Mobilizing citizen effort to enhance environmental outcomes: a randomized controlled trial of a door-to-door recycling campaign

Abstract

This paper tests whether doorstep canvassing can raise participation in curbside recycling using a randomized controlled trial. Existing research shows that canvassing can confront negative attitudes, increase understanding and resolve structural obstacles, but there is less known about the longitudinal effects of such interventions, which may fall away over time. 194 streets in Trafford, in the North West of England, UK were randomized into a treatment and a control group. Random-effects multilevel regression models, controlling for baseline recycling, street size, deprivation and size of ethnic minority population, show that the canvassing raised recycling participation ratios for the treatment group, but there was a decline in the impact of the intervention over time. The intervention was more effective on streets with low levels of recycling at baseline.

Three key words: Canvassing Recycling Experiment

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INTRODUCTION

It is widely acknowledged that the actions of citizens are essential to implement policies for a better environment, largely through modifying their individual behaviors (Jackson 2005). Creating environmentally positive outcomes is a massive task for the twenty-first century as societies across the planet face the challenge of dealing with climate change and disposing of the products of an increasingly consumerist society. It is possible that much of the change will happen through growing awareness of the problems, in reaction to market incentives, or in response to information and regulation from government and other actors. But it is also likely that interventions will be needed, which involve some direct contact with the citizens. While some of these could be the result of demonstration projects or the recruiting of community champions, it is likely that face-to-face contact between an advocate and the citizens has a good chance of achieving a desirable outcome. The state or the voluntary sector may recruit a body of activists to mobilize the citizens to behave differently, much in a similar way that canvassing can get people to vote, for example (Green & Gerber, 2008). The voting literatures and existing studies suggest an effect based on the communication of information in a face-to-face context, a degree of persuasion and taking an interest in the person. With some activities, such as change in car use, it is hard to imagine such a change taking place without a considerable effort in persuasion. But many other activities require modest lifestyle adjustments, with reminders and cues being effective at bringing about behavior change. Here curbside recycling is such an activity, where households leave out recyclable waste for collection by the local authority.

This paper provides an experimental test of an intervention designed to increase recycling. A number of studies have been conducted to test the effectiveness of different interventions to raise participation in curbside recycling schemes, comparing areas or households with or without the intervention (examples include Bryce, Day & Olney, 1997, Woodward, Bench & Harder, 2005, Timlett & Williams, 2008). Schultz (1998) conducted a randomized controlled trial examining the impact on recycling behavior of providing written feedback on individual and neighborhood recycling behavior. Lyas, Shaw & Van Vugt (2004) undertook a randomized controlled trial to examine the impact of different types of motivational leaflets. This paper presents the first randomized controlled trial to test the impact of a door-to-door canvassing campaign. Recycling behavior was measured by observing which households put out a recycling container for collection over a three-week period. We randomly assigned half the streets to be canvassed with half of the streets placed in a control group and received no special attention. Recycling participation rates for all households were measured after the intervention. Our research study is also unique in that it measures the impact of a recycling campaign three months after the intervention so is able to understand the longer term impact of such an intervention rather than a short-term fillip.

The paper first reviews the literature on interventions designed to improve curbside recycling, the contribution of street effects, the role of habit, the importance of baseline rates and the impact of socio-demographic variables on recycling behavior. Following the CONSORT guidelines for reporting randomized controlled trials (Moher, Schulz, & Altman, 2001), it then sets out the research design and methods, describes the analytical approach, reports the results of the experiment, and then concludes with a discussion of the implications of the findings for recycling and mobilization campaigns and policy. We report the CONSORT flow diagram in Figure 1 and the checklist in Appendix 1.

BACKGROUND

Interventions to raise participation

There are a number of potential approaches to encouraging recycling. They range in type, appealing to different kinds of motivation. Promotional and educational campaigns can raise participation rates by ensuring people understand the scheme and motivating people to get involved. High visibility events and road-shows can be successful in building awareness (Read, 1999), but face to face contact on the doorstep is more effective than simply providing literature (Bryce, Day, & Olney, 1997; Reams & Ray, 1993). Door-stepping has not been found to be effective in increasing set out rate or reducing contamination in areas where recycling is already high (> 60 per cent of households) but it does raise the range of materials recycled (Timlett & Williams, 2008). This is the approach adopted in the current research: there is no compulsion, just the direct approach by a friendly and engaging team. Such kinds of intervention have increased participation in elections by about seven percent in the UK (John & Brannan, 2008); replicating the effect sizes in US studies (Green and Gerber 2008). This intervention may be contrasted with social influencers, the use of existing recyclers as role models to encourage others to participate (Bryce, Day & Olney, 1997) or incentives, which work particularly well in areas with low recycling participation rates (Harder, Woodard, & Bench, 2006). However, it may be that incentives act to crowd out intrinsic motivation (Bryce, Day & Olney, 1997), and a recent survey of householders in London found that recycling scheme improvements and active promotion are likely to be more effective than financial incentives in encouraging recycling (Shaw & Maynard, 2008). Another alternative is asking people to make a verbal pledge or commitment to recycle (Bryce, Day & Olney, 1997; Reams & Ray, 1993). A study of student housing found that pledges secured through personal contact led to higher recycling rates than pledges through indirect contact or providing information, but it may be the personal contact that makes the

difference here rather than the act of making a commitment (Bryce, Day & Olney, 1997; Reams & Ray, 1993), reinforcing the usefulness of a door-to-door campaign. Feedback cards left by collection crews to highlight boxes that contain contaminated material can be effective in reducing the amount of contamination and it is a cheap approach (Timlett & Williams, 2008).

A few studies have tried to change recycling behavior by activating personal or social norms. The provision of feedback leaflets to households giving borough-wide recycling participation rates had no effect in changing householder behavior in a London borough, regardless of whether the feedback was presented in a positive, negative or neutral way (Lyas, Shaw, & Van Vugt, 2004). The provision of more specific feedback which states the recycling activity of the individual household or the surrounding streets during the current or previous week was successful in raising the frequency of participation and the amount of material recycled (Schultz, 1998).

Street and Area Effects

The unit of the intervention needs to fit in with the actual spaces that people live in, which can be blocks, streets or neighborhoods. Streets are important units of analysis because they are reference groups for households, and the place where their behavior may be observable by neighbors. Observation of the recycling boxes by each household may stimulate more recycling and help an intervention through peer effects: households are more likely to recycle if they see that others in their street are doing the same and they may be influenced by social norms that develop over recycling as more people do it. Theories of collective action imply this is the case: that the members of a unit, such as a street, will tend to act in relationship to each other. If one person can see another's activity, they will feel

encouraged to behave in a more community-based way. Of course, streets may be too big to serve this purpose with too many households split up by natural breaks within the street.

Several studies evaluate the spatial scale of recycling. Tucker (1999) examined the impact of social norms within streets, using data from five newspaper recycling schemes in Northern England and Scotland. They found that there is a normative influence - households are more likely to set out a recycling box if others in their street recycle regularly. Living on a street with high participation rates may encourage non- or infrequent recyclers to regularly recycle small amounts of material, rather then increasing the absolute amount of material recycled (Tucker, 1999). Shaw (2008) monitored the recycling behavior of 1700 households in a London borough. Contiguous blocks of households were identified by sectioning streets, taking account of road intersections, open spaces, non-domestic buildings etc. In shorter contiguous blocks (less than 15 houses) householders were influenced by the recycling actions of their nearest next door neighbors. The influence was more marked in cul-de-sacs than in linear streets. The influence of neighbors diminished as the length of the blocks increased. This study considers only interactions with nearest immediate neighbors, not a wider street effect (Shaw, 2008). Harder, Woodard, & Bench (2006) found that households on small roads tend to have a higher recycling participation rate. Suggested factors include increased attachment to the neighborhood, community spirit and peer pressure, but the reasons are not explored in depth and this is identified as a potential area for future research.

Recycling, like other behaviors, takes place in nested contexts including streets, and neighborhoods where people are clustered together and households within those contexts have some similarities along social dimensions such as levels of income and the composition of those neighborhoods by different ethnic and social groups. These nested features of individual actions have implications for the study design and the statistical modeling. We chose a street based research design to avoid the contamination that might have arisen if

randomization was done at a household level and we conducted the analysis at both the street and super output area level.

Habit

Less discussed in the literature is the importance of habit and how this may interact with a recycling campaign. Recycling is itself habitual, involving regular sets of activities at the same time of the week. Once a pattern is in train, then by doing the act, it then becomes normal and occurs without thinking. If a campaign succeeds in raising recycling, new habits may form and persist into the future, without the need for on-going persuasion but done as a reflex action. Psychological studies of "foot in the door" techniques seem to reinforce this: current participation makes future participation more likely (Freedman and Fraser, 1966; Cialdini, Trost and Newsom, 1995), which is based on strengthening self-perception with people determining their attitudes from observing their own behavior (Beaman, Cole, Preston, Klentz and Mehrkens Steblay, 1983; Burger and Caldwell 2003). To these positive effects of doorstep campaigning on recycling habits, we should also pose the contrary hypothesis – that changes to behavior might be temporary, with citizens returning to their long-term habits. That raises the question about how long the habit effect lasts for, and how many months after which we might expect the effects of a door-to-door campaign to dissipate. Promotional campaigns tend to be most effective in the short term, with a high initial rise in response to a campaign and a falling off over time (Timlett & Williams, 2008; Woodward, Bench, & Harder, 2005). Despite this recycling decay an effect can still be measured several months later (Woodward, Bench, & Harder, 2005).

The importance of the baseline

Given the importance of habit, we may expect that interventions will need to work with different starting points that will be hard to shift. Thus those with high levels of activity may carry on in that vein; and so too those who have not such a high participation rate. While shifting habit is hard, it is likely that different levels of recycling may react to the treatment in different ways. Arceneaux and Nickerson (2009) theorize that there are different relationships between the propensity to vote and the impact of a mobilization campaign. Aggregating the results across eleven vote experiments, they find that the lower baseline increases the impact of the treatment, though this is weakened at very low baselines. With recycling, it is likely that a lower baseline will offer more susceptibility to treatment because a campaign is likely to provide information and incentive to act differently. It is also possible to control for the effects of deprivation on reducing the impact of a treatment.

The covariates of recycling

Like any public action, recycling is affected by socio-demographic variables. Surveys indicate that non-recyclers tend to be younger, less affluent and live in rented accommodation, while recyclers, in contrast, tend to be more mature, more affluent, home owners and better educated (see Martin, Williams and Clark 2006 for a review). It may be that households from lower socio-economic groups tend to devote less effort to recycling because their economic and social deprivation mean they face more pressing needs (Martin, Williams and Clark 2006).

Neighborhoods with a high proportion of ethnic minorities tend to have lower recycling rates, but these often coincide with areas of high deprivation and mobility and little research has been done to separate out these effects. Case studies in two ethnically diverse areas of the UK found that ethnic origin had no significant difference on the participation of individuals in curbside recycling (Martin, Williams and Clark 2006, Perry and Williams,

2007). One study identified differences between first and second generation ethnic minorities: second generation ethnic minorities had adopted stronger environmental concerns including pro-recycling attitudes and used much more processed and packaged foods, producing more recyclable waste. For those who did not recycle, the main reasons were not having a box, not being sure of the collection day or other barriers like lack of space and lack of understanding, suggesting that recycling can be promoted by making it easy to do and providing encouragement and information. Among some religious and ethnic groups, there is a particular emphasis on re-using clothing, plastics and glass, either using them for another purpose in the home or via charity organizations (Perry and Williams 2007).

There is some limited evidence that transience is an issue here (Perry and Williams 2007). It is likely that neighborhoods with high levels of deprivation and a high proportion of ethnic minorities also have a high population turnover. Households moving to an area for the first time are unlikely initially to understand the local waste management system and they may have more immediate demands on them than mastering a system involving a number of different waste containers and collection days. In these circumstances, it might be advantageous for service providers to target canvassing campaigns in areas of high population turnover. This again points to the importance of baseline interactions.

Research Objectives

The rationale for the study is that a field-force can appeal to people's environmental motivations reminding them of their underlying willingness to contribute. While previous experiments have examined the impact of written feedback (Schultz, 1998; Lyas, Shaw, & Van Vugt, 2004) or leaflets and pledges (Bryce, Day, & Olney, 1997), this is the only field experiment to measure the impact of canvassing on curbside recycling and it is the first to

consider the longitudinal effect of an intervention to raise recycling. The central research question is to ask whether, to what degree and for how long a door-to-door campaign can raise the recycling level.

H_1: The canvass group will have a higher level of street level recycling than the control group post canvassing campaign.

The second hypothesis tests the idea that the impact of the intervention lasts three months after the canvassing campaign, so that the intervention is sustained over time. This tests the idea that the canvassed group has developed a habit of recycling.

H_2: The canvass group will have a higher level of recycling than the control group over time.

Finally, we expect the treatment to vary according to the baseline rate, but negatively.

H_3: The canvass group will respond more to the treatment at lower baseline rates

RESEARCH DESIGN AND METHODS

The study site

We focused on one geographical area within which to do the research. This is very common in mobilization or canvassing campaigns because of the economics of a field force needed to cover an area, and in this case also needed because of the localized nature of recycling services. There is no reason to believe the area we selected is very different from others of its type, or that we would observe very different effects in other cities in the UK. The research was conducted in two adjoining neighborhoods, Old Trafford and Gorse Hill, which are within Trafford Metropolitan Borough Council (MBC), close to inner city Manchester. Old Trafford is close to Manchester City Centre. It is an ethnically diverse area.

Among our sample streets in Old Trafford, 47 per cent of its residents are from an ethnic minority. The largest single ethnic group is Asian or Asian British, of Pakistani and Bangladeshi origin. It is a relatively deprived area: seven of the ten super output areas[1] in Old Trafford are within the lowest twenty per cent of all areas in England in terms of multiple indicators of deprivation and three areas are within the lowest six per cent. Most of the housing is traditional low-rise properties, both terraced and semi-detached. Gorse Hill is further from the City Centre. It is less ethnically diverse, with 13 per cent of residents being from an ethnic minority. It is relatively deprived in national terms, but less deprived than Old Trafford: its five super output areas are in the lowest 20-30 per cent of areas in England. Most of the housing is traditional semi-detached and terraced. Table 1 gives the background statistics.

[Table 1 about here]

The recycling service

The curbside recycling service is provided by EMERGE, a not-for-profit organization commissioned by Trafford metropolitan borough council to provide a weekly recycling service to the target streets. It collects a very broad variety of materials: households have a box (for glass bottles, cans, thin card, directories and textiles) with three separate bags for paper, plastic bottles and batteries. The box system offered here is probably easier for the terraced households, where wheeled bins are problematic because of storage and access issues, but they may be less convenient for the houses with driveways. Households have a weekly recycling collection. There is currently no co-ordination with the council's residual

[1]. Super output areas are boundaries used by the UK Office for National Statistics to provide statistics at a local level.

waste collection, so some households may have collections on two different days each week. The vehicle collecting recycling waste is visibly different from residual waste collection vehicles and materials are sorted as they are placed in the van, giving a clear message that the service is trustworthy and recyclables will not end up in landfill sites (Woodward, Bench, & Harder, 2005). If households leave out contaminated boxes (including non-recyclable waste or placing waste in the wrong containers), they are left with a card explaining how the service works.

Street-based design

We chose a street based research design, using the street as the unit of analysis. The primary reason was that we expected household recycling behavior would be influenced by other households in the street: households are more likely to recycle if they see that others in their street are doing the same and they may be influenced by social norms that emerge over recycling. We therefore anticipated that canvassing one household might have an effect on the behavior of its neighbors in the control group, which would contaminate the experiment. A street based design reduced the possibility of such contamination.

The number of streets was determined by the coverage of the recycling service, allowing us to hold constant the level of service offered to each household. Flats and commercial properties were not included in the study because they are not eligible for the recycling service. All streets in Old Trafford and Gorse Hill with at least one residential property were eligible for inclusion in the research study. There were a total of 194 streets, with 6,580 households, ranging in size from two households to 190, with an average of 33.9 households per street.

The intervention

One of four canvassers visited all households in the streets in the intervention group. The canvassing focused on three factors that are expected to influence recycling behavior: awareness, attitudes and structural barriers (Shaw, Lyas, Maynard, & Van Vugt, 2007). Canvassers made sure householders were aware of the service by confirming the day and time of collection, explaining the variety of materials that can be recycled. They promoted positive attitudes to recycling and were enthusiastic about encouraging people to take part. They addressed barriers to recycling by providing any plastic bags as required and ordering new boxes if they were lost or missing. They dealt with any problems or queries about the service or passed any difficult queries onto an EMERGE manager. The canvassers were encouraged to be enthusiastic and conversational on the doorstep. They were provided with scripts to use as prompts but were encouraged to adapt them to their own conversational style.

Canvassers were asked to take a different approach dependent on whether the household were currently recyclers or non-recyclers (Shaw, Lyas, Maynard and Van Vugt, 2007). Existing recyclers were thanked for using the recycling box, reminded of the variety of recyclable materials and – if they seemed enthusiastic - asked if they would like to become recycling champions. Non-recyclers were encouraged to recycle, informed of the day and date of collection and materials collected. They were then asked if they could be counted on to recycle regularly. Both groups were asked if they had any questions or concerns about the service and if they needed replacement receptacles. An information leaflet was given to every household canvassed and was also delivered to houses where no one was at home. The leaflet described what materials could be recycled, outlined the service provided, gave details of the time and day of collection and provided contact details for more information.

A log sheet was produced for each street. It listed every household on the street, with an indication of whether they currently recycle or not. Canvassers entered the date and times

that the street was canvassed. The information was used to monitor when the best times to canvass were and to ensure a different time for follow up visits. Canvassers completed the sheets to show which houses had been canvassed and whether additional bags had been provided and new box requests were entered. About 500 new boxes were requested. There was space to note if there was no one in the household who spoke English well enough for the canvassing to take place. The intention had been to re-visit these households with a translator, but the number of cases was lower than anticipated (29 households) and the number of languages was very varied, so the re-visits were not undertaken. Volunteer recycling champions were noted on the sheet and contacted by EMERGE at a later date. We found that 29 people had volunteered.

The four canvassers were specifically recruited and trained for the task. The canvassers received one day's training on the EMERGE recycling service, the benefits of recycling, possible arguments against recycling, canvassing issues and practical issues relating to health and safety. The canvassing took place over a period of six weeks in May and June 2008 between 3pm and 7pm Monday to Friday and 11am – 3pm on Saturday. These times were chosen to maximize the number of contacts, based on previous best practice (WRAP, 2006b). After all the selected intervention streets had been canvassed once, a repeat canvass of the whole area was done, calling at all the households who had not been contacted on the first occasion. These second visits were arranged at a different time of day from the first visit to maximize contact. During the first canvass 40 per cent of households were spoken to. By the end of the second canvass, 61 per cent of households had been contacted, 2,129 of the 3,468 households in the intervention group. The rate of contact rate of 61 per cent compares favorably with other canvassing projects (WRAP, 2006b).

Outcome Measurement

We measured recycling behavior by observing which households put out a recycling container for collection. Questionnaires asking people to self-report their recycling activity are sometimes used, but they tend to be unreliable, over-reporting the extent of recycling activity (Shaw, Lyas, Maynard, & Van Vugt, 2007; Timlett & Williams, 2008). The monitoring was done on the same day as the waste collection. The monitor sat in the recycling vehicle while the crew were working and noted all the houses on the street that had placed recycling material outside the house boundary. The monitoring was repeated over three consecutive weeks: some households may not recycle weekly because of holidays or having low levels of recyclable waste. Any household who recycled at least once in the three-week period is counted as a recycler, following the most recent guidance from the Waste and Resources Action Programme, supported by the environment department, Defra, (WRAP, 2006a).

The participation ratio of each street was calculated as the proportion of households placing material out for collection at least one week in three:

$$\text{Participation ratio} = \frac{\text{No. of households placing material out for collection at least once in 3 weeks}}{\text{Total no. of households}} \times 100$$

Where the impact of an intervention is being studied, measurement is usually done shortly before the intervention and then repeated immediately afterwards (Read, 1999). But there is often an increase in recycling in response to an intervention and then a drop off in participation as time passes. Repeating the measurement a third time a few months later can take account of this recycling decay (Woodward, Bench, & Harder, 2005) and test whether habits have been formed and sustained. Participation in the recycling scheme was measured

for all households in the intervention and control groups at three time points: in March/April 2008 prior to the canvassing, in July 2008 after its completion in October 2008 to test the habit effect. Monitoring was not done at bank holidays (because services were disrupted) or during school holidays (when some households might be away).

Sample size and randomization

Prior to the randomization, in association with the Trials Unit at the University of York, we estimated that we would have 80 per cent power to show a difference of 11 per cent between the two groups assuming an estimate of the correlations between the pre and post recycling ratios of 0.8, based on a small sample of existing data, which we had from EMERGE. If the correlation turned out to be higher then our power would increase or the difference we could detect would fall.

The data was handed over to the York Trials Unit for randomization, using the SPSS random sampling function, to an intervention and control group of equal size. The data was stratified by district (Old Trafford or Gorse Hill) and street length prior to randomization.[2] The treatment group contained 3,468 households in 97 streets and the control group contained 3,112 households in 97 streets.

[Figure 1 about here]

Blinding

Households were not informed that they were taking part in a research study: those in the canvassed group did not know that some other streets were not being canvassed and those

2. We thank Professor David Torgerson of the York Trials Unit, Department of Health Sciences, University of York, who executed the randomisation

in the control group did not know that some other streets were being canvassed. Both groups were blind to the fact that their recycling was being monitored. This was ethically approved because no personal data was collected during the study. The canvassers who administered the intervention were not blind: they knew they were delivering the canvassing as part of a research study and understood that the canvassed streets would be compared to control streets. They were encouraged to be very careful in implementation to avoid contaminating any of the control streets by canvassing them in error. The three sets of participation monitoring were each done by a different person, none of whom were involved in any other aspect of the project, and who were unaware which streets were in the treatment and control groups. The members of the recycling collection crew were aware of the research project, but did not know which streets were in the treatment and control groups.

