# Happy to Help:

# Welfare Effects of a Nationwide Volunteering Programme

Christian Krekel, Ganga Shreedhar, *London School of Economics*

Helen Lee, Claire Marshall, *National Health Service (NHS)*

Alison Boler, Allison Smith, *Royal Voluntary Service*

Paul Dolan, *London School of Economics*

## Abstract

We study the wellbeing benefits from volunteering in England's National Health Service (NHS) Volunteer Responders programme, which was set up in response to the Covid-19 pandemic. Using combined survey and administrative data, we exploit the oversubscription of volunteers to the programme and the random allocation of tasks via a smartphone app to estimate causal wellbeing returns. Volunteers show significantly stronger personal wellbeing and feelings of belongingness and social connectedness to their local area. Welfare analyses suggest that the benefits of the programme exceeded its costs by a multiple. We are the first to study the welfare effects of such a large-scale, nationwide volunteering programme. Our findings advance our understanding of the ways in which pro-social behaviour can improve personal wellbeing as well as social welfare.

**Key Words:** Volunteering, pro-social behaviour, wellbeing returns, quasi-experiment, welfare analysis

**JEL Codes:** I31, I38, D61, D64

Corresponding Author: Christian Krekel. E-Mail: c.krekel@lse.ac.uk. London School of Economics, Centre for Economic Performