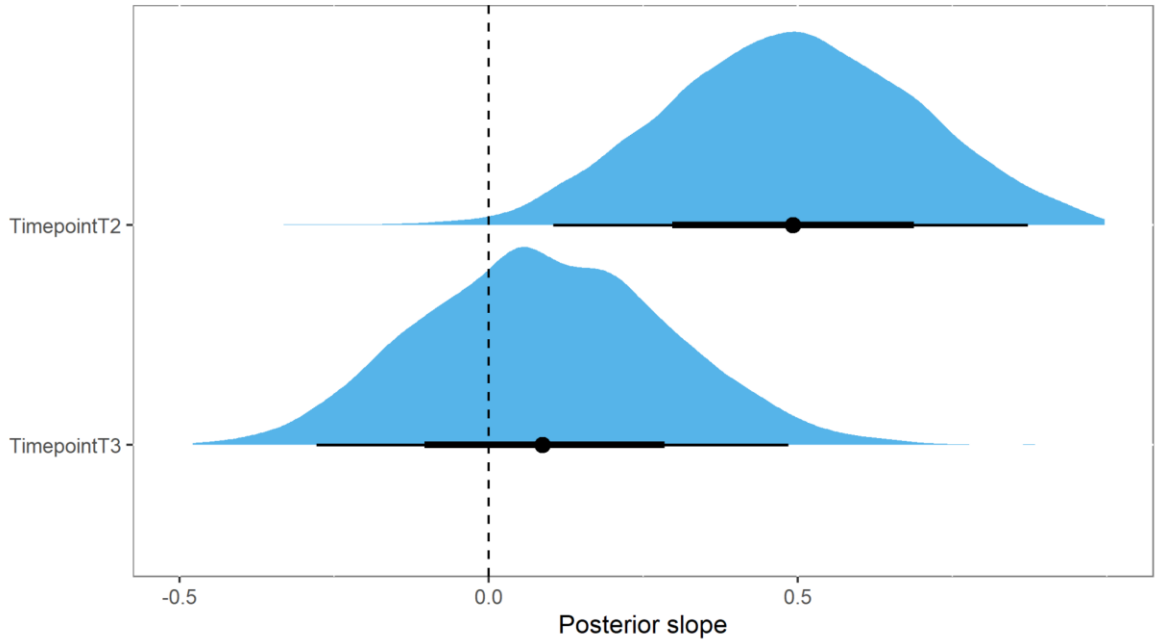
Posterior Distribution of Model Parameters for North Shore ITT Participation Analysis



Note: Points are median posterior estimates, thick black lines are 66% CIs, and thin black lines are 95% CIs.

Figure 19

Posterior Distribution of Model Parameters for North Shore ToT Participation Analysis



Note: Points are median posterior estimates, thick black lines are 66% CIs, and thin black lines are 95% CIs.

We next tested whether participation is higher post- versus pre-intervention in the postcard plus bin liners (H3b) or canvassing plus bin liners (H3c) conditions when compared to the bin liners-only condition – that is, whether the postcard or canvassing treatments have a positive effect on participation over and above the effects of providing free bin liners. The findings from the ITT and ToT analyses are presented in Table 16 and the posterior distributions for the parameters are illustrated in Figures 20 and 21.

Table 16

Regression Findings for North Shore Food Scraps Participation

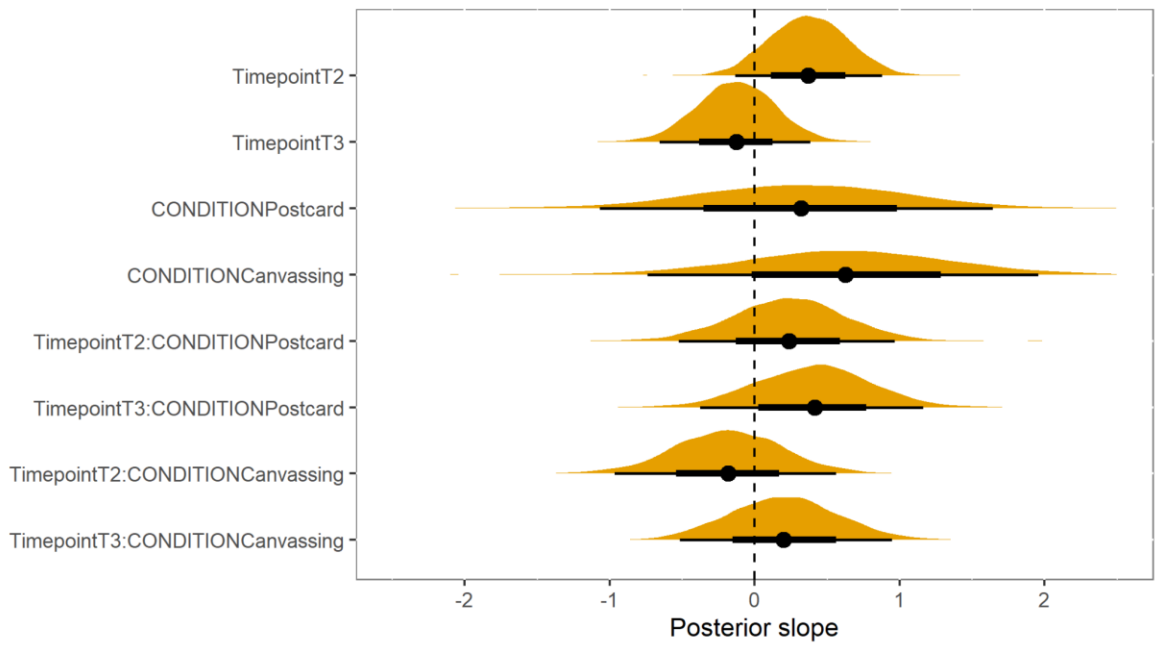
Parameter	Intention to Treat				Treatment on Treated			
	Estimate	Est.Error	Q2.5	Q97.5	Estimate	Est.Error	Q2.5	Q97.5
Intercept	-3.71	0.59	-4.81	-2.52	-3.68	0.61	-4.83	-2.43
TimepointT2	0.37	0.26	-0.13	0.88	0.40	0.27	-0.13	0.92
TimepointT3	-0.13	0.27	-0.65	0.39	-0.10	0.27	-0.63	0.43
CONDITIONPostcard	0.32	0.70	-1.07	1.65	0.44	0.70	-0.96	1.81
CONDITIONCanvassing	0.64	0.70	-0.74	2.01	0.67	0.80	-0.91	2.21
TimepointT2:CONDITIONPostcard	0.23	0.38	-0.52	0.97	0.23	0.39	-0.52	1.00
TimepointT3:CONDITIONPostcard	0.40	0.39	-0.37	1.16	0.40	0.39	-0.36	1.15
TimepointT2:CONDITIONCanvassing	-0.19	0.38	-0.96	0.56	0.16	0.55	-0.91	1.27
TimepointT3:CONDITIONCanvassing	0.20	0.37	-0.51	0.95	0.42	0.54	-0.64	1.50

Note:

Table presents regression findings from repeated measures logistic regression models, with random effects for household and street cluster. Coefficients are on the log odds scale for ITT (orange) and ToT (blue).
Q2.5 and Q97.5 refers to Bayesian 95% Credible Intervals.

Figure 20

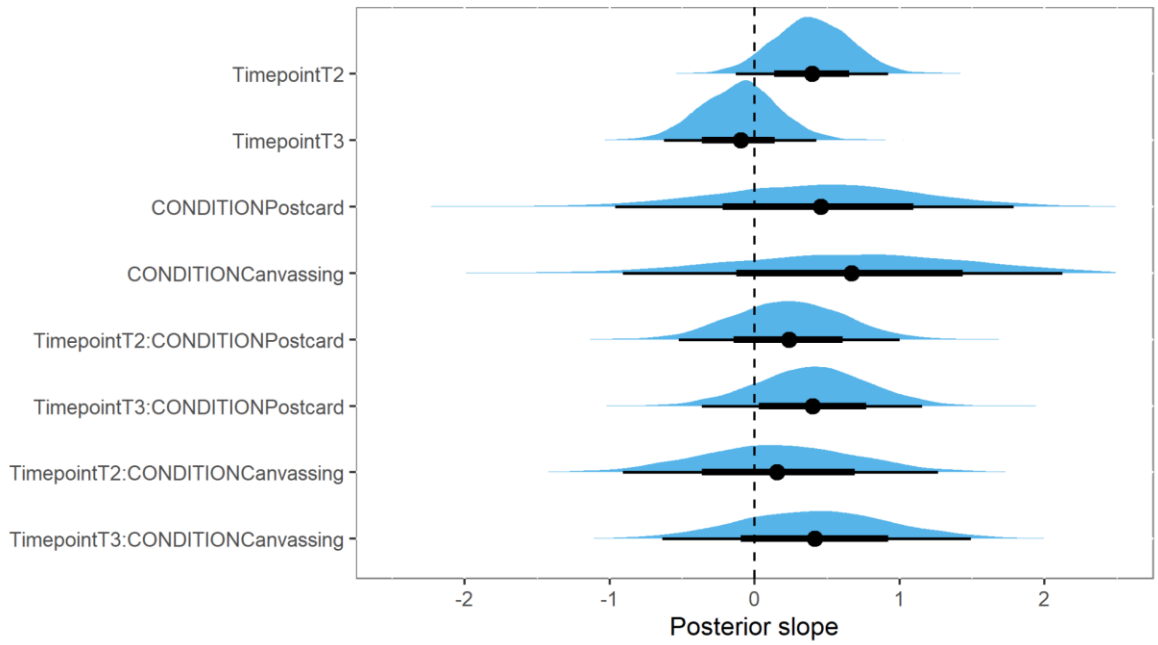
Posterior Distribution of Model Parameters for North Shore ITT Participation Analysis



Note: Points are median posterior estimates, thick black lines are 66% CIs, and thin black lines are 95% CIs.

Figure 21

Posterior Distribution of Model Parameters for North Shore ToT Participation Analysis



Note: Points are median posterior estimates, thick black lines are 66% CIs, and thin black lines are 95% CIs.

There was no evidence of support for H3b or H3c, as the results in Table 16 and Figures 20–21 indicate. There were no meaningful effects between time point and any of the treatment conditions, meaning that we have no evidence that any of the interventions were effective, in both types of analysis.

As with the set out variable, we conducted follow-up analyses comparing the results in Tables 15 and 16 with those from models that excluded the households in Milford that have a Wednesday collection day (and which were subject to the collection day discrepancy, as discussed in Section 2.8). For the follow-up ITT model testing H3a, the credible interval for the T2 parameter showed a slight overlap with zero; this was not the case in the follow-up ToT model where the effect of T2 remained unambiguous. The implications of this will be discussed in the Discussion chapter. The follow-up models testing H3b–3c gave similar estimates to those in the original models; that is, none of the predictors showed evidence of an effect. See Tables A9 and A10 in Appendix G for follow-up analysis results.

4. Discussion

4.1. General overview

This study aimed to test and evaluate four different behaviour-change tools designed to increase usage of Auckland Council's food scraps collection service through two randomised controlled field experiments in different areas of Auckland: Papakura and the North Shore. In Papakura, we found that of the four different types of interventions tested (sticker prompts, postcards, free bin liners, and canvassing), only the sticker prompt and postcard conditions showed a measurable increase in food scraps bin set out post-intervention, when compared to a control. On the North Shore, we lacked a post-intervention control, but found that across three intervention conditions (free bin liners plus postcards, free bin liners plus canvassing, and free bin liners-only) food scraps bin set out increased post-intervention, and the postcard condition showed a measurable increase in set out post-intervention, when compared to a bin

liners-only condition. We found similar effects when considering food scraps recycling participation (over three weeks) rather than set out, however there was no evidence that the postcard condition was any different to the bin liners-only condition.

4.2. Did any of our treatments increase set out or participation in Papakura, post-intervention?

Yes. In the Papakura trial it was predicted that compared to a control condition, participation (H1) and set out (H2) in the sticker prompt (H1a, H2a), postcard (H1b, H2b), bin liner (H1c, H2c) and canvassing (H1d, H2d) conditions would increase in the post-intervention period, relative to pre-intervention. We found a clear and consistent effect of the sticker prompt and postcard treatments at T2 and T3, when considering both Intention-to-Treat (ITT) and Treatment-on-the-Treated (ToT) samples, on set out. There was less support for any treatment effects on participation, with the exception of an effect of the sticker at T2 in Papakura, when considering households that received the sticker prompt (ToT). This means that we found clear support for H2a, H2b and some support for H1a but no clear support for the other predicted effects. Importantly, the findings that relative to controls, the sticker prompt treatment increased set out and participation, and the postcard treatment increased set out in Papakura, are robust across a range of analyses. The difference in pre- versus post-intervention change was markedly greater in the sticker prompt and postcard conditions compared to the control, which during these same time period comparisons (T1–T2 and T1–T3), experienced a decline in the odds of both set out and participation.

4.3. Did any of our treatments increase set out or participation in the North Shore, post-intervention?

Yes. It was predicted that there would be an overall effect of the interventions on participation (H3a) and set out (H4a) in the post-intervention period. And that compared to the bin liners-only condition, participation (H3) and set out (H4) in the postcard plus bin liners (H3b, H4b) and canvassing plus bin liners (H3c, H4c) conditions would increase in the post-intervention

period, relative to pre-intervention. We found a positive effect at T2 and T3 when testing for an overall effect of the interventions, for set out, and a positive effect at T2 for participation. This was the case in both ITT and ToT analyses. We also found a clear and consistent effect of the postcard treatment at T3, when considering both ITT and ToT, on set out. This was not the case when participation was the outcome variable. This means we have evidence of support for H3a, H4a and H4c but no clear support for the other predicted effects.

Due to the food scraps collection day discrepancy, which was discussed in Section 2.8, where up to 40 food scraps bins were not counted in Milford Wednesday households' set out count during week 3 of T2, we ran a follow-up analysis where all households with a Milford Wednesday collection day were removed. The analysis found that the incident did not change our findings in relation to set out or participation, in any meaningful way (estimates and credible intervals are very similar in original and follow-up models – see Tables 8 and 9 in Section 3.4 for original set out model results, and Tables A4 and A5 in Appendix G for follow-up set out model results). In follow-up analyses for the participation outcome, testing H3b–3c, there remained no evidence of effects for any of the coefficients (see Table A10 in Appendix G). Follow-up models testing H3a (whether there was an overall pre- versus post-intervention effect) also showed only minor changes in the estimates and credible intervals. In the follow-up ITT analysis, the credible interval is wider than in the original ITT analysis, and there is a very slight overlap with zero (see Table 15 in Section 3.8 for the original participation model results and Table A9 in Appendix G for follow-up model results). However, the posterior distributions almost entirely overlap, and the estimates are basically identical (original ITT model: log odds = 0.37, 95% CI [0.03, 0.70]; follow-up ITT model: log odds = 0.39, 95% CI [-0.02, 0.79]; see figure A5, Appendix G). For these reasons we do not believe that the follow-up model results warrant changing our original conclusion that H3a is supported. The credible interval is very close to zero in both original and follow-up (ITT) models. It is expected that the credible interval would become wider with a reduction in power since the sample size has been reduced by approximately 20%. Further follow-up exploratory analysis suggests that

Milford has higher average participation at all time periods, than the other suburbs, which may help to explain the change in results (see Table A11 in Appendix G). If anything, the fact that the estimate remained virtually unchanged shows some robustness in the face of a relatively significant loss in statistical power.

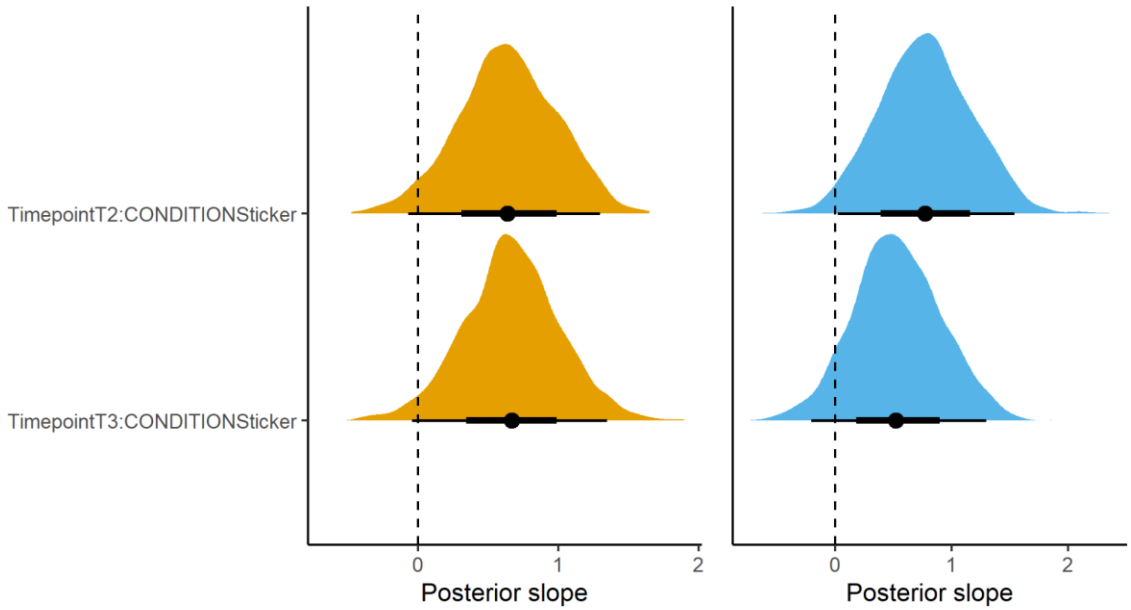
4.4. The efficacy of the sticker prompt intervention

The sticker prompt proved to have a clear and consistent effect on food scraps recycling set out, resulting in a meaningful increase immediately post-intervention at T2. Although the positive effect becomes attenuated over time, the difference between the control and the sticker prompt condition is still present at T3 (seven weeks from the end of the intervention delivery period). The size of the increase in set out was also meaningful: amongst those households that received a refuse bin sticker (ToT), the odds that they would set out their food scraps bin for collection at T2 increased by 48% when compared to T1. At T3, the effect had lessened somewhat (to a 3% decrease relative to T1), however, the effect was still seen against the control group. We may claim with 95% probability, that the T1–T2 and T1–T3 changes in set out were greater in the sticker prompt condition than in the control condition. The effects were not only seen amongst the households that received the sticker but amongst the sticker prompt treatment group as a whole (ITT); the average increase in the odds of food scraps bins being set out amongst the sticker treatment group at T2 was 28% compared to T1. At T3 the odds had decreased slightly relative to T1, however, the T1–T3 change was still markedly greater than the change seen in the control group. This indicates that any effects of stickering refuse bins are measurable even when untreated households within the treatment area are included. There was a substantial difference between the sample size for the sticker prompt treatment groups for ITT and ToT. While the former had n=496, the latter had only n=332 (one-third fewer households). It is therefore promising for the sticker prompt intervention that the effect was also seen when considering the ITT sample.

Not only did we see evidence that the sticker prompt led to an increase in set out, there is also evidence that it led to increased participation – that is, encouraged additional households to participate in the service. There was evidence of a clear sticker effect at T2 (*treated* households in the sticker prompt condition experienced a 70% increase in the odds of participating at T2 when compared to T1). It is likely, though not conclusive, that the sticker effect on participation is also present in the ITT analysis, since the estimates for the interaction of the sticker with both T2 and T3 are positive, posterior distributions have substantial overlap and the credible intervals are very close to zero. This is illustrated in Figure 22, where it is evident that there is a significant overlap in the posterior distributions for the T2:sticker parameter (top left) and T3:sticker parameter (bottom left) in the ITT analysis, and the T2:sticker parameter (top right) in the ToT analysis (see also Table 12).

Figure 22

Posterior Distribution of Sticker Interaction Parameters for ITT and ToT Participation Models



Note: Figure shows parameters from ITT model for sticker at T2 (top left) and T3 (bottom left) and ToT model for sticker at T2 (top right) and T3 (bottom right). Points are median posterior estimates, thick black lines are 66% CIs, and thin black lines are 95% CIs.

We ran follow-up analyses for set out and participation that excluded set out and participation data for the 25 households that received their refuse bin sticker on Monday 12 September (week 1 of the T2 monitoring period), because we wanted to explore whether excluding these households would produce a stronger effect. The results from the follow-up analyses for set out did not change the findings in any important way (see Section 3.2). The follow-up participation analyses, also produced only slightly different results to the original analyses. In the follow-up ToT model there is a slight overlap with zero in the credible interval for the T2:sticker parameter (see Tables A6 and A8, Appendix G). We do not think that this affects the conclusions we have drawn. Firstly, the estimates for the relevant parameter in both models are positive and very similar: log odds = 0.77 versus log odds = 0.71 for the original ToT model and follow-up ToT model, respectively. Moreover, the posterior distributions and credible intervals for the two models almost entirely overlap (see Figure A6, Appendix G). When comparing the follow-up ToT set out model (Table A3, Appendix G), in which the 95% credible intervals did not overlap with zero, with the follow-up ToT participation model (Table A6, Appendix G), the estimates for the T2:sticker coefficient are both positive and similar. Together, these results suggest that the sticker prompt had a positive effect on participation, at least among households that received the treatment.

These findings are in line with previous research from the UK which found (Shearer et al., 2017) and suggests (WRAP, 2021b) that a simple visual prompt in the form of a proscriptive reminder sticker, can be an effective tool for increasing food scraps recycling. The study which used a sticker design that influenced our own design, found a positive and sustained effect of the sticker prompt on the weight of food waste collected (Shearer et al., 2017). Although the outcome variable in the UK study differed from ours, the positive trend for capture of food waste found in both studies is consistent. The WRAP pilots (WRAP, 2021b) also concluded that refuse bin stickers are a promising option to increase food waste recycling, bearing in mind the methodological limitations of the study, discussed in Section 1.8.7. Based on increased participation and weight of food waste collected, which was associated with study

areas where a refuse bin sticker was trialled, WRAP's recommendations were to include a refuse bin sticker as part of a package of options for raising participation in existing food waste collection services.

The fact that we did not see unequivocal evidence in support of the sticker prompt treatment on participation (as opposed to set out) could be because stickers help motivate households that already put out their bins regularly to do so more often (that is, increased set out), but they may not be as effective in getting households that do not participate to start participating. It could also be explained by lack of statistical power. Although a power calculation was done prior to experimental design where it was determined that we should aim for at least n=400 in each treatment condition, the power analysis was generic and not tailored to the specific type of statistical analysis we performed. Effect sizes were also smaller than we expected. We may not have had sufficient power to detect some of these small effects. With set out we had a lot more data, given that there are observations for each household every week for three consecutive weeks, rather than one observation over a three-week period during our three monitoring rounds. This extra power could explain why a clear effect was observed for set out but not for participation.

Our findings showing the effect of the sticker prompt, together with prior work examining the effects of visual prompts, supports the prediction from nudge theory that, more generally, visual prompts can act as a reminder to perform the desired behaviour. They may work as a reminder because of their timing (influencing people at the moment when they are disposing of their food waste by drawing attention to a message that they may not previously have had the time or motivation to allocate cognitive resources to) and simplicity (very little attention and cognitive load is required to look at and register the message of the visual prompt). These two factors mean that the sticker prompt partially operates on unconscious cognitive processes, to deter people from the behaviour, without requiring much (or any) deliberative thinking. They

are likely also effective because they are seen in close proximity to the opportunity to perform the behaviour (Sussman et al., 2013).

4.5. The efficacy of the postcard intervention

Like the sticker prompt treatment, the social norms postcard also proved to have a clear and consistent effect on food scraps bin set out, when compared to the control condition. In Papakura, the postcard treatment resulted in an increase in set out at T2. The effect was attenuated, but still measurable, up to seven weeks later at T3. This was the case when both ToT and ITT samples were considered. The size of the effect was smaller than that seen in the sticker prompt condition, but was present nevertheless: among households in the ToT analysis, the odds that they would set their food scraps bin out for collection at T2 increased by 20% when compared to T1. At T3, the odds of set out had decreased by 8% relative to T1, but there was still an effect relative to the control condition. The effect was also present when considering the postcard treatment group as a whole (ITT); the average increase in the odds of a food scraps bin being set out amongst the postcard treatment group at T2 was 19% compared to at T1. At T3 the effect of the postcard was still present, but the odds had decreased slightly to 8% relative to T1. Most importantly, these effects were seen against a control condition, which between T1 to T2 saw around a 20% decrease in the odds of set out and declining further at T3 to approximately 40% lower, relative to T1. In contrast to set out, there is no evidence of an effect of the postcard on participation for either ITT or ToT analyses. Figures 14 and 15 illustrate that the credible intervals for the effect of the postcard treatment at both T2 and T3 substantially overlap with zero.

We also found evidence for the postcard's efficacy at increasing set out in the North Shore trial. In the North Shore, the size of the effect on set out was relatively large (the largest effects seen in any of our models in either trial). When comparing the postcard plus bin liners treatment condition to a quasi-control condition which received only bin liners, we saw evidence of an additional effect (on top of bin liners), on set out at T3 when compared to T1,

for those receiving the postcard. At T3, the odds of set out for households who received a postcard had increased by 80% for ITT (83% for ToT), compared to T1. This is a very clear effect of the postcard at T3. Interestingly, in both ITT and ToT regressions, the effect at T2 was positive but not as strong, as the posterior distribution is centred closer to zero, with some overlap with zero in the credible interval. There is still a large amount of overlap between the posterior distributions for T2:postcard and T3:postcard (Figure 10 and Figure 11). During these two time period comparisons, the bin liners-only condition experienced a much smaller increase in the odds of set out. Unlike with the sticker condition, since almost all households in the postcard condition received a postcard, there was very little difference between the postcard condition sample sizes for ITT and for ToT; the former being *n*=492 and the latter *n*=488. This is a negligible difference, and therefore it is unsurprising that an effect which was seen in the ToT regression was also seen in ITT.

It is interesting that the effect of the postcard on set out was seen immediately post-intervention in Papakura but then attenuated, whereas in the North Shore trial, there is only clear evidence of an effect at T3. As noted, the differences between T2 and T3 effects in Papakura and the North Shore are not large and may be due to noise, which could explain this difference. In contrast to set out, there is no evidence to suggest that the postcard treatment produced participation outcomes any different from the control condition in Papakura, and the bin liners-only condition in the North Shore. This is the case for both ITT and ToT analyses. Figures 14, 15, 20 and 21 illustrate that the credible intervals for the effect of the postcard treatment at both T2 and T3 substantially overlap with zero. Thus, while postcards encouraged households to put their food scraps bins out more often, this did not appear to translate into *more* households putting their bins out.

The set out findings in relation to our social norms postcards are consistent with prior work showing that social norms messaging is effective at changing recycling behaviour. Results from several prior studies aimed at improving dry recycling or food waste recycling behaviour

have demonstrated that targeting descriptive and/or injunctive norms, can successfully improve participation, collection weights or diversion rates (see for example, Schultz, 1999, Linder et al., 2018, Flygansvær et al., 2021, Geislar, 2017, Nomura et al., 2011, Milford et al., 2015, Mertens & Schultz, 2020, and Czajkowski et al., 2019).[14] Some of these studies also found an effect on participation, specifically, which we did not. Our negative participation results are in line with one study, carried out in Peru that used different types of social norm messaging aimed at increasing participation in dry recycling, and which found that none of the four types of messages (including use of descriptive social comparisons with similar others) had any effect (Chong et al., 2013). While none of these studies use set out as an outcome measure, we may compare changes in set out and participation to get an indication of whether our interventions were primarily influencing existing participators or new participators. If set out increases post-intervention but participation does not, this is consistent with the explanation that existing participators are using the service more frequently but that new households are not participating. If participation increases post-intervention this is consistent with baseline non-participators now using the service.[15] Although we did not test for this, it is a reasonable assumption that our results are explained by our interventions primarily influencing existing participators. If this explanation is true, then we may have helped to avoid a boomerang effect in households that were already using the service at T1, by including an injunctive norm in the postcard.

The findings that the postcard increased set out but not participation are useful because they tell us that the postcard did work, however it was not effective at increasing the total number of participating households, many of whom will have been baseline non-participators. The discrepancy between set out and participation could be explained by failure of the norm messages to act on non-participators as well as they did on existing participators. Schultz

[14] None of the studies mentioned use set out as an outcome measure.
[15] It is not the only explanation. The same result could be seen if a similar number of T1 participators stopped participating at T2/T3, as there are non-participators at T1 who began participating at T2/T3 (and if overall there results in a slightly greater number of participators at T2/T3 than at T1).

(2014) has emphasised that social norm messages regarding pro-environmental behaviours do not unanimously work, but are sensitive to contextual factors. Carlson (2001) makes the case (in the context of dry recycling studies) that the significant effects that are seen may be positively associated with and dependent on existing pro-recycling attitudes found in the population, and that in a context lacking such attitudes, effects may not be present (pp.1281-1282). This is relevant to the current context. In Auckland and in New Zealand as a whole, food scraps recycling is not currently widespread; in fact, in Auckland it is limited to our two trial areas. In total, no more than 25,000 Aucklanders (1% of the population) is currently offered a food scraps collection service and for most of these households it has only been in place for four years. It is, therefore, not a widespread behaviour. Consequently, a widespread pro-food-scraps-recycling attitude has not yet formed in Auckland. It is reasonable to expect that current participators are more likely to have a positive attitude toward food scraps recycling, which non-participators do not share (Moussaoui et al., 2020), and that this could help to explain the discrepancy between the effect of the postcard on set out and participation.

Postcard wording could also help explain why participation did not increase. The descriptive norm message was not targeted at individual households or streets. It was deliberately chosen to be vague ("join thousands of your neighbours") since participation is relatively low (approximately 35% of households currently participate in the food scraps collection service, where 50% is considered good [WRAP, 2021c]), and we wanted to avoid explicitly pointing out to people that the target behaviour is not widespread (Cialdini, 2003; Cialdini et al., 2006). A number of studies related to recycling and waste diversion that use social norm appeals and that have shown positive effects, have used targeted feedback with more specific messaging for the descriptive norm. For example, the postcard used by Nomura et al. (2011) stated: "Did you know: X% of homes on A Street recycle their food waste. The average for the area is Y%" (with the contents of the card tailored to each street). To investigate how referent group specificity affects waste diversion outcomes, Mertens and Schultz (2020) compared descriptions of social normative feedback with four different referent groups that varied in how

specific they were, against information-only and no-treatment control conditions. The four referent group conditions a household was compared to, included either: specific (comparing your household to your five closest neighbours); exemplary (comparing your household with the best recyclers in the city); generic (comparing your household to similar households in your city); or statewide (comparing your household to a statewide waste diversion target). They found that all forms of personalised normative feedback increased recycling rates in the short-term, when compared to the information-only and control conditions, however differences between the four feedback conditions were not statistically significant (Mertens & Schultz, 2020).[16] Regardless, all norm descriptions used in the Mertens and Schultz (2020) study used personalised feedback for each household. This differs from our own study where no feedback was given, and a non-personalised descriptive norm message was used for all households in the study.

Finally, as with the discussion of the sticker prompt in Section 4.4, a lack of statistical power could also help to explain why no effect of the postcard was seen for participation. With set out there is a significantly greater amount of data (three times as many observations). Following Cohen (1988), an Odds Ratio (OR) less than 1.44 is considered equivalent to a very small effect size (Ben-Shachar et al., n.d.). In the participation models, given that effect sizes are very small for the parameters involving the postcard treatment (for example, in the Papakura participation ITT regression the OR = 0.89), there simply may not have been enough statistical power to detect an effect when analysing participation.

4.6. The efficacy of the free bin liners intervention

The free bin liners intervention did not prove to be effective in changing food scraps recycling behaviour in Papakura. There is no evidence that the free bin liners condition was different to

[16] Despite a *p*-value ($p = .090$) for the relevant interaction parameter that was not statistically significant, follow-up tests indicated that immediately post-intervention, the “specific” and “generic” social normative messages were significantly different than the no-contact control. These two referent groups are therefore promising for future research.

the control condition in either of the outcome variables. This is the case for both ITT and ToT analyses. In the North Shore trial there was no control condition and all treatment groups received bin liners between T1 and T2. While there was evidence of an effect of the interventions overall, at T2 and (sometimes) T3 relative to T1 for both set out and participation (see Tables 8 and 15), when analysed by condition, there is no evidence that the bin liners are doing anything to increase either set out or participation post-intervention (see Tables 9 and 16). This increase is most likely attributable to the postcards; we have already seen that for set out there was a postcard effect at T3 relative to T1 when compared to the bin-liner only condition.

Providing households with free kitchen caddy liners is a BI tool intended to make the desired behaviour easier to perform by removing physical (the need to clean the food scraps bin), psychological (the messiness associated with food waste) and financial (the cost of the bin liners) barriers. There is a large body of evidence showing that making a desired behaviour easier, is a successful behaviour change tool (Osbaldiston & Schott, 2012; Mertens et al., 2022). As there are very few studies testing “make it easy” interventions to promote food waste recycling, I will also discuss results related to dry kerbside recycling, a behaviour with many similarities to food waste recycling. Osbaldiston and Schott (2012) found in their meta-analysis of 253 treatments to promote pro-environmental behaviour (PEB) that “make it easy” type treatments were moderately effective, with an average weighted effect size of g=0.46. When combined with prompts, they were particularly effective, and when broken down by type of PEB, those most effective for promoting kerbside recycling were “make it easy” and rewards. In their meta-analysis of 70 treatments to promote household recycling, Varotto and Spagnolli (2017) found that the most effective treatments were social modelling and environmental alterations. They define the latter as consisting of “making recycling more convenient and easy to perform by modifying the physical environment, for instance by…providing home equipment for sorting waste” (p.172). They found that it had an average effect size of g=0.73.

Four studies specifically tested interventions to increase the ease of food waste recycling and found positive effects. Bernstad (2014) found a significant pre versus post increase in the weight of food waste collected after installation of kitchen equipment to hold compostable food waste bags. Flygansvær et al. (2021) found an increase in the weight of food waste and Langeveld et al. (2020) in the frequency of depositing food waste, after free sorting bags, bins or caddies were provided. DiGiacomo et al. (2017) and Langeveld et al. (2020) found that reducing the distance to a communal food waste collection point improved composting rates and frequency of correct disposal, respectively. Two other studies involving food waste recycling have explicitly included free bin liners for kitchen caddies. The WRAP research pilots found that participation and weight of food waste collected was increased in areas that received free bin liners as part of a combination of interventions being trialled (WRAP 2021b). However as free bin liners were included with at least one other intervention in the different trial areas, it was not possible to differentiate any effect of the free bin liners in isolation. An Irish study also found that inclusion of a kitchen caddy and liners increased participation, quality and quantity of food waste collected, over and above the effect seen from canvassing plus an information leaflet (CRE, 2019). This study, like that of Bernstad (2014), Flygansvær et al. (2021) and Langeveld et al. (2020) mentioned above, differed from our own, since households did not have existing kitchen food scrap sorting equipment prior to the study period, whereas in ours they did. It is difficult to know whether the increase seen in their respective outcome variables was due to the novelty of a new system being introduced, or the provision of free kitchen sorting equipment. It is quite surprising nevertheless, that providing free kitchen caddy liners to reduce financial, psychological and physical barriers did not result in any effect for either set out or participation in either of our trial areas, as we had assumed that increasing ease would cause an increase in participation and/or set out.

4.7. The efficacy of the canvassing intervention

The canvassing intervention did not have any effect on changing food scraps recycling behaviour in Papakura. There is no evidence that the canvassing treatment condition was

different to the control condition in either of the outcome variables. This is the case for both ITT and ToT analyses. This is also the case in the North Shore trial where we see no effect of canvassing in any of our models. Canvassing is a traditional behaviour change tool and not something specifically used under the BI umbrella. It is time and resource intensive. Despite these financial constraints, it is commonly used by local governments. Auckland Council wanted to test the efficacy of canvassing and compare it to other interventions.

There is mixed support in the literature for canvassing to change food waste recycling behaviour, meaning that our findings are in line with some previous studies. One study found no effect of canvassing on the weight of collected food waste when compared with a group that were not canvassed (Bernstad et al., 2013). There was no control group in this study and both groups received written information about a new system for sorting food waste that was introduced into the area. There was no evidence that the two groups were statistically different. A study carried out in China saw a statistically significant increase of 12.5% in the capture rate of food waste (Dai et al., 2015). The aims of this study were to test if canvassing was effective, as well as to investigate which elements of canvassing make it successful. Since canvassing (also referred to as doorstepping) as an intervention is neither well defined nor consistent across studies, this makes it difficult to know which elements are actually having an effect on behaviour change and which are not. The researchers found that of eleven clusters of behaviour-change determinants, only social norms and emotion were important, while prompts was a minor determinant. All other determinants, including beliefs about environmental consequences (that actions make a difference), and knowledge of how the system works, were found to be unimportant.

Several studies have tested canvassing as an intervention to increase dry kerbside recycling. One study found that canvassing produced a 5.4% increase in participation in a kerbside recycling programme, compared to a control group (Cotterill et al., 2009). On the other hand, two other studies found no significant differences in recycling participation post-intervention,

compared to a control group (Willman, 2015) or between treatment conditions (Timlett & Williams, 2008). Based on the mixed results in the canvassing space, it is not particularly surprising that no evidence of its efficacy was found in our study. The focus of the canvassing script was primarily educational – to ensure (a) that residents know how the food scraps service works; (b) its purpose (environmental consequences); and (c) to provide tips for dealing with common complaints about smell, pests and hygiene. The lack of an effect in our study supports the qualitative findings of Dai et al. (2015) about the determinants of food waste recycling. The script also included the opportunity for residents to ask questions and to make a commitment to using the food scraps service (or use it more often) in the future, however this was not the main message of the script.

4.8. Limitations and directions for future work

An important caveat on the results we report across the two studies is that the effects are relatively weak. The odds ratios we infer in our models range from 0.92 to 1.82. An Odds Ratio (OR) of less than 1.44 is considered to be a very small effect size and between 1.44 and 2.47 is small (Ben-Shachar et al., n.d.). All effect sizes detected in our analyses were small or very small by these standards. The largest effect seen (an OR of 1.82) was for the effect of the postcard at T3 on set out, in the North Shore ToT model.

There are three possible reasons for these small effect sizes. The most straightforward is that the interventions were not very effective. When compared to effect sizes in the BI literature generally, this is a plausible explanation. For example, a 2019 systematic review and quantitative analysis of nudges found that they have a median relative effect size of 21% and that only 62% of nudging treatments were statistically significant (Hummel & Maedche, 2019).[17] Mertens et al. (2022) found that nudge interventions had on average, a small to medium effect size of d=0.45.

[17] The relative effect size is defined as “the percentage change between the dependent variable of the treatment group and the control group” and is used to measure effect size when other measures, such

Another explanation is that our data contains a large amount of noise due to measurement error. This can make it more difficult to differentiate between real effects and random variability. Since effect size is a measure of the strength of a difference between two estimates, less precise measurement will tend to reduce the size of the observed effect. Noise is a particular problem when effect sizes are small: the greater the measurement error the more difficult it is to detect true but small effects; their presence can get lost in the surrounding random variation (Cohen, 1988, p.25).

Beyond measurement error, there were many other potential sources of noise in our data. Most of this came from street clusters and households, where variation between households and clusters was high. This is not something that could be reduced, which is why we included street clusters and households as random effects in our models. However, there were also other sources of noise introduced by the difficulty of measuring behaviour accurately. In light of this, we employed a number of strategies to minimise the noise our measurement procedures introduced. These included, providing brief training for the surveyors along with written instructions; standardising recording processes and refining these over the course of the study; beginning data collection at approximately the same time each day; to the extent that it was possible, using the same people for the same monitoring routes each week; including a “previous notes” column in the data sheet, to allow monitors to see important information, to ensure consistency in data recording (this was not introduced until the beginning of the T2 surveying period); and data auditing being repeatedly carried out by the same person.

Despite the steps we took, there are various strategies that could be used to further reduce noise. The following is a list of recommendations for a follow-up study of this kind.

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as Cohen’s *d*, which rely on the pooled standard deviation (that is not reported in all studies) cannot be used (Hummel & Maedche, 2019, p.49).

- Greater training of monitors. We had an approximately one-hour training session prior to each of the three-week monitoring periods. The first included practical training (practice at bin recording) as well as written instructions. However, not all people were able to attend the training session. Those who did not attend were given written instructions and on their first round, were paired with someone who had attended the training. Despite this, there was still some confusion about the correct process of recording bins, and potential variation between monitors. We believe that more practice, prior to the study, would have been beneficial to reduce both between- and within-monitor noise.
- Using fewer monitors more often. We had approximately 10 monitors throughout the trial period. It would be beneficial to use fewer people more frequently to reduce between-monitor noise.
- Using the same people for the same streets repeatedly. Although we attempted to do this, practically, this was not always possible, due to people changing jobs or being unavailable, for example. Using the same person on the same streets each week has the benefit of greater consistency for data collection on that route.
- Communicating with the collection contractors. This was one source of noise that we attempted to address, with limited success. We were unable to liaise directly with collection truck staff despite our desire to, which meant that monitors had to guess the routes that collection trucks would take on a given day. Knowing the timing and exact route that the food scraps truck would take, would have been beneficial in helping to reduce noise by ensuring that monitors could always visit a street just prior to the truck collecting bins from the street. Although monitors attempted to follow the truck collection routes, sometimes drivers took different routes from the previous week and/or began at different times. This was a source of variation in whether bins were or were not counted on the day. While it was a source of noise, we do not believe that truck route variability and timing was also a source of bias, given that the variation was random and not restricted to one particular collection day or route. We strongly

recommend future studies of this kind alert collection contractors that a study is taking place, and of the need to remain consistent in their collection routes and timing. This would have prevented the discrepancy with some Milford households having their bins collected on Wednesday September 26 (discussed in Section 2.8).

- Beginning in week 1 of T1 (pre-intervention), having monitors record bin address labels on the data sheets, for every bin that was set out on their route. Although we recorded relevant identifying data if a bin was missing an address label (as well as marking the bin with a letter code), both between- and within-monitor noise could have been further reduced by having a clear bin reference number recorded in the "previous notes" column of the data sheet, so that in subsequent weeks monitors could cross-check bins that were set out, to ensure they were recording the *same* bin.
- Writing a letter code on *all* bins that were missing an address label, even if it was clear to the monitor on the day that a bin belonged to a particular household. This could have removed ambiguity later, especially given the assumption that more bins were likely to be set out in the post-intervention period, leading to the future possibility of confusion around unlabelled bins.
- A "previous notes" column in the data sheet should be included. We did not realise the importance of this until after the study had begun. Having this column is essential to pass on important information for the following week, and to prevent mistakes in data collection.

The above recommendations are all factors that could reduce noise further, decreasing overall error and increasing the power of the study to identify or reject a true effect. Given that we randomised the treatments however, we do not expect these factors to have produced the effects we observe as an artefact.

A second, related, limitation of our study is that we would have benefitted from having a larger sample size in both trials. This is another potential explanation for the marginal effects seen in some of the models, which were smaller than anticipated. Since small effect sizes need

greater statistical power to be detected, two options to increase power are to reduce noise (discussed above), and to have a larger sample size. Greater power means lessening the probability of making a Type II error (failing to detect a difference between conditions when a difference exists; Bernard, 2011, p.529). Increasing sample size has the benefit of making the sample more representative of the population, and reducing the role of chance in the results, thereby increasing the chance of detecting a true result (Wittes, 2002). It was an interesting and unanticipated finding that there was large variation in size between the sample that we intended to treat and the sample that were actually delivered a treatment (the latter being much smaller). This was particularly true for the canvassing treatment, where less than 30% of households in the condition were actually canvassed in each trial. This meant that the ToT sample sizes in particular, were smaller than anticipated. It is likely that due to the size of some of our samples, we did not have sufficient power to detect real effects in some of our models. Although one way to increase power, and therefore the likelihood of detecting a small but real effect, is to increase the sample size, this comes with its own costs. A larger sample requires more expense and resourcing (for example, higher printing costs and more people to carry out the study).

We had not fully considered that certain interventions are much easier to deliver than others, meaning that most people we intended to treat had the treatment delivered. For example, almost all households received the free bin liners and postcard treatments. Other interventions, like canvassing, are time consuming to deliver and can only be delivered to households with members who are willing to take part. Future research should consider these differences and plan for them accordingly. One way of helping to ensure that more of the households that we intended to treat were actually treated, is to extend the intervention delivery window. We canvassed in a relatively short window (for this kind of activity where significant numbers of people are not expected to be home or available when the canvassers door-knock). Households in the treatment condition were only visited once, so if no one was home at the time, there were no further opportunities to speak with a resident. The canvassing

approach could, in future, use a longer window. Canvassing at a time when more people are expected to be home, for example, late afternoon or evening, could also help. However, extending the canvassing window itself requires greater cost and resourcing. Given the financial and practical constraints related to canvassing, and no evidence of an effect for either set out or participation, we suggest that interventions that are both easier to deliver, and that are shown to have an effect (postcards and stickers) may represent a more cost effective choice.

Stickers were relatively easy to deliver and were shown to be effective, but we believe that this intervention would also benefit from a longer delivery window, given that not all households put their refuse bins out weekly and weather can hinder delivery. Even two extra weeks would have been beneficial during our study. Postcards were the easiest of the interventions to deliver, given that almost all households have a letterbox. However, a postcard effect was only seen on set out. We know that food scraps recycling is not yet an established social norm in Auckland, with a collection service that is currently only offered to less than 1% of Auckland's population. We also know that regarding BEBs, social norm messages are sensitive to contextual factors and do not unanimously work (Schultz, 2014). The message may need to be targeted to specific households or streets, giving personalised comparisons between own behaviour and that of a relevant referent group. In addition, the injunctive norm that we used was not directly about beliefs about *food scraps recycling*, since we lacked a true norm of this kind. Instead we used a related norm, which highlighted people's beliefs about *reducing waste going to landfill*, and linked this to the use of the food scraps service.

In light of these points, and given that we saw a positive effect of the postcard on set out, it would be worthwhile trialling a social norm intervention in a future study when the food scraps collection service is offered to a greater proportion of Aucklanders, and is therefore more visible and normalised. As the roll-out of the service is taking place in 2023, this could be in the near future. Modifications to the design for a future study could include, using an injunctive

norm that specifically invokes people's beliefs about the correct food scraps recycling behaviour (gathered through survey data), and providing targeted descriptive comparisons to individual households or streets. Planned RFIDs on food scraps bins across the city will greatly increase the ease (and lower the cost) of implementation, since information about household participation and weight of food waste diverted from landfill will be automatically captured, meaning that physically counting bins would not be required. As this information could be collected weekly, it is feasible that it could be communicated to residents through means such as email or an app. This would reduce the costs associated with printing and delivering feedback via post or by hand. Prior research has also shown that to produce ongoing and long-term changes in behaviour, feedback needs to be continuous (Mertens & Schultz, 2020; Nomura et al., 2011). Utilising participation and weight data provided by RFID to give continuous feedback to residents through ongoing social comparisons, is a cost-effective means to scale up a promising intervention.

Another potential limitation is that factors outside of our control meant that the free bin liners given to North Shore residents included potentially confounding communications about the bin liners being more affordable. We were concerned that directly advertising their affordability to households could incentivise use of the food scraps service, confounding the effect of *free* bin liners (see Appendix F for more information). Ultimately, we do not believe that this posed an issue for interpreting our results. We did not see any effect of the free bin liners on set out or participation, therefore the need to try and disentangle the incentive effect from the effect of free bin liners is redundant. As the Papakura study (which used a non-confounding flyer to accompany the free bin liners) also found no effect, this further supports bin liners (more affordable or free) not being an effective intervention.

A final limitation relates to the potential for bias to be introduced in our ToT analyses. As discussed, we carried out two types of analysis: Intention-to-Treat and Treatment-on-the-Treated. In general, there was not a lot of difference in outcomes between the two types of

analysis, however in a number of cases we saw slightly clearer effects in the ToT samples. For example, in our regression analysis for North Shore set out, the overall effect of the treatments was larger for the ToT sample than for the ITT sample (see Section 3.4). And in Papakura, the sticker prompt was shown to have a clear effect on participation when considering ToT, but was marginal when considering ITT (see Section 3.6). The fact that effects are larger and/or stronger in ToT regressions than in ITT regressions is consistent with the increases in set out and participation, post intervention, being due to the treatment, since we would expect that limiting the sample to those who received the treatment would produce a stronger effect. However, this is not the only explanation. With ITT analyses we can more confidently claim a causal effect of the interventions that were successful, on our outcome variables. This is because we randomised street clusters to condition, which helps to eliminate systematic biases that may otherwise confound experimental outcomes. However, with ToT analyses it is less straightforward to claim a causal effect. ToT analyses reduce sample sizes to include only those participants who received a treatment. This can introduce bias. For example, if we consider the analyses for canvassing, a large number of households in the ITT sample were not included in the ToT sample. We may have introduced sample selection bias, since whether a household received a treatment is not completely randomly determined but instead, determined based on non-random characteristics such as a tendency to be at home during the morning (when the canvassers called) and a willingness to talk to the canvassers about the food scraps service (people who are more concerned about environmental issues may be more likely to do this). Consequently, conclusions about results from ToT analyses need to be interpreted more carefully. It is important to note that we do not *know* that the kinds of characteristics mentioned above do confound results; it is simply possible. Other considerations, such as whether ITT and ToT estimates are similar and pointing in the same direction, how similar set out and participation estimates are, and the proximity of the credible interval to zero, are all important.

In the case of canvassing, we did not find any meaningful effects for set out, participation, ITT or ToT so the above point is moot. However, for the sticker prompt intervention it is relevant, since an unambiguous effect of the sticker prompt on participation was only seen in the ToT analysis.[18] The sticker prompt treatment condition had a much smaller sample size for the ToT sample (*n*=332) compared to the ITT sample (*n*=496). There are four potential reasons why approximately one third of refuse bins in the sticker prompt condition did not receive a sticker on their bin. First, not all households put their refuse bin out every week for collection; households that produce less refuse have no need to. Second, the cost of refuse bin tags[19] may deter some households from setting their refuse bin out weekly, meaning the bin had less chance of being stickered. Third, inconvenience is likely to have played a role. Some households are more tolerant of the inconvenient aspects of waste collection (for example, poor weather, or the distance to the kerbside; Yang et al., 2022). Refuse bins belonging to households less tolerant of inconvenience are more likely to have been missed, given that it rained on three out of the six opportunities for affixing stickers. Finally, some residents put their bins out just before and/or take their bins in from the kerbside just after the collection truck has emptied them, and for these households it is more likely that their rubbish bin will have missed receiving a sticker, since collection timing varied and the intervention delivery did not always take place close to the collection truck timing.

How might these factors potentially bias the ToT sample? Recall that although we allowed three weeks for affixing stickers in the intervention period, as discussed in Section 2.8, there were delays with the printing of stickers. This resulted in the entire first week and Monday of the second week being ruled-out for sticker application. In the end we only had two

[18] The postcard intervention ITT and ToT samples were very similar sizes (in the North Shore there were only 11 households that did not receive a postcard; in Papakura there were only four). In addition, wherever a clear effect of the postcard was seen in the ToT analyses, it was also seen in the corresponding ITT analyses. We therefore do not need to justify a causal effect for the postcard treatment in the ToT set out analyses.

[19] Both trial areas are part of a pay-as-you-throw refuse collection service; households are required to purchase bin tags to attach to their refuse bin in order to have their rubbish collected.

opportunities per collection day to affix stickers to bins. If a household did not put their refuse bin out for collection on either of those two occasions, they were missed. This could create a sample selection bias towards households that (1) produce more refuse and therefore put their refuse bin out more often, (2) can afford to put their bin out more frequently, and (3) are more tolerant of inconvenience. Regarding the final factor mentioned, timing of bin collection, it is more likely that bins belonging to residents who work full time and are not at home during the day would be stickered. Conversely, elderly, or unemployed have more opportunity to take in their refuse bins promptly and therefore miss receiving a sticker.

While these potentially confounding factors may seem concerning, we believe that in the Papakura study they do not undermine our findings regarding the sticker prompt intervention. There are three main reasons for this. First, we saw similar effects for participation and set out. When comparing set out with participation in both ITT and ToT analyses, the estimates for the sticker at T2 coefficient are all positive and similar (see Table A12, Appendix G). Secondly, there are very similar estimates for the ToT and ITT analyses (in which sample selection bias cannot explain our findings; see Table A12, Appendix G). Finally, in each analysis the posterior distributions for this parameter are almost identical and the credible intervals are close to zero. While it is true that for the ITT participation analysis, the 95% credible interval overlaps zero, it is very marginal. When the analysis is restricted to ToT (where the effect of the intervention is expected to be stronger) then we unsurprisingly see a clearer effect (see Figure A4, Appendix G). These additional results provide good reasons to believe that the effect of the sticker prompt on participation, while small, is nevertheless real, for both the ToT and ITT analyses. Therefore, it is reasonable to conclude that although confounding is possible in the ToT sample, we think in the case of our Papakura trial it is unlikely to have affected our results and the conclusion we have drawn regarding participation. We may say then, that the refuse bin sticker prompt caused an increase in both set out and participation in the Papakura food scraps collection service.

It is also reasonable to believe that in cases where both regressions show meaningful effects but where the ToT effects are slightly stronger than the corresponding ITT effects (for example, for set out in the North Shore trial), the post-intervention increase is still attributable to the treatment; not sample selection bias. It is possible that sample selection bias is playing a role, however the fact that we always see similar estimates and largely overlapping posterior distributions in both models is reason to believe that the treatments are causing the increase. In the example of North Shore set out, the estimates for T2 and T3 in the ToT regression were slightly stronger than in the ITT regression (see Table 8). This analysis combined the three conditions and tells us whether food scraps set out increased overall in the post-intervention time points (T2 and T3), relative to T1 (H4a). In the hypothesis test for H4b–4c, there was only evidence of an effect for the postcard at T3, which suggests that it is the postcard that is primarily causing the overall effect seen in H4a. Importantly, the effect of the postcard at T3 is seen in *both* ITT and ToT analyses and the estimates are in all relevant respects, identical (ITT, log odds = 0.43, 95% CI [0.04, 0.82]; ToT, log odds = 0.44, 95% CI [0.04, 0.85]).

4.9. Generalisations

What general conclusions, then, can we draw on the basis of our findings? We believe our findings indicate that a proscriptive sticker prompt affixed to refuse bins of residents is an effective solution if it is scaled up Auckland-wide or to other parts of New Zealand. The predicted effect sizes are small (OR = 0.92–1.82) but stickering is relatively cheap and could have a sizable impact at scale. For example, (to sticker refuse bins across a city the size of Auckland would cost approximately $253,500). Based on model estimates this could lead to an additional 8,400 tonnes (95% CI [-1709, 18409]) of food scraps being recycled, which results in a median estimate of $27.40 per tonne (based on the current estimates of food scraps collected by the Council's food scraps service, per household, per annum – see Appendix H for a full explanation of the cost-benefit analysis). Our findings for the sticker prompt may also generalise to other countries with similar values, culture, style of government and level of wealth, for example, Australia. As we note above, a further study in Auckland

involving a larger sample size and longer intervention-delivery window would be useful to test whether there is a clearer effect of the sticker prompt on participation among the ITT sample.

Generalisations we can make about the social norms postcard are somewhat limited by the fact that the food scraps service is not currently Auckland-wide; if recycling food scraps was a more established norm, we may have seen an effect on participation as well as set out. We can confidently claim that the postcard we used is likely to encourage current users to increase using the service. As discussed in Section 4.8, we have recommended a different type of norm message be tried, which uses social comparisons that are personalised and targeted to individual households, and that feedback be repeated regularly.

Regarding canvassing, it is also difficult to generalise. There are many ways of canvassing and other ways may be more effective. One relevant finding to consider, from Dai et al.'s (2015) study investigating which elements of canvassing were effective, is that social norms and emotion were shown to be important elements of canvassing that influenced residents. In particular, the behaviour, characteristics and manner of the canvassers was found to be important. The study used young, student volunteers (who were accompanied by a local resident). The researchers emphasise that students and older people are likely to be particularly effective for this task, given the positive regard in which they are held. They concluded that the findings are insightful and can inform local planning efforts by highlighting the importance of the character and behaviour of the doorsteppers, putting more emphasis on promoting community behaviour change while reducing or eliminating messages about environmental consequences (Dai et al., 2015). However, this study was conducted in a collectivist nation (China), hence a very different context to the individualist setting of the current study (New Zealand) and this finding may not generalise. In general, collectivist societies value interdependence, cooperation, obedience and group harmony. The collective goals of the community are prioritised over individual goals and language reflects this, with a focus on the collective rather than the individual. There is an expectation of conformity to in-

group norms. In contrast, individualist societies prioritise personal goals and independence. People are encouraged to be creative, self-expressive, and make decisions based on their own views and interests. (Abadeer, A. S. Z., 2015). These differences may affect the impact of canvassing in New Zealand where the lack of collective pressure may reduce the effect of the norms conveyed by canvassers.

We do not believe that we have enough information to make generalisations about the effect of bin liners. Certainly, we saw no evidence of an effect of free bin liners in either of our trial areas. However, an important point to note is that we treated each study area as homogeneous when it may be that other factors determine how effective this intervention is. In particular, we think that a useful area for further research would be to conduct an analysis of how other factors such as socio-economic status change how effective free bin liners as a behaviour-change tool is. This would provide insights into additional barriers to food scraps recycling and what may be generalisable about free bin liners as an intervention.

Lastly, we should not expect that the postcard and sticker interventions, if scaled up, will produce profound changes (keeping in mind that this is not something that proponents of nudge have argued to be the case). BI interventions do not typically produce large effect sizes, particularly when compared to s-frame (system-level) change (Chater & Loewenstein, 2022). Chater and Loewenstein (2022) argue that there needs to be more focus on s-frame interventions as this is where substantial change can happen. They cite DellaVigna and Linos (2022) who recently analysed all 126 RCTs, involving 23 million people, run by two large US Nudge Units and found that they had an average impact of 1.4%, notably lower than the 8.7% average impact of nudges reported in academic journals (which they attribute to publication bias). Chater and Loewenstein (2022) also argue that nudging will likely only ever be mildly impactful. Referring to a study on the effect of green energy defaults on carbon savings, they argue that the ultimate impact of the defaults will likely be small since multiple barriers stand in the way of its application universally. However, in the case of encouraging food waste

capture at the household level, there is no reason why s-frame and i-frame solutions cannot work in tandem. Auckland Council is introducing s-frame change (with the roll-out of the service Auckland-wide). At the same time, i-frame solutions such as stickers and postcards can help to promote usage of the service at an individual level, since every little bit of progress towards reducing carbon emissions counts. Furthermore, utilising i-frame interventions does not preclude other, more impactful s-frame interventions (such as incentives for electric vehicle purchases, and carbon taxes) from being introduced concurrently; there is space for multiple solutions all targeting the same goal.

4.10. Conclusion

The study aimed to compare a range of promising interventions regarding how well they improved food scraps recycling behaviour through two concurrent field experiments in Auckland, New Zealand. This was the first study of its kind in New Zealand and is one of only a small number of studies on food scraps recycling, globally, to measure the effect of BI interventions on participation and set out outcomes. We aimed to overcome limitations of prior research, notably, a lack of rigorous experimental design, the focus on communal measures of food scraps recycling behaviour, and Northern Hemisphere settings.

Our results from the two field experiments show that proscriptive sticker prompts can successfully influence food scraps recycling behaviour, both for set out rates overall, and for the number of households participating, when compared to a control group. They also show that social norms messaging through a postcard can successfully increase the frequency of food scraps set out.

These findings point to two promising scalable solutions to increase use of food scraps collection services which divert food from landfills, nationwide. Based on our postcards treatment that included an appeal to social norms, our recommendation for a social norms-based intervention is to change the type of message (personalised, comparative feedback to

a relevant referent group), medium (digital), and frequency (repeated or ongoing). With all new food scraps bins in the Auckland urban area roll-out being RFID-tagged, this is a realistic and cost-effective option. We also recommend the inclusion of a proscriptive sticker on all refuse bins to coincide with the roll-out of food scraps bins across Auckland in 2023. Given that we saw a stronger ToT effect, and that Auckland Council will not be limited by a short intervention-delivery window, it is desirable that a greater proportion of households' refuse bins receive a sticker than the proportion achieved in our trial. While their effectiveness may be relatively small compared to system-level changes (Chater & Loewenstein, 2022), and smaller than some other individual-level behaviour change interventions (Mertens et al., 2022), refuse bin stickers are easy to implement, cost-effective, and semi-permanent, meaning they will not need to be replaced often. They are therefore a promising option for local authorities with small budgets, to scale up.

Our study's findings for the sticker prompt and postcard treatments also provide some promising support for nudge theory; in particular, the claim that simple, cost-effective solutions that are designed to circumvent cognitive barriers can be effective behaviour change tools. This is in line with the results of other studies which also found support for the results of nudge interventions (Mertens et al., 2022; Hummel & Maedche, 2019) across a variety of domains and in particular, at promoting PEBs (Osbaldiston & Schott, 2012; Nisa et al., 2019). Nudge is only part of the solution but nevertheless provides measurable, meaningful effects. The results of this study suggest that nudge interventions can be an effective tool for behaviour change, including in the context of waste management and recycling. Further research is needed to identify the most effective strategies for implementing them.

Appendices

Appendix A: Postcards sample

Note: image depicts front side of Papakura postcard.

Note: image depicts front side of North Shore Postcard.

Note: image depicts reverse side of both postcards.

Appendix B: Sticker prompt sample

Note: Image depicts the refuse bin sticker used in the Papakura field experiment. The dimensions of the sticker are 20 cm by 6 cm with a landscape orientation. The original choice for the sticker size and orientation was A5 portrait orientation, however, due to constraints on bin lid design, a compromise was reached. A smaller sized sticker of landscape orientation was agreed upon so it would not cover any pre-existing text displayed on the bin lid.

Appendix C: Bin liners and accompanying flyers sample

Note: images are of the food scraps caddy liners used in both field experiments.

FREE LINERS COMPLIMENTS OF AUCKLAND COUNCIL

A kitchen caddy is a small bin or container to collect food scraps in. Keep it somewhere handy, like on or under your bench.

To keep your caddy clean and avoid smells, you can line your caddy or any container with a compostable liner.

Scrape food scraps into your kitchen caddy.

When the caddy is full, tie the compostable liner closed and place in your green kerbside food scraps bin. Use a new compostable liner before you start filling it again.

Green liners have now changed to pink. The new Auckland Council bin liners now come in larger more affordable rolls of 20. Purchase from your local Countdown (ask at the checkout), library or service centre.

Store your compostable liners somewhere dry and away from direct sunlight. Extended exposure to water or direct sunlight could cause the bags to break down faster.

Find out more about your food scraps service at **aucklandcouncil.govt.nz/foodscraps**

AUCKLAND COUNCIL
WASTE SOLUTIONS

Note: image depicts front side of North Shore A5 flyer accompanying caddy liners.

FREE LINERS COMPLIMENTS OF AUCKLAND COUNCIL

A kitchen caddy is a small bin or container to collect food scraps in. Keep it somewhere handy, like on or under your bench.

To keep your caddy clean and avoid smells, you can line your caddy or any container with a compostable liner.

Scrape food scraps into your kitchen caddy.

When the caddy is full, tie the compostable liner closed and place in your green kerbside food scraps bin. Use a new compostable liner before you start filling it again.

Green liners have now changed to pink. Purchase from your local Countdown (ask at the checkout), library or service centre.

Store your compostable liners somewhere dry and away from direct sunlight. Extended exposure to water or direct sunlight could cause the bags to break down faster.

Find out more about your food scraps service at **aucklandcouncil.govt.nz/foodscraps**

AUCKLAND COUNCIL
WASTE SOLUTIONS

Note: image depicts front side of Papakura A5 flyer accompanying caddy liners.

USING YOUR GREEN ROADSIDE BIN

Keep your food scraps bin somewhere outside, such as next to your recycling bin.

Open the bin by moving the handle to this position. Put your compostable liner full of food scraps into the food scraps bin.

Lock the bin shut to prevent spills and odour, by moving the handle to one of these two positions.

On your usual collection day, take the bin to the kerbside by 7am. Keep bin handle in the upright position. Leave 30cm or a ruler space between bins.

You don't have to use a compostable liner for your food scraps (you can use newspaper, paper bags or paper towels), but if you do use a compostable liner it needs to be an Auckland Council liner.

Find out more about your food scraps service at
aucklandcouncil.govt.nz/foodscraps

AUCKLAND COUNCIL
WASTE SOLUTIONS

Note: image depicts reverse side of both North Shore and Papakura A5 flyers accompanying caddy liners.

Appendix D: Auckland Council food scraps service brochure

What goes in your food scraps bin:

✔ Vegetable scraps, including peelings, stalks and skin
✔ Fruit scraps, including peelings, cores, stalks and skins
✔ Meal scraps
✔ Bread
✔ Pasta and rice
✔ Dairy products
✔ Meat bones and scraps including fat trimmings
✔ Fish scraps and bones, and shellfish shells
✔ Eggshells
✔ Paper towels and tissues
✔ Coffee grounds, tea leaves and tea bags
✔ Indoor cut flowers

✘ Garden waste
✘ Disposable food containers and utensils, including compostable items
✘ Plastic bags
✘ Cigarette butts
✘ Sanitary products and nappies
✘ Pet waste
✘ Cling film, waxed paper or aluminium foil

Your food scraps or para kai will be turned into compost to be used to nourish the soil and grow more plants.
Think of it as recycling your food scraps.

Questions and answers

Will this food scraps service cost me?
This service is paid through a targeted rate to households in your neighbourhood.

What happens to the food scraps once they are collected?
Food scraps are full of valuable nutrients. The collected material will be composted for agriculture, fruit and vegetable growing and farming, across the North Island.

Do I need to use compostable liners in my kitchen caddy?
No, they are optional. Either use the official Auckland Council compostable liners (available at retail outlets), or old newspaper, paper towel or no liner at all.
Please don't use plastic bags or non-official liners.

I still want to compost, do I have to use the bin?
The food scraps service is complementary to home composting. The food scraps bin takes meat, small bones and shellfish. Please visit **compostcollective.org.nz** for more information on home composting.

What about compostable plates, cutlery and coffee cups?
Compostable packaging can't go into the food scraps bin because they take much longer to break down than food scraps.

What if I live in a multi-unit property (e.g. an apartment or a gated community)?
Your property may have a special arrangement for food scraps collections. Check with your property manager to see where to put your food scraps for collection.

Find out more about your food scraps collection at **aucklandcouncil.govt.nz/foodscraps**

How to make the most of your

FOOD SCRAPS COLLECTION SERVICE

Together we can protect our land and waste nothing.
Mā te mahi tahi e tiakina ai e tātou ō tātou whenua, tē tuku ai kia moumou he paku aha.

Find out more at
aucklandcouncil.govt.nz/foodscraps

Avoid smells

These tips will help you keep your bins clean.

1. **Line** your kitchen caddy with an Auckland Council compostable liner. These are available at local supermarkets or the local library.
2. **Wrap** your food scraps in newspaper, paper bag, or a paper towel to soak up any liquid.
3. **Store** your food scraps in the fridge or freezer before putting in the food scraps bin for collection.
4. **Rinse or wash out** the caddy and bin regularly.
5. **Put** your food scraps out for collection each week, even if you don't have a lot.

Use it even if you never cook

Use it for the things you don't eat like:

Paper towels, Bones, Coffee grounds, Tea leaves and tea bags, Orange peels, Banana peels, Apple cores, Mouldy bread, Eggshells

LITTLE SCRAPS, BIG DIFFERENCE

Using the food scraps bin a tiny bit will save you money on rubbish bin tags because you won't need to put your rubbish bin out as often.

Turning kai into kai

Food scraps can be made into compost then used to feed soil that in turn grows more plants and vegetables. Collecting and composting food scraps or para kai will mean less rubbish goes to the landfill.

If you need a food scraps bin or your bin is broken, visit **aucklandcouncil.govt.nz/rubbish-recycling/bin-requests** or call **09 301 0101**. The food scraps bin service is included in the annual targeted rate for all households in your neighbourhood, so there's no additional costs to have it emptied.

If you live in a multi-unit property, talk to your property manager about your collection process.

Note: Food Scraps Service brochure was offered to residents during canvassing if they asked for more information or did not wish to speak to a canvasser.

Appendix E: Auckland Council food scraps service brochure

Food Scraps Canvassing Script 2022

Script	Action	Log Sheet
Introduction → Hi my name is …… from Auckland Council. This is …… We are WasteWise Advisors here to talk to you today about your Council kerbside food scraps collection. ➢ **Q1. Is this a convenient time to speak with you? We'll only take about 5mins of your time.** • If No, then thank and offer an information brochure, so they know how to use their bins.	If no one is home leave a calling card in the letterbox - with your name and the date/time recorded Give flyer and/or guide	Record house number and **CC** where calling card is left Q1. Y/N.
How the service works ➢ **Q2.** Are you aware of the council kerbside food scraps collection? If they are not aware: ➢ The service is a *weekly* collection of food waste from the kerbside, *"like recycling but for food"*. ➢ **Show short 2min** video clip on iPad ➢ Explanation of service – and show story board of how to use the kitchen caddy and the kerbside bin. Explain what can and cannot go into the food scraps bin.	If the resident is new to the area or is not aware of the service show them the video and story board explaining how the food scraps collection works.	Q2. Y/N

Q3. Why are we separating food scraps from rubbish? [Ask if they are aware of the reasons since many people do not know of the connection between food in landfill and climate change.] • Food does not breakdown in landfill – creates methane which leads to climate change • Explanation of what happens to the food scraps that they put out e.g. the food scraps that you put out will be turned into compost that can be used to grow food and fertilize the land.	ACTION – show the resident the breakdown of food scraps in the rubbish bin in the flyer and explain these resources could be put to better use. Question them if they have a bin (or not). If no, or it is damaged, advise how they can go about getting one call or on the council website (either new or replaced by visiting AC website) https://www.aucklandcouncil.govt.nz/rubbish-recycling/bin-requests/Pages/request-new-bin-replacement-repair-online-form.aspx	Answer not recorded.
Q4.a How often do you use your food scraps bin? This leads on to congratulations for being an awesome user. For most people we know that they could use it more – follow up question: **Q4b**. Do you mind telling me why you are not using the service/using the service much at the moment?	Show storyboard of all the types of food that can go into the bin – highlight leftovers, meat, fish, cheese,	Q4a. Weekly/Bi-weekly/once per month/Other (record)/never Q4.b. NB = No Bin, U = unable/disabled/elderly, NI = Not interested, B = too busy, NE = Not enough, M = mistrust, C = confused about items, DN = Don't know, Y= yuck factor S = space HC = home compost Other = list

New liners are coming to replace the current ones, you should receive a voucher for a free packet. The new liners are will be cheaper, they are now 20 liners for $2.80.		
Q5. Have you had any issues with smells?	Talk to the residents about some of the tips and tricks others have found useful - Liner or newspaper - Keep scraps in the freezer until collection day - Empty and clean kitchen caddy regularly New liners are coming to replace the current ones; NORTH SHORE – you should have received a roll and guide showing you how to use them in the mail. PAPAKURA - you should receive a voucher for a free packet in the post over the next few weeks. The new liners are will be cheaper, they are now 20 liners for $2.80.	Q5 Y/N
Q6. Can we count on you to use the food scraps collection (more often)? → If No, record response in comments section	Show storyboard and provide translation if needed	Q6. Y or comment

CLOSE **Q7. Do you have any other questions?** → Here is Council's food scraps guide. Thank you for your time. Enjoy the rest of your day/evening If the householder wants you to stay longer, explain that you have to go to speak to their neighbours, but if they want to email you (leave a calling card) or they can phone the call centre 09 3010101. If the householder asks about anything else related to Council or another department, ask them to phone the call centre. If they are adamant that you help them or if they are confused, take note of the problem and phone it through to the call centre on their behalf once you have left their property	Give Guide Leave calling card with contact details if necessary. Take note of the problem and phone it through to the call centre on their behalf once you have left their property	Q7. Record any questions OR any other issues etc.

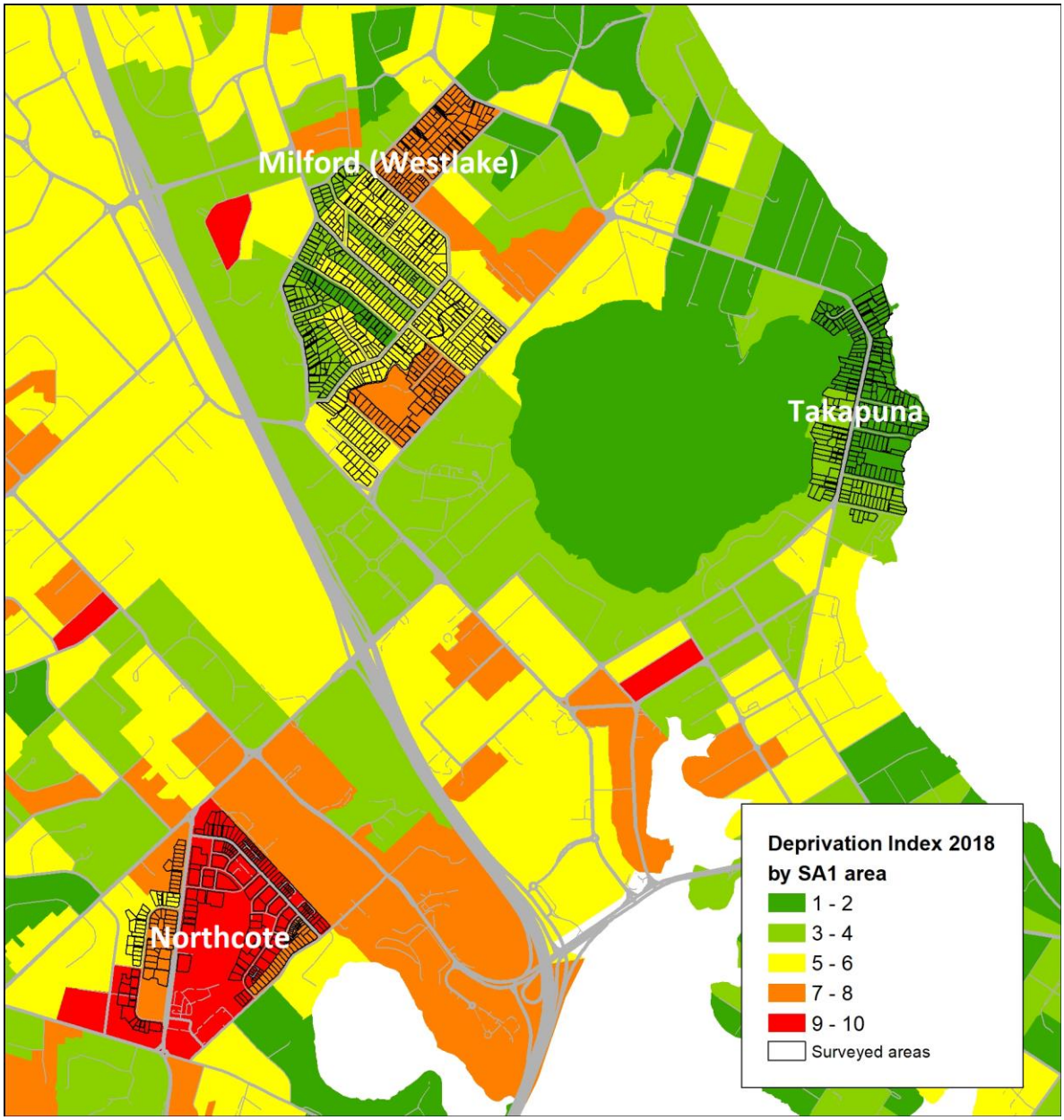
Codes for log sheet							
Q1: Y = yes N = no	Q2: Y = yes N = no	Q4.a: Record verbatim	Q4.b: NB = No Bin, U = unable/disabled/elderly, NI = Not interested, B = too busy, NE = Not enough, M = mistrust, C = confused about items, DN = Don't know, Y= yuck factor S = space HC = home compost Other = list	Q5: Y = yes N = no	Q6: Y/ or Record reasons verbatim	Q7: Record verbatim	Separate Bin Request log sheet if they have no bin – record reason: • M = Moved/renting • S = Stolen/missing • B = Broken • R = Removed

Appendix F

Supplementary Methods

Figure A1

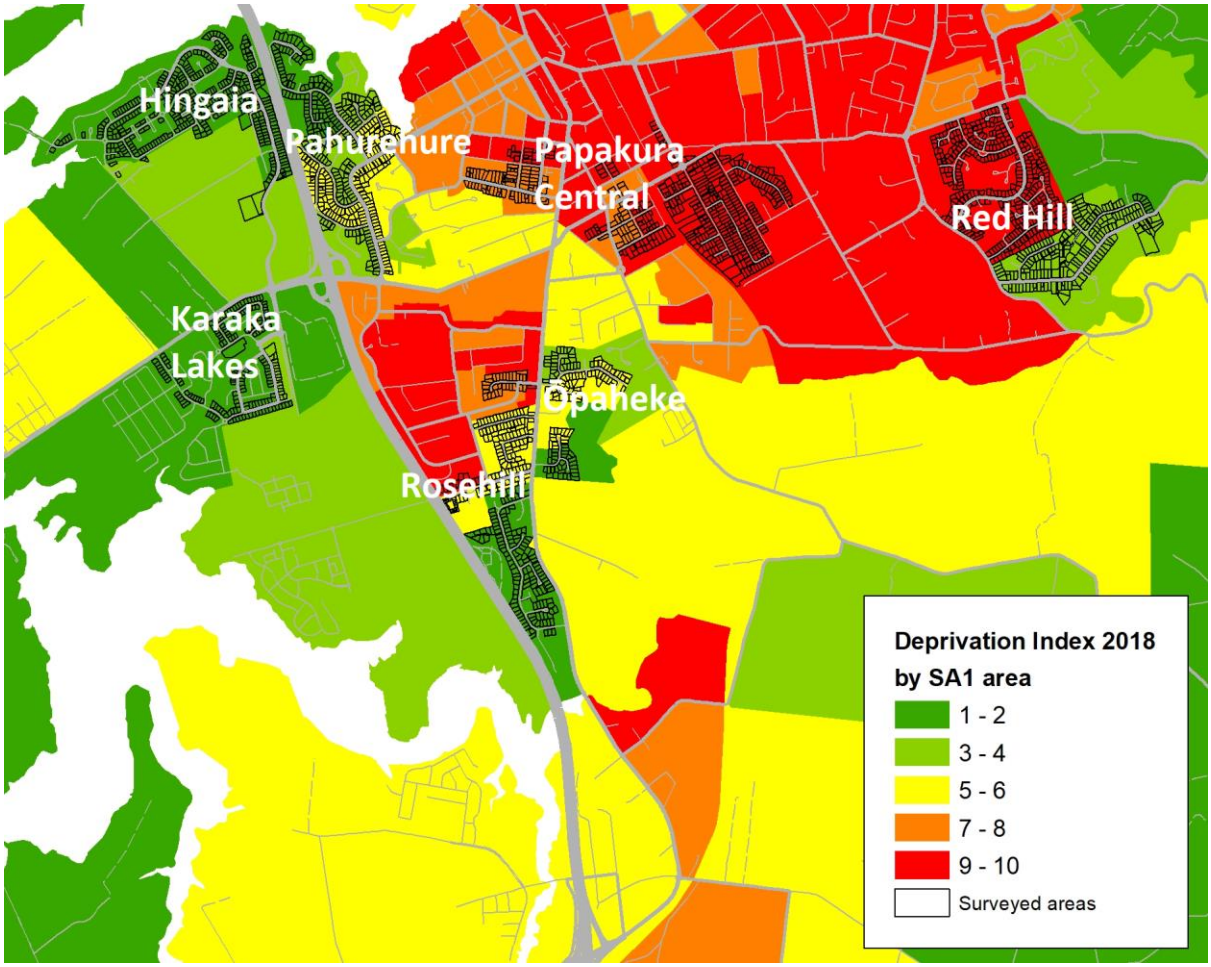
NZ Deprivation Index 2018 by SA1 Area, North Shore Food Scraps Service Area



Note: The Deprivation Index value given to each household is of the average value of the Statistical Area 1 (SA1) area that the property resides in. SA1 is an output geography that comprises up to 500 residents). An individual property's "effective Deprivation Index" value, therefore, may differ from the average value. Average values are shown in Figure A1.

Figure A2

Deprivation Index 2018 by SA1 Area, Papakura Food Scraps Service Area



Note: The Deprivation Index value given to each household is of the average value of the SA1 area that the property resides in. SA1 is an output geography that comprises up to 500 residents). An individual property's "effective Deprivation Index" value therefore, may differ from the average value. Average values are shown in Figure A1.

Additional information about Intervention 3 - free kitchen caddy liners.

Our trials coincided with the timeline for a planned campaign to introduce a new type of caddy liner to replace the existing one. The new food scraps caddy liners are pink, come in a roll of 20 and cost $2.80 per roll; whereas the liners being replaced were green, came in a roll of 8 for $2.00 per roll. Our preference was that the flyer accompanying the free liners in the bin-

liners treatment groups only include visual and written information about how to use the food scraps caddy liners and food scraps bin. However, some potentially confounding communications needed to be included with the free liners given to North Shore residents. Because of this, it was decided that we would have two versions of the A5 flyer: one for Papakura and another for the North Shore trial. One side of the North Shore trial flyer depicts, in a story-board style, how to use the food scraps caddy with caddy liners as well as optimal storage for the liners. It also includes the following communications: “Green liners have now changed to pink. The new Auckland Council bin liners now come in larger more affordable rolls of 20. Purchase from your local Countdown (ask at the checkout), library or service centre.” A roll of 20 caddy liners is expected to last a household on average about seven weeks. This means that for most households in the North Shore trial area, they would run out of the free liners during the trial period and would need to purchase more. Advertising the more affordable liners to these households via the flyer could incentivise people to begin using the food scraps service (or to using it more frequently) during the trial period. We ensured consistency across the sample by every household being given the same flyer in addition to the free liners. Although we would not be able to attribute any potential effect directly to the free liners, we could conclude that a combination of the free liners, plus potentially incentivising communications material, caused an effect. The reverse side of the flyer depicts, in a story-board style, how to use the food scraps bin. It also lets people know that they can use newspaper, paper bags or paper towels to line their caddy with, as an alternative to the Auckland Council-approved caddy liners. The Papakura trial A5 flyer that accompanies the free caddy liners is identical to the North Shore flyer with the exception of the wording at the bottom of side one. It states: “Green liners have now changed to pink. Purchase from your local Countdown (ask at the checkout), library or service centre.”

Table A1

Deprivation Index Score Across Conditions in the North Shore Trial

Condition	Mean Deprivation Index Score	SD
Bin Liners	5.18	2.25
Postcard	5.20	2.15
Canvassing	5.34	2.26

Note: Mean Deprivation Index Score is the average value assigned to a household for the SA1 area to which the household belongs, averaged across conditions.

Table A2

Deprivation Index Score Across Conditions in the Papakura Trial

Condition	Mean Deprivation Index Score	SD
Control	6.05	3.37
Sticker	5.58	3.43
Postcard	5.41	3.63
Bin Liners	5.33	3.31
Canvassing	5.20	3.36

Note: Mean Deprivation Index Score is the average value assigned to a household for the SA1 area to which the household belongs, averaged across conditions.

Additional information about data sheets and data collection. Our data collection sheets used in the T1 monitoring round included 5 columns: “Street name”; “House number”; “Long drive”’ (where Yes or No needed to be circled during the first week of monitoring in the T1 monitoring period, for whether or not the household shares a driveway with another household); “Food Scraps Bin” (where 1 = a food scraps bin was set out for the household (if more than one bin was present then the number of food scraps bins set out is recorded) and

? = there was an unidentified bin that could belong to the household); and “Comments”. This last column was where the surveyor filled in details about the unidentified bin, or any other helpful notes, for example, “bin is missing a handle”. A modification was made to the data collection sheets used in the T2 and T3 monitoring rounds. Two additional columns were added. “Previous notes” for important information that could help surveyors identify bins, for example, the number that appears on a bin, in cases where the bin number differs from the letterbox number; and “Record bin label” (where the surveyor was required to specifically record the number as it appears on the bin, for certain bins; this was to help clear up any ambiguities that had arisen). A sample data sheet is provided below.

Whenever a monitor came across a bin where it was ambiguous as to which household the bin belonged, the following process was followed:

(1) They marked the bin with a letter code “W”, “X”, “Y” or “Z”, with a permanent marker. This would allow the person monitoring bin set out in subsequent weeks to identify if the same bin is out.
(2) Next, a question mark would be entered into the “Food scraps bin” column for any households to which the unidentified (and now coded) bin could belong.
(3) A comment would be written in the “Comments” column stating that one bin was marked with a letter, for example, “W” (and the date this was done) and any other relevant information such as “Bin marked with W is next to recycling bin #23”.
(4) In subsequent weeks it was often possible to unambiguously assign a previously unidentified bin to a specific household. For example, if the resident spoke to the person monitoring and confirmed that they were the owner of the bin, or if the unidentified food scraps bin was placed next to a rubbish or recycling bin belonging to a specific address and there were no other addresses’ bins nearby.

It was not always possible to unambiguously assign unidentified bins that had been coded with a letter, to specific household addresses. At the end of the study there were six

unidentified bins remaining in the Papakura trial and 10 in the North Shore trial. For these cases, in post-data-collection processing, we removed addresses assigned a question mark and replaced them with the same number of hypothetical addresses, each representing a bin, including the observed bins (for example, W, Y, Y, Z). Observed bins were assigned a 1 for weeks when they were observed. In this way, we were able to track household food scraps bin set out for cases where there were up to four houses sharing a driveway, even if we could not unambiguously assign each bin a specific household address.

Sample data sheet.

Participation Survey MNS 1							
Date:	Week # 1				Surveyor:		
	Street	House #	Long drive	Previous Notes	Food waste bin	Record bin label	Comments
1							
2							
3							
4							
5							
6							
7							
8							
9							
10							
11							
12							
13							
14							
15							
16							
17							
18							
19							
20							
21							
22							
23							
24							
25							
26							
27							
28							
29							
30							
31							
32							
33							
34							
35							
36							
37							
38							
39							
40							

Description of Explanatory variables.

Papakura Study

The explanatory variables in our set out and participation analyses include:

- The *treatment condition* – the condition to which a household is allocated. Treatment condition is a factor with five levels: sticker prompt, social norms postcard, free bin liners, canvassing, control.

- The *time point* when measurement took place. A factor with three levels: T1 = baseline, T2 = first post-intervention period, T3 = second post-intervention period.
- *Cluster number* - an area-level factor variable which indicates the street cluster (1 to 50) to which a household in each sub-group was assigned.
- Individual household *unique ID* – a household-level factor variable.
- *Received treatment* – a binary variable where (1) indicates that the household received the treatment (whether it was delivered) and (0) that it did not. For some households the treatment was not delivered for some reason (for example, the house had no letterbox so the postcard could not be delivered).
- *Sticker on Monday 12 September* – a binary variable which only applies to those in the sticker prompt condition, where (1) indicates that the household's refuse bin received the sticker on Monday 12 September, which overlapped with the first day of the post-intervention period and (0) that it did not. This variable is explained in Section 2.8 and is only used in a follow-up analysis.
- In addition, we also took the opportunity to collect other potentially useful data outlined below. These variables were not part of our study pre-registration, however. They included:
- *Collection day* – the weekday that a household usually has its food scraps recycling collection: either Monday, Thursday or Friday.
- *Deprivation Index* – a numeric deprivation index classification from NZDep(2018). NZDep for 2018 is an area-based measure of socioeconomic deprivation in New Zealand and measures the level of deprivation for people in each Statistical Area 1 (SA1) in New Zealand. It is based on nine 2018 New Zealand Census variables and gives a deprivation score for each SA1, displayed as deciles. Decile 1 represents areas with the least deprived scores while Decile 10 represents areas with the most deprived scores. Higher deprivation is associated with lower levels of recycling, since poverty, transience, high-density housing and other pressing issues faced by lower-socioeconomic households mean that recycling is not prioritised (Nomura et al., 2011).

- *Long/shared driveway* – a binary variable which indicates that an address shares a driveway with another address in such a way that their bins were set out on the same berm space (1) or not (0).

North Shore Study

The explanatory variables in the North Shore study are the same as those in the Papakura study, with the following exceptions:

- *Treatment condition* only has three levels: free liners-only, social norms postcard plus free bin liners, canvassing plus free bin liners.
- *Cluster number* – there are 27 clusters to which a household in each sub-group could have been assigned.
- The *sticker on Monday 12 September* variable is not used.
- *Milford Wednesday collection day* – a binary variable which only applies to households in Milford with a Wednesday collection day where (1) indicates that the household is located in Milford and has a Wednesday usual collection day (0) it does not. This variable is explained in Section 2.8 and is only used in a follow-up analysis.

Additional information about other factors that had the potential to affect the outcome variables.

As outlined in Section 2.8, we had planned to avoid any public holidays and school holidays since people's food scraps behaviour changes from their typical behaviour during such times. However, due to unforeseen circumstances, the New Zealand government inaugurated a public holiday memorial day for Queen Elizabeth II on Monday 26 September; this fell during the third week of our post-intervention monitoring period (T2). The consequence of this public holiday was that rubbish, recycling and food scraps collection days were moved back by one day. Consequently, the last monitoring day of the post-intervention period took place on Saturday 1 October, rather than Friday 30 September. The school term ended on Friday 30 September. Many people go on holiday during school holidays, which potentially meant that

there were fewer food scraps bins set out on Saturday 1 October than there would normally be. This change in collection days also led to a discrepancy in the collection of food scraps bins for several streets in Milford on Wednesday of that week. On weeks where collection days are changed, people often continue to put their bins out on their usual collection day. This occurred on Wednesday 28 September, when some residents in the Milford study area put their bins out for collection on Wednesday, but due to the public holiday they were scheduled to be collected on Thursday 29 September. Unfortunately, the food scraps contractor emptied the bins of some households in our study that had put their bin out a day early. When spoken to, the contractor estimated that they had collected approximately 40 bins on Wednesday that were meant to be in Thursday's collection. Some of these bins were therefore not counted in the set out monitoring that took place on Thursday 29th. There was no way to adjust for these approximately 40 bins, as we had no idea to which specific households they belonged.

Also outlined in Section 2.8 was the issue of an overlap between intervention-delivery window and the first post-intervention monitoring round (T2). On Monday 12 September the sticker prompt intervention was delivered to 25 households who set their refuse bin out for collection that day and who had not previously received a sticker. The overlap was caused by delays printing stickers which meant that they could not be applied to refuse bins on the first two Monday collections during the intervention period, leaving just one Monday of the intervention period for all bins to get stickered. As significantly fewer bins had received the sticker prompt treatment than we had anticipated, we chose to run a catch-up day for applying the sticker to households' bins in this condition, which overlapped with the first day of participation monitoring for the post-intervention period.

Appendix G

Supplementary Results

Table A3

Regression Findings for Papakura Food Scraps Set Out Follow-Up Analysis

Parameter	Follow-up ITT				Follow-up ToT			
	Estimate	Est.Error	Q2.5	Q97.5	Estimate	Est.Error	Q2.5	Q97.5
Intercept	-3.19	0.36	-3.90	-2.49	-3.14	0.36	-3.82	-2.41
TimepointT2	-0.21	0.13	-0.47	0.04	-0.21	0.13	-0.45	0.05
TimepointT3	-0.56	0.13	-0.82	-0.32	-0.55	0.13	-0.81	-0.30
CONDITIONSticker	0.04	0.49	-0.90	1.01	-0.24	0.50	-1.24	0.75
CONDITIONPostcard	-0.40	0.51	-1.39	0.59	-0.39	0.50	-1.38	0.60
CONDITIONBinLiners	0.17	0.51	-0.81	1.17	0.14	0.51	-0.86	1.14
CONDITIONCanvassing	-0.67	0.51	-1.69	0.31	-0.32	0.58	-1.45	0.82
TimepointT2:CONDITIONSticker	0.47	0.18	0.11	0.83	0.61	0.21	0.19	1.01
TimepointT3:CONDITIONSticker	0.51	0.18	0.17	0.86	0.52	0.20	0.13	0.93
TimepointT2:CONDITIONPostcard	0.39	0.19	0.02	0.76	0.39	0.19	0.03	0.77
TimepointT3:CONDITIONPostcard	0.48	0.19	0.11	0.85	0.48	0.19	0.10	0.86
TimepointT2:CONDITIONBinLiners	0.21	0.18	-0.14	0.58	0.20	0.18	-0.15	0.55
TimepointT3:CONDITIONBinLiners	0.05	0.19	-0.31	0.42	0.05	0.18	-0.31	0.41
TimepointT2:CONDITIONCanvassing	0.08	0.20	-0.31	0.46	0.20	0.28	-0.35	0.74
TimepointT3:CONDITIONCanvassing	0.15	0.20	-0.25	0.53	0.40	0.28	-0.17	0.97

Note:

Table presents regression findings from follow-up repeated measures logistic regression models, with random effects for household and street cluster. Follow-up analysis excludes set out data for Monday 12 September for addresses receiving sticker on Monday 12 September. Coefficients are on the log odds scale for follow-up ITT (orange) and follow-up ToT (blue). Q2.5 and Q97.5 refers to Bayesian 95% Credible Intervals.

Table A4

Regression Findings for North Shore Food Scraps Set Out Follow-Up Analysis 1

Parameter	Intention to Treat				Treatment on Treated			
	Estimate	Est.Error	Q2.5	Q97.5	Estimate	Est.Error	Q2.5	Q97.5
Intercept	-3.87	0.28	-4.40	-3.30	-3.89	0.3	-4.47	-3.31
TimepointT2	0.31	0.09	0.14	0.48	0.33	0.1	0.14	0.54
TimepointT3	0.29	0.08	0.13	0.46	0.39	0.1	0.20	0.58

Note:

Table presents regression findings from follow-up repeated measures logistic regression models, with random effects for household and street cluster. Follow-up analysis excludes set out data for Wednesday Week 3 of T2 for Milford households. Coefficients are on the log odds scale for ITT (orange) and ToT (blue).
Q2.5 and Q97.5 refers to Bayesian 95% Credible Intervals.

Table A5

Regression Findings for North Shore Food Scraps Set Out Follow-Up Analysis 2

Parameter	Intention to Treat				Treatment on Treated			
	Estimate	Est.Error	Q2.5	Q97.5	Estimate	Est.Error	Q2.5	Q97.5
Intercept	-4.19	0.41	-4.97	-3.36	-4.22	0.40	-4.99	-3.41
TimepointT2	0.25	0.15	-0.04	0.54	0.24	0.15	-0.05	0.55
TimepointT3	0.16	0.14	-0.13	0.44	0.16	0.14	-0.12	0.44
CONDITIONPostcard	0.30	0.54	-0.78	1.36	0.37	0.49	-0.59	1.38
CONDITIONCanvassing	0.59	0.53	-0.47	1.61	0.67	0.59	-0.48	1.81
TimepointT2:CONDITIONPostcard	0.26	0.21	-0.14	0.66	0.28	0.21	-0.14	0.67
TimepointT3:CONDITIONPostcard	0.44	0.20	0.05	0.82	0.44	0.20	0.04	0.83
TimepointT2:CONDITIONCanvassing	-0.07	0.20	-0.46	0.32	-0.20	0.29	-0.79	0.37
TimepointT3:CONDITIONCanvassing	-0.03	0.20	-0.42	0.36	0.32	0.29	-0.24	0.87

Note:

Table presents regression findings from follow-up repeated measures logistic regression models, with random effects for household and street cluster. Follow-up analysis excludes set out data for Wednesday Week 3 of T2 for Milford Households.
Coefficients are on the log odds scale for ITT (orange) and ToT (blue).
Q2.5 and Q97.5 refers to Bayesian 95% Credible Intervals.

Table A6

Regression Findings for Papakura Food Scraps Participation Follow-Up Analysis

Parameter	Follow-up ITT				Follow-up ToT			
	Estimate	Est.Error	Q2.5	Q97.5	Estimate	Est.Error	Q2.5	Q97.5
Intercept	-2.76	0.48	-3.69	-1.80	-2.65	0.49	-3.62	-1.68
TimepointT2	-0.23	0.24	-0.70	0.24	-0.23	0.24	-0.70	0.24
TimepointT3	-0.51	0.24	-1.00	-0.05	-0.50	0.25	-1.00	-0.01
CONDITIONSticker	0.08	0.68	-1.23	1.37	-0.35	0.69	-1.70	1.01
CONDITIONPostcard	-0.26	0.67	-1.55	1.05	-0.28	0.70	-1.64	1.09
CONDITIONBinLiners	0.33	0.68	-0.97	1.65	0.32	0.68	-1.00	1.66
CONDITIONCanvassing	-0.88	0.67	-2.21	0.44	-0.26	0.79	-1.82	1.29
TimepointT2:CONDITIONSticker	0.60	0.35	-0.07	1.29	0.71	0.38	-0.04	1.47
TimepointT3:CONDITIONSticker	0.60	0.34	-0.06	1.29	0.44	0.40	-0.31	1.23
TimepointT2:CONDITIONPostcard	0.10	0.36	-0.62	0.78	0.15	0.36	-0.55	0.88
TimepointT3:CONDITIONPostcard	0.24	0.36	-0.45	0.94	0.28	0.37	-0.44	1.03
TimepointT2:CONDITIONBinLiners	0.19	0.35	-0.48	0.89	0.19	0.34	-0.51	0.88
TimepointT3:CONDITIONBinLiners	0.02	0.35	-0.70	0.68	0.00	0.36	-0.71	0.70
TimepointT2:CONDITIONCanvassing	-0.05	0.35	-0.77	0.62	-0.20	0.53	-1.25	0.82
TimepointT3:CONDITIONCanvassing	0.35	0.36	-0.34	1.08	0.75	0.54	-0.28	1.81

Note:

Table presents regression findings from repeated measures logistic regression models, with random effects for household and street cluster. Follow-up analysis excludes addresses that received the Sticker on Monday September 12.
Coefficients are on the log odds scale for follow-up ITT (orange) and follow-up ToT (blue).
Q2.5 and Q97.5 refers to Bayesian 95% Credible Intervals.

Table A7

Regression Findings for Papakura Food Scraps Participation Original and Follow-Up ITT Analyses

Parameter	Original ITT				Follow-up ITT			
	Estimate	Est.Error	Q2.5	Q97.5	Estimate	Est.Error	Q2.5	Q97.5
Intercept	-2.77	0.47	-3.72	-1.85	-2.76	0.48	-3.69	-1.80
TimepointT2	-0.22	0.25	-0.70	0.27	-0.23	0.24	-0.70	0.24
TimepointT3	-0.51	0.24	-0.98	-0.03	-0.51	0.24	-1.00	-0.05
CONDITIONSticker	0.14	0.66	-1.13	1.50	0.08	0.68	-1.23	1.37
CONDITIONPostcard	-0.27	0.67	-1.61	1.03	-0.26	0.67	-1.55	1.05
CONDITIONBinLiners	0.36	0.68	-0.98	1.71	0.33	0.68	-0.97	1.65
CONDITIONCanvassing	-0.85	0.67	-2.16	0.49	-0.88	0.67	-2.21	0.44
TimepointT2:CONDITIONSticker	0.64	0.35	-0.07	1.31	0.60	0.35	-0.07	1.29
TimepointT3:CONDITIONSticker	0.67	0.34	-0.01	1.35	0.60	0.34	-0.06	1.29
TimepointT2:CONDITIONPostcard	0.10	0.36	-0.59	0.80	0.10	0.36	-0.62	0.78
TimepointT3:CONDITIONPostcard	0.24	0.36	-0.46	0.94	0.24	0.36	-0.45	0.94
TimepointT2:CONDITIONBinLiners	0.17	0.35	-0.53	0.85	0.19	0.35	-0.48	0.89
TimepointT3:CONDITIONBinLiners	0.01	0.35	-0.67	0.70	0.02	0.35	-0.70	0.68
TimepointT2:CONDITIONCanvassing	-0.07	0.37	-0.82	0.67	-0.05	0.35	-0.77	0.62
TimepointT3:CONDITIONCanvassing	0.34	0.37	-0.37	1.07	0.35	0.36	-0.34	1.08

Note:

Table presents regression findings from repeated measures logistic regression models, with random effects for household and street cluster. Follow-up analysis excludes addresses that received the Sticker on Monday September 12.
Coefficients are on the log odds scale for original ITT (orange) and follow-up ITT (pink).
Q2.5 and Q97.5 refers to Bayesian 95% Credible Intervals.

Table A8

Regression Findings for Papakura Food Scraps Participation Original and Follow-Up ToT Analyses

Parameter	Original ToT				Follow-up ToT			
	Estimate	Est.Error	Q2.5	Q97.5	Estimate	Est.Error	Q2.5	Q97.5
Intercept	-2.61	0.52	-3.61	-1.54	-2.65	0.49	-3.62	-1.68
TimepointT2	-0.24	0.25	-0.74	0.24	-0.23	0.24	-0.70	0.24
TimepointT3	-0.49	0.24	-0.96	-0.01	-0.50	0.25	-1.00	-0.01
CONDITIONSticker	-0.30	0.70	-1.70	1.08	-0.35	0.69	-1.70	1.01
CONDITIONPostcard	-0.32	0.69	-1.69	1.05	-0.28	0.70	-1.64	1.09
CONDITIONBinLiners	0.34	0.69	-1.05	1.67	0.32	0.68	-1.00	1.66
CONDITIONCanvassing	-0.27	0.75	-1.72	1.23	-0.26	0.79	-1.82	1.29
TimepointT2:CONDITIONSticker	0.77	0.39	0.02	1.54	0.71	0.38	-0.04	1.47
TimepointT3:CONDITIONSticker	0.53	0.38	-0.21	1.30	0.44	0.40	-0.31	1.23
TimepointT2:CONDITIONPostcard	0.16	0.36	-0.56	0.86	0.15	0.36	-0.55	0.88
TimepointT3:CONDITIONPostcard	0.27	0.36	-0.44	0.94	0.28	0.37	-0.44	1.03
TimepointT2:CONDITIONBinLiners	0.19	0.36	-0.50	0.90	0.19	0.34	-0.51	0.88
TimepointT3:CONDITIONBinLiners	-0.01	0.35	-0.68	0.69	0.00	0.36	-0.71	0.70
TimepointT2:CONDITIONCanvassing	-0.22	0.52	-1.23	0.78	-0.20	0.53	-1.25	0.82
TimepointT3:CONDITIONCanvassing	0.72	0.50	-0.22	1.72	0.75	0.54	-0.28	1.81

Note:

Table presents regression findings from repeated measures logistic regression models, with random effects for household and street cluster. Follow-up analysis excludes addresses that received the Sticker on Monday September 12.
Coefficients are on the log odds scale for original ToT (blue) and follow-up ToT (purple).
Q2.5 and Q97.5 refers to Bayesian 95% Credible Intervals.

Table A9

Regression Findings for North Shore Food Scraps Participation Follow-Up Analysis 1

Parameter	Intention to Treat				Treatment on Treated			
	Estimate	Est.Error	Q2.5	Q97.5	Estimate	Est.Error	Q2.5	Q97.5
Intercept	-3.41	0.54	-4.45	-2.32	-3.31	0.57	-4.36	-2.09
TimepointT2	0.39	0.21	-0.02	0.79	0.47	0.23	0.03	0.91
TimepointT3	0.03	0.21	-0.37	0.44	0.01	0.23	-0.44	0.47

Note:

Table presents regression findings from follow-up repeated measures logistic regression models, with random effects for household and street cluster. Follow-up analysis excludes households in Milford with a Wednesday collection day.
Coefficients are on the log odds scale for ITT (orange) and ToT (blue).
Q2.5 and Q97.5 refers to Bayesian 95% Credible Intervals.

Table A10

Regression Findings for North Shore Food Scraps Participation Follow-Up Analysis 2

Parameter	Intention to Treat				Treatment on Treated			
	Estimate	Est.Error	Q2.5	Q97.5	Estimate	Est.Error	Q2.5	Q97.5
Intercept	-3.55	0.74	-4.89	-1.97	-3.54	0.72	-4.86	-2.05
TimepointT2	0.47	0.30	-0.13	1.06	0.48	0.30	-0.11	1.08
TimepointT3	-0.03	0.30	-0.62	0.56	-0.03	0.30	-0.62	0.54
CONDITIONPostcard	0.17	0.81	-1.47	1.71	0.26	0.78	-1.29	1.77
CONDITIONCanvassing	0.20	0.78	-1.35	1.72	0.48	0.83	-1.16	2.14
TimepointT2:CONDITIONPostcard	-0.02	0.43	-0.87	0.82	-0.02	0.44	-0.89	0.82
TimepointT3:CONDITIONPostcard	-0.03	0.44	-0.88	0.83	-0.02	0.43	-0.84	0.83
TimepointT2:CONDITIONCanvassing	-0.18	0.43	-1.00	0.65	0.00	0.59	-1.13	1.14
TimepointT3:CONDITIONCanvassing	0.22	0.43	-0.61	1.09	0.41	0.58	-0.74	1.56

Note:

Table presents regression findings from follow-up repeated measures logistic regression models, with random effects for household and street cluster. Follow-up analysis excludes households in Milford with a Wednesday collection day. Coefficients are on the log odds scale for ITT (orange) and ToT (blue).

Q2.5 and Q97.5 refers to Bayesian 95% Credible Intervals.

Table A11

Participation in the North Shore Food Scraps Service by Suburb, for Each Timepoint and Overall

	Timepoint			
	T1	T2	T3	Total
Milford	0.37	0.38	0.37	0.37
Northcote	0.19	0.22	0.19	0.20
Takapuna	0.26	0.28	0.28	0.27

Note: Table displays participation proportions for each suburb in the North Shore trial area and for overall (Total) participation.

Table A12

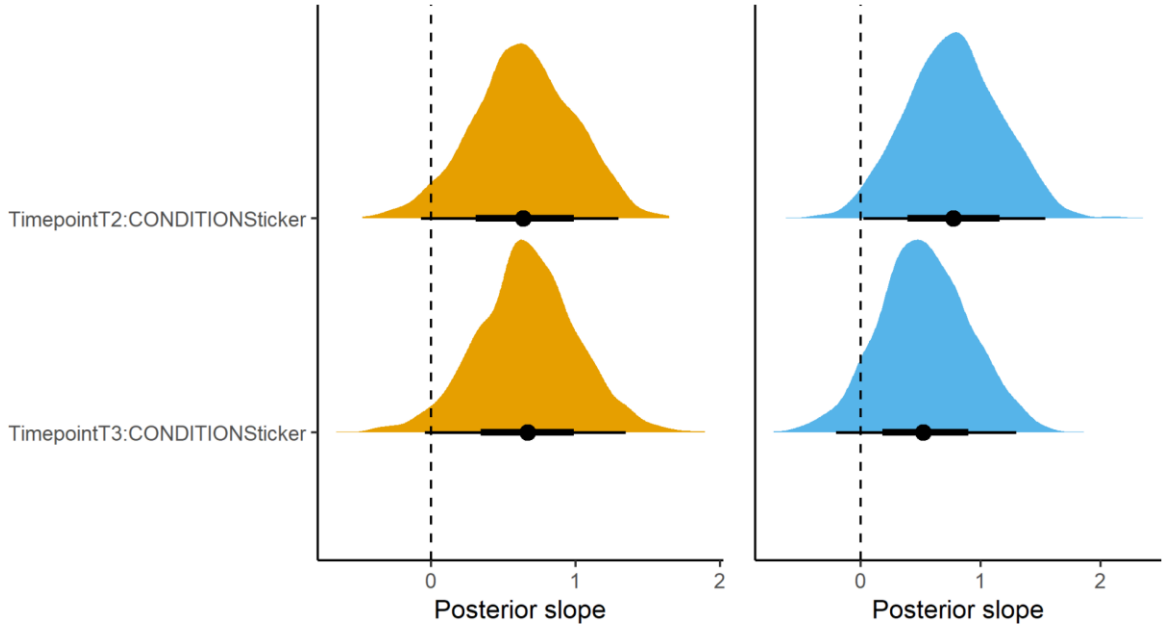
Estimates or the T2:Sticker Coefficient, for Set Out and Participation in the Papakura Trial

	Set Out Estimate	Participation Estimate
ITT	1.28	1.52
TOT	1.48	1.72

Note: Table displays posterior mean OR estimates for the T2:sticker parameter for set out analyses (left) and participation analyses (right).

Figure A3

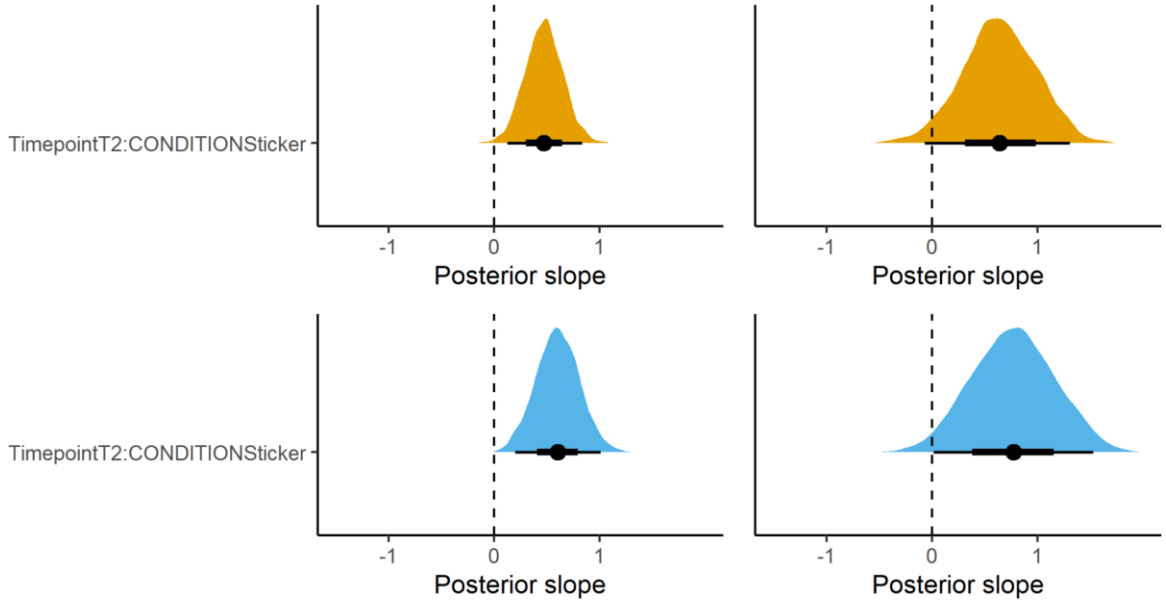
Posterior Distribution of Sticker Interaction Parameters for Papakura ITT and ToT Participation Models



Note: Figure shows parameters from ITT model for sticker at T2 (top left) and T3 (bottom left) and ToT model for sticker at T2 (top right) and T3 (bottom right). Points are median posterior estimates, thick black lines are 66% CIs, and thin black lines are 95% CIs.

Figure A4

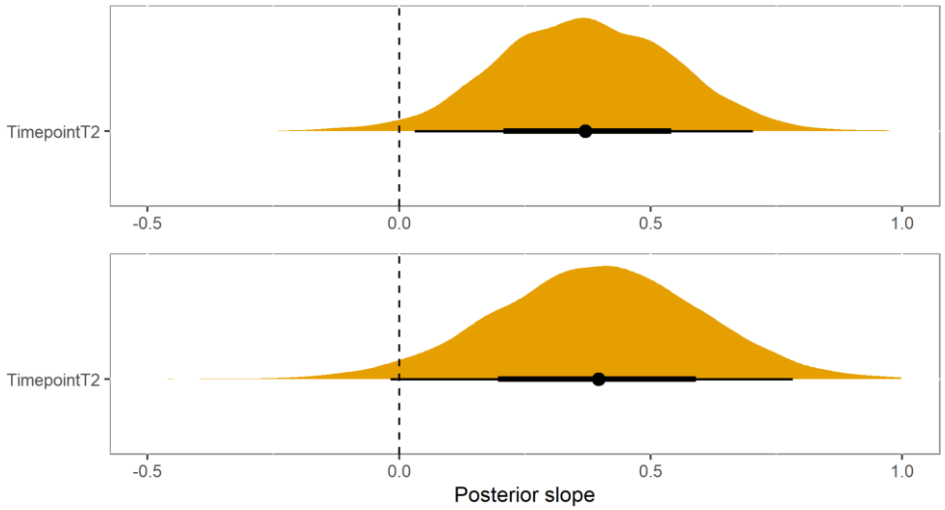
Posterior Distribution of T2:Sticker Parameter for Papakura ITT and ToT Set Out (left) and Participation (right) Models



Note: Figure shows parameter for sticker at T2 from set out ITT model (top left), set out ToT model (bottom left), participation ITT model (top right), and participation ToT model (bottom right). Points are median posterior estimates, thick black lines are 66% CIs, and thin black lines are 95% CIs.

Figure A5

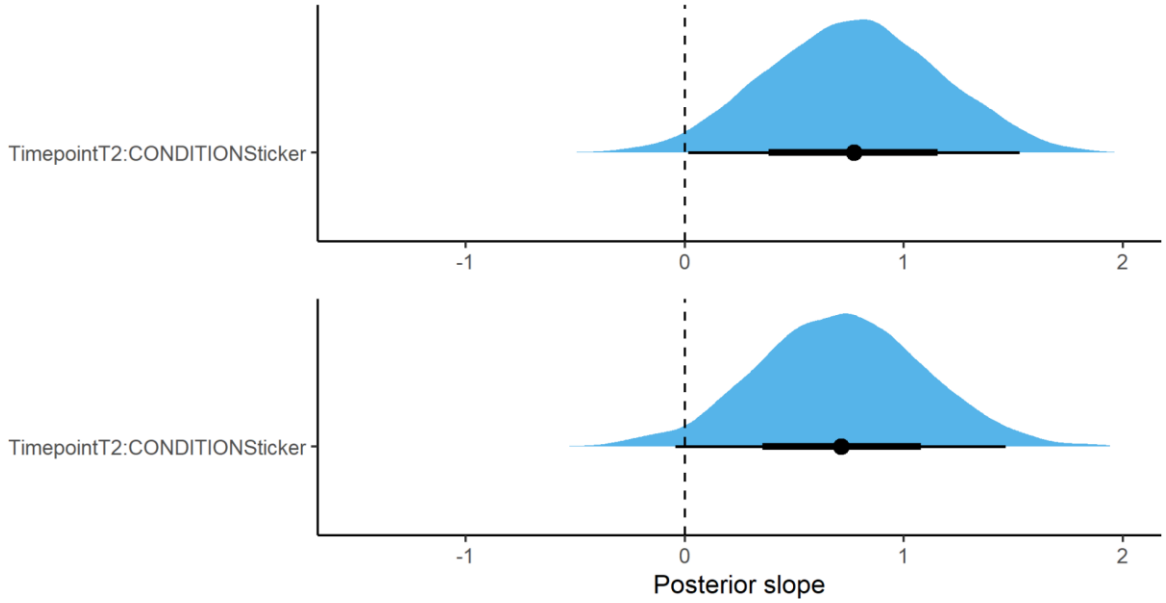
Posterior Distribution of T2 Parameter for Original and Follow-up North Shore ITT Participation Models



Note: Figure shows T2 parameter from original participation ITT model (top), and follow-up participation ITT model (bottom) testing H3a. Points are median posterior estimates, thick black lines are 66% CIs, and thin black lines are 95% CIs.

Figure A6

Posterior Distribution of T2:Sticker Parameter for Original and Follow-up Papakura ToT Participation Models



Note: Figure shows parameter for sticker at T2 from original participation ToT model (top) and follow-up participation ToT model (bottom). Points are median posterior estimates, thick black lines are 66% CIs, and thin black lines are 95% CIs.

Appendix H

Supplementary Discussion

Additional information about the cost-benefit analysis for the sticker prompt.

A number of assumptions were made when completing the cost benefit analysis for the sticker prompt intervention.

- The figure used for the total number of households included if the sticker was applied across the whole of Auckland urban area was 463,358. This was calculated in the following way:
    - Dividing the total population of Auckland Urban Area (1,440,300) by the total population of Auckland Region (1,695,200) finds that 85% of the population of Auckland lives in Urban Auckland (Stats NZ, n.d.-a; Stats NZ, n.d.-f; Stats NZ, n.d.-g). The total number of private dwellings in Auckland Region is 545,127 (Stats NZ, n.d.-e). The total number of private dwellings in Urban Auckland was then estimated (545,127 x 0.85): 463,358.
- Baseline participation in the current Auckland Council food scraps service area was estimated as 33%. This figure is from our descriptive statistics estimate from the Papakura and North Shore trails baseline monitoring period. It is the mean of the North Shore and Papakura figures. This figure is very similar to estimates from previous participation surveys conducted by Auckland Council.
- Total food waste collected p.a. from households in the current Auckland Council food scraps service area using the service is estimated to be 1,022,805 kg (1,022 tonnes). This figure was found by taking the average of the monthly weights for January-August 2022 (prior to our interventions being delivered), then calculating the average weight (kg) across a 12-month period.[20]
- An estimate of the number of participating households in the current Auckland Council food scraps service area was found to be 7,208 households. This figure was found by

[20] Monthly weights were provided by Auckland Council.

multiplying the number of households in the current food scraps areas in Papakura and the North Shore (19,743 + 2,100) by the baseline participation estimate (0.33).

- Total average food waste (in tonnes) per current participating household p.a. was then estimated to be 0.14 tonnes (1,022 tonnes / 7,208 households).
- Next some inferences were made to find the expected number of participating households and the expected weight of total food waste collected *pre-intervention*, if the service was offered across the whole of Urban Auckland. These figures were calculated as follows:
    - Expected number of participating households pre-intervention (if the service was offered across all of Urban Auckland): 154,935 (0.33 × 463,358)
    - Expected total weight of food waste collected pre-intervention (if the service was offered across all of Urban Auckland): 21,984.48 tonnes (154,935 × 0.14)
- To find the expected number of participating households and the expected weight of total food waste collected *post-sticker intervention*, posterior samples from our model for ToT participation in Papakura were used. This involves an assumption that all, or almost all, houses receive a refuse bin sticker (we used posterior samples for the sticker parameter from the ToT model since the ITT model did not produce unambiguous effects of the sticker on participation).
    - Expected number of participating households after sticker intervention (if the service was Auckland wide): 212,426, 95% CI [140866, 282,647] (model estimate × 463,358)
    - Subtracting the expected number participating at baseline from the expected number participating post-intervention, there would be an estimated 59,518 extra households participating after the sticker intervention (95% CI [-12041.94, 129739.10])
    - Expected total weight of food waste collected after sticker intervention (in tonnes) per annum (for all of Auckland): 30,142, 95% CI [19988, 40106] (model estimate × 0.14).

    - Subtracting the expected total weight of food waste collected at baseline from the expected total weight collected post-intervention, there would be an estimated 8,445 extra tonnes of food waste collected after the sticker intervention applied across Urban Auckland (95% CI [-1709, 18,409]).
- The cost of the sticker is estimated to be $0.54 per sticker. This was the cost of the printing of the sticker, per unit in the study.
- The total cost of sticker intervention (when applied Auckland-wide) is estimated to be $250,213 (0.54 × 463,358)
- The expected cost per tonne of extra food waste collected was calculated to be $27.40, 95% CI [-148.28, 298.82]. This figure is based on the current food scraps collection per household estimate of 0.14 tonnes p.a.

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