Data and variables used in the analysis

The dataset has fifteen super output areas, boundaries used by the UK Office for National Statistics to provide statistics at a local level. The data set has two levels, with level one units being the streets and level two units being the super output areas. The data is longitudinal in the sense that the participation ratio was measure on three occasions: prior to the canvassing campaign, immediately afterwards and three months later.

The outcome variable is the follow-up (or post intervention) recycling participation ratio, the percentage of households per street observed placing recyclable material out for collection at least once in three weeks. The explanatory variables include four street level variables and two super output area level covariates. The street level variables are: canvass group: coded 0 = not canvassed, 1 = canvassed; baseline participation ratio: percentage of households per street observed placing recyclable material out for collection at least once in three weeks at time one (prior to the canvassing campaign); times: coded 1 = first follow-up

participation ratio, 2 = second follow-up participation ratio; street size: total number of households in a street. The super output area level covariates are: percentage of population that is non-white (source: 2001 census, Office for National Statistics); index of Multiple Deprivation (IMD) score: higher score means greater deprivation (source: 2007 index, Office for National Statistics). Intention to treat analysis was adopted: all of the households in the intervention group were included in the analysis, whether or not they were actually canvassed. Appendix 2 contains a table of descriptive statistics.

RESULTS

Descriptive statistics

The descriptive statistics in Table 2 confirm that the mean participation ratio in the canvassed streets rose from 47.7 per cent before the campaign to 52 per cent immediately afterwards and then rose again to 52.6 percent three months later. Among the control group streets the mean participation ratio fell from 54 per cent to 52.9 per cent but then rose to 57.2 per cent. When comparing first follow-up to baseline, recycling participation in the canvassed group rose by 4.3 per cent whilst the control group fell by 1.1 per cent, so overall, in the short term, the treatment group improved its recycling by 5.4 per cent in comparison to the control group. When comparing second follow-up to baseline, recycling participation in the canvassed group rose by 4.9 per cent and the control group rose by 3.2 per cent, so overall, in the longer term the treatment group improved its recycling by 1.7 per cent in comparison to the control group. So, among those streets that were canvassed, the mean number of households taking part in recycling rose immediately after the canvassing. The rise was not only sustained, but rose three months later. Meanwhile, among the streets that were not canvassed, the mean number of households taking part in recycling fell slightly after the

intervention, but it then rose, so that three months later participation was higher than at the start. Table 3 presents the differences in the means across the whole time period. The change within the treatment group was significant, but the change in the control group was not. The difference of 1.7 per cent between the baseline and the follow up two periods later was not significant (t=.98). When compared to the control group, the effect of the canvassing lessened over time.

Tables 2 and 3 about here

These results indicate that something outside the experiment caused recycling ratios to dip immediately after the campaign, but to rise three months later to higher levels than before the campaign. One reason is that at the time of the canvassing and the post-campaign monitoring the recycling collection crew was short-staffed and there was a reliance on casual staff. The canvassers found that there was a minority of householders who complained of missed collections, rude staff, dirty boxes and pedantry over contamination. By the time of the final monitoring three months later, a permanent collection crew was in place, which might be expected to have contributed to an improved service and a consequent rise in recycling rates in both groups.

Regression models

[Table 4 about here]

We ran tests which confirmed that the follow-up measures of recycling are normally distributed, meaning the data is suitable for regression analysis. The Kolmogorov-Smirnov test (Chakravarti, Laha, & Roy, 1967) result is $D = 0.050$, $p = 0.021$ and the Shapiro-Wilk test

(Shapiro & Wilk, 1965) result is W = 0.986, p = 0.001. We use random-effects multilevel models (Littell, Milliken, Stroup & Wolfinger, 1996) to take into account the two levels of our data: streets and super output areas. The models are implemented in Stata using the *xtreg* command, with Maximum Likelihood (ML) estimators.

Table 4 presents the estimates of four random-effects multilevel regression models on recycling participation rate by street. Estimates of the random part include residual errors at both level 2 (super output areas) and level 1 (streets), intraclass correlation (ICC), the squared multiple correlation ($_{R^2}$) and the fit-of-model statistic (deviance). ICC is the proportion of variance estimated at the different levels. We calculate a statistic analogous to the multiple R^2 expressed difference as a proportion for the total error variance (Hox, 2002: 64), best achieved separately for each level (Hox, 2002: 64).

We start with the multilevel regression analysis by running a baseline or intercept-only model (M0), which contains no explanatory variable, which serves as a benchmark with which other models are compared (Hox, 2002: 64). Model 1 (M1) examines the influence of the street level variables on post-canvassing recycling participation ratios. Within the variable Canvass Group, there is a no significant difference between the streets that were canvassed and those in the control group, with recycling ratios not being higher among the canvassed streets. This appears to reject our first hypothesis (H_1), that the canvass streets have a higher level of recycling than the control group after the recycling campaign. Model 1 also shows a very strong and significant ($p<0.001$) positive effect of the baseline recycling participation ratio (Baseline rate), confirming that there is a strong effect of habit on recycling behavior: across both groups in the sample, a large proportion of those who recycled at the start were still recycling at the end of the experiment. The variable Times is only significant at the ten per cent level, indicating that there were changes in recycling across the periods, but its effect was weak. The final variable in Model 1 is street size and we found no significant relationship

between street size and recycling ratios unlike previous studies (Harder, Woodard, & Bench, 2006).

Model 2 (M2) introduces an interaction terms between the baseline rate and the treatment in line with our hypothesis 3 that there should be a negative interaction between the treatment and the baseline rate. This has some important implications for our model, which now is more fully specified. Treatment is now positive and significant at ($p. < .05$) with a treatment effect of 8.3 per cent. Baseline retains its power in the model. The interaction term is negative as expected, though significant at the lower bar of ten per cent, confirming the hypothesis about the relationship between the baseline and the treatment: streets with lower baseline recycling participation ratios responded more to the canvassing than streets with a higher baseline.

Model 3 (M3) introduces two super output area level covariates, the multiple deprivation score and the percentage of the population that is non-white. The results of model 3 indicate that there is a significant ($p<0.001$) and negative relationship between the multiple deprivation score of an area and recycling ratios. In our sample, streets in super output areas with high levels of poverty participate less in curbside recycling. Households from lower socio-economic groups devote less effort to recycling because their economic and social deprivation mean they face more pressing needs (Martin, Williams and Clark 2006). When we control for deprivation, the streets in super output areas with a high ethnic minority population are more likely to participate in a curbside recycling scheme than those in nearby areas that have a smaller proportion of ethnic minorities. This questions previous studies that have found no difference in recycling rates between ethnic groups, though the results are only significant at the ten per cent level. The inclusion of these terms has no impact on the model estimates for the hypothesized terms of the treatment group, the effect over time and the

interaction. In models not reported in the table, we find no influence of other interactions of treatment with habit (times), deprivation or percentage of non-white.

(Figure 2 about here)

We explore the impact of the interaction term by plotting the predicted values of the interaction term with the outcome, the follow up rate. This is a two-way linear prediction plot with confidence intervals. The solid line is the fitted value of treatment group while the dash line is the fitted values of control group. This figure shows that when the base rate is low there is more of an effect on participation rate in recycling. However, as base rate increases the effect of treatment decreases.

DISCUSSION AND CONCLUSION

The research did not start out with the expectation that canvassing would lead to radical changes in behavior because the intervention is modest. There is also the risk that an intervention might be short-term. And so it proved to be. Immediately after the canvassing, recycling in the intervention group rose by 5.4 percentage points when compared to the control group, and the effect was significant, but the effect lessened over time to just under 1.7 per cent. There is a decline in participation three months after the intervention, which shows that canvassed streets do not tend to get the habit of recycling. Nonetheless, when analyzed in regression models, recycling rose more in the treatment group than in the control group. These effects are robust to a variety of controls, such as ethnicity, deprivation and baseline controls.

In terms of making an inference outside the study area, we should bear in mind that this is in an inner city area where the recycling service is comprehensive, and where there are terraced and semi-detached houses which are easy to canvass, so it may be the case that other areas either have more potential for recycling or less by this method - we cannot know. But such results are consistent with existing observational studies (WRAP, 2006b) and with the effects of door-to-door canvassing for other contexts. The randomized controlled trial is a robust test of the intervention. In the absence of confounds to the experiment – we were not aware of any - we can assume that the canvassing had a modest effect in the short term and tailed off over time.

The positive results appear to justify the street-based nature of the intervention, which may be justified because of the ease of randomizing streets and canvassing them. However, we present no comparison of efficiency with a campaign based on randomizing individual properties or street blocks or neighborhoods. Moreover, the street effects in our data were not there as expected because length of streets did not matter. Nonetheless our multilevel models showed that the areas within which the canvassing takes place did matter, and that it appears that recycling clusters in larger areas. Further examination of the street and area effect could be examined in further studies.

Finally the interaction of the treatment with the baseline rate has both academic and policy implications. For academics, the prior propensity to participate is an important condition of canvassing experiments. Policy-makers may wish to know that even though more deprived areas are less likely to recycle, low participators are more receptive to canvassing messages than high participators.

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Table 1. Neighborhood Statistics for Old Trafford and Gorse Hill

		Old Trafford	Gorse Hill
Ethnicity (2001 census)	White	53%	87%
	Non white	47%	13%
Multiple Deprivation Score (higher score indicates more deprived) (2007)		41.7	30.6
Housing type (ONS 2004)	Detached	4%	3%
	Semi-detached	42%	44%
	Terraced	38%	45%
	Flats and other	16%	8%
All data from UK Office of National Statistics http://www.neighbourhood.statistics.gov.uk accessed 17/09/08			

Table 2. Summary statistics of recycling participation rate by street

Participation rate by group						
		Obs	Mean	Std. Dev.	Min	Max
Control group						
	Baseline	97 streets	0.540	0.205	0	1
	1st follow-up	97 streets	0.529	0.195	0	1
	2nd follow-up	97 streets	0.572	0.212	0	1
Canvass group						
	Baseline	97 streets	0.477	0.207	0	1
	1st follow-up	97 streets	0.520	0.202	0	1
	2nd follow-up	97 streets	0.526	0.206	0	1

Participation rate over time			
	Baseline	1st follow-up	2nd follow-up
Control group	0.54	0.53	0.57
Canvass group	0.48	0.52	0.53
Total	0.51	0.52	0.55

Table 3. Pre-Post changes on recycling participation rate of streets by group – Post-Canvassing and Follow-up rate 3 month after intervention

	Post-Canvassing Mean (St. Err.)	Follow-up (3 month later) Mean (St. Err.)
Control (n=97)	-0.011 (0.015)	0.032 (0.013)
Treatment (n=97)	0.043 (0.018)	0.049 (0.015)
Difference	-0.054 (0.024)	-0.017 (0.019)
Paired sample t-test on pre to post	t(96)= -2.262, p<0.025*	t(96)= -0.857, p<0.392

Note: Difference=mean (control)-mean (treatment)

Table 4. Estimates of the recycling participation rate of streets

Model	Regression coefficients (standard errors) (β (SE))			
	M0: Intercept-only model	M1: Street level variables-only model	M2: M1 + interaction Canvass group*district	M3: Street level variables & area covariates model
Fixed part				
Intercept	0.534*** (0.022)	0.133*** (0.032)	0.101*** (0.037)	0.184** (0.059)
Canvass group		0.020 (0.014)	0.083* (0.038)	0.080* (0.038)
Baseline rate		0.700*** (0.036)	0.761*** (0.050)	0.750*** (0.049)
Groupbase			-0.122+ (0.069)	-0.117+ (0.069)
Times		0.025+ (0.014)	0.025+ (0.014)	0.025+ (0.014)
Street size		0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)
Multiple deprivation score				-0.282* (0.112)
Percentage of non-White population				0.085+ (0.052)
Random part				
Variance of area level residual errors (σ^2_{u0j})	0.005	0.001	0.001	0.000
Variance of street level residual errors (σ^2_{eij})	0.036	0.019	0.018	0.018
Intraclass correlation at area level	0.123	0.051	0.060	0.027
Explained variance at area level (R_2^2) (appr.)		0.804	0.773	0.900
Explained variance at street level (R_1^2) (appr.)		0.487	0.493	0.494
Deviance		268.10	3.256	6.245

+: p.< .1
*: p<0.05
**: p<0.01
***: p<0.001

Figure 1: CONSORT Participant Flow Diagram

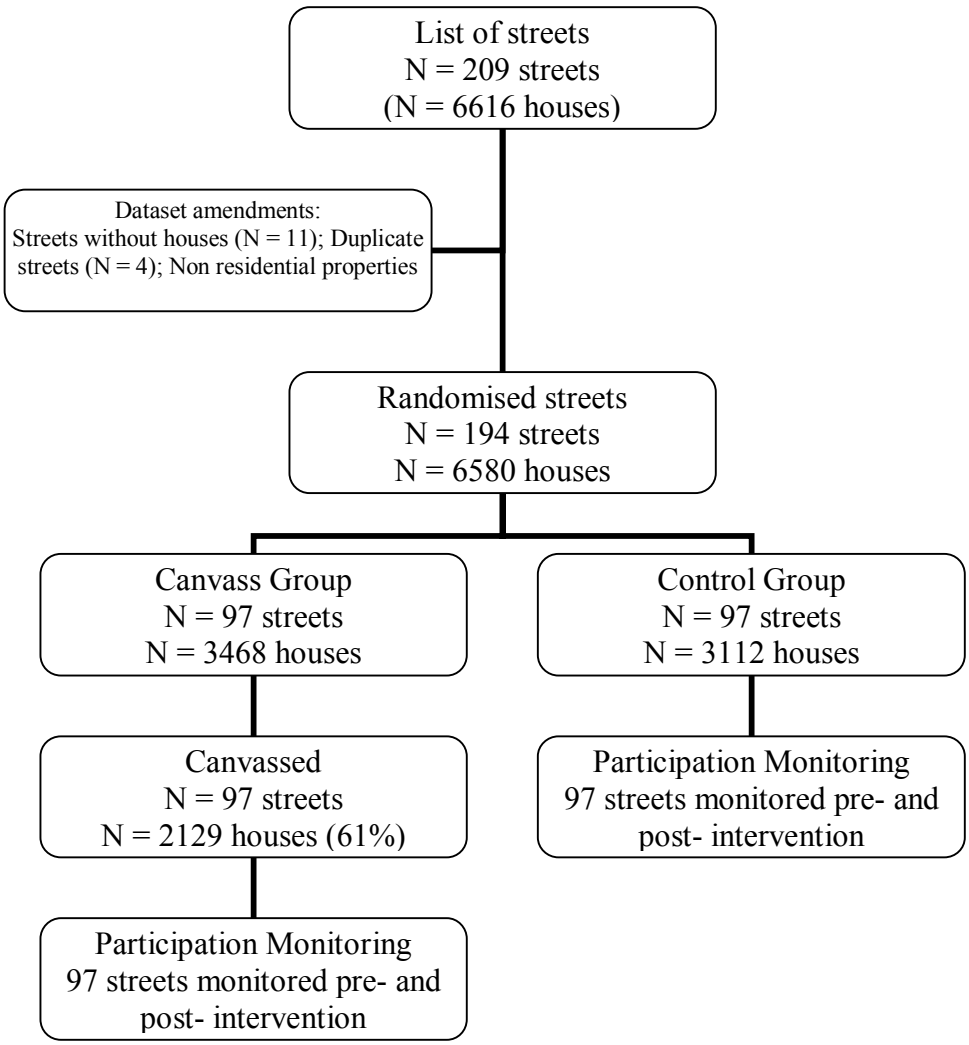
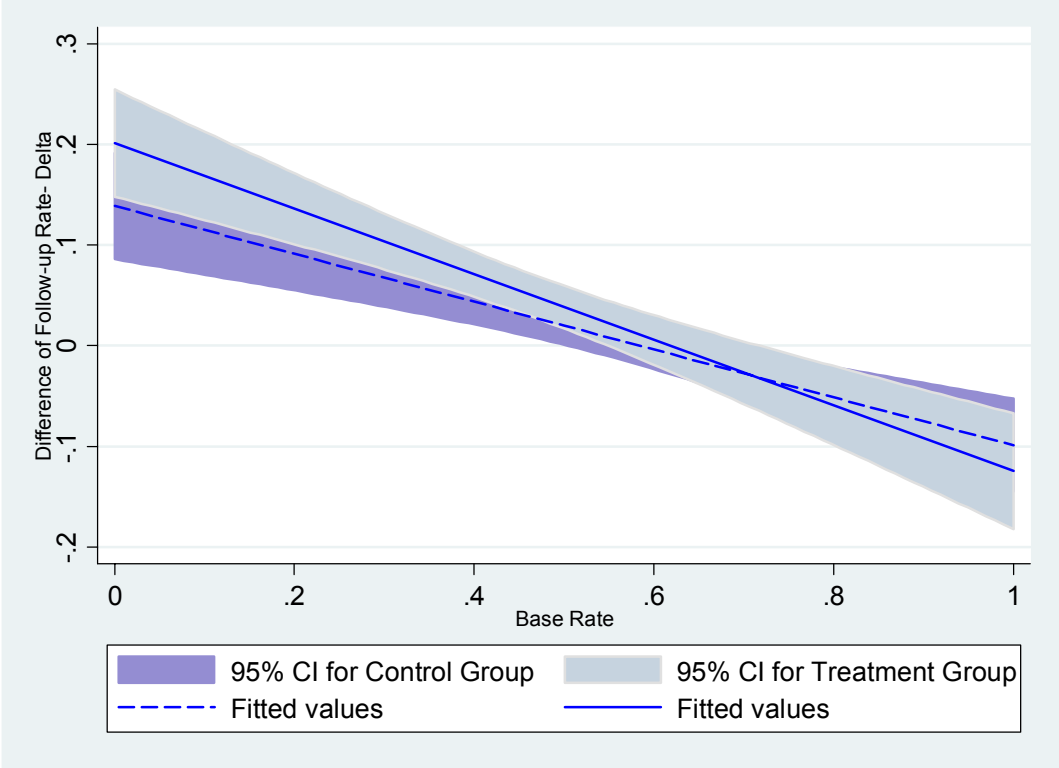


Figure 2 Marginal Effect of Group to Post-Intervention Participation Rate as Base Rate Changes



Appendix 1
Checklist of items to include when reporting a randomized trial (Moher, D., Schulz, K., & Altman, D., 2001).

PAPER SECTION And topic	Item #	Descriptor	Reported on page #
TITLE & ABSTRACT	1	How participants were allocated to interventions (e.g., random allocation", "randomized", or "randomly assigned").	1
INTRODUCTION			
Background	2	Scientific background and explanation of rationale.	4-9
METHODS			
Participants	3	Eligibility criteria for participants and the settings and locations where the data were collected.	12; 10-11
Interventions	4	Precise details of the interventions intended for each group and how and when they were actually administered.	13-14
Objectives	5	Specific objectives and hypotheses.	9-10
Outcomes	6	Clearly defined primary and secondary outcome measures and, when applicable, any methods used to enhance the quality of measurements (e.g., multiple observations, training of assessors).	15-16
Sample size	7	How sample size was determined and, when applicable, explanation of any interim analyses and stopping rules.	176
Randomization:			
Sequence generation	8	Method used to generate the random allocation sequence, including details of any restriction (e.g., blocking, stratification).	N/A
Allocation concealment	9	Method used to implement the random allocation sequence (e.g., numbered containers or central telephone), clarifying whether the sequence was concealed until interventions were assigned.	N/A
Implementation	10	Who generated the allocation sequence, who enrolled participants, and who assigned	16

		participants to their groups?	
Blinding (Masking)	11	Whether or not participants, those administering the interventions, and those assessing the outcomes were blinded to group assignment. If done, how the success of blinding was evaluated.	16-17
Statistical methods	12	Statistical methods used to compare groups for primary outcome(s); Methods for additional analyses, such as subgroup analyses and adjusted analyses.	19-20
RESULTS			
Participant flow	13	Flow of participants through each stage (a diagram is strongly recommended). Specifically, for each group report the numbers of participants randomly assigned, receiving intended treatment, completing the study protocol, and analyzed for the primary outcome. Describe protocol deviations from study as planned, together with reasons.	Figure 1
Recruitment	14	Dates defining the periods of recruitment and follow-up.	16
Baseline data	15	Baseline demographic and clinical characteristics of each group.	Tables 1 & 2
Numbers analyzed	16	Number of participants (denominator) in each group included in each analysis and whether the analysis was by intention-to-treat". State the results in absolute numbers when feasible (e.g., 10/20, not 50%).	18
Outcomes and Estimation	17	For each primary and secondary outcome, a summary of results for each group, and the estimated effect size and its precision (e.g., 95% confidence interval).	Table 4 (but we do not have confidence interval for the effect sizes)
Ancillary analyses	18	Address multiplicity by reporting any other analyses performed, including subgroup analyses and adjusted analyses, indicating those pre-specified and those exploratory.	20-22
Adverse events	19	All important adverse events or side effects in each intervention group.	N/A
DISCUSSION			
Interpretation	20	Interpretation of the results, taking into account study hypotheses, sources of potential bias or imprecision and the dangers associated with multiplicity of analyses and outcomes.	19-22
Generalizability	21	Generalizability (external validity) of the trial	22-23

		findings.	
Overall evidence	22	General interpretation of the results in the context of current evidence.	22-23

Appendix 2: table of summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Follow-up participation ratio	388	.5365528	.203837	0	1
Canvas Group	388	.5	.5006456	0	1
Baseline participation ratio	388	.5082427	.2073471	0	1
Times	388	1.5	.5006456	1	2
Street Size	388	33.91753	31.05373	2	190
Non-white population	388	.3542688	.1913425	.1031	.7019
IMD (deprivation) score	388	.3784407	.089929	.2655	.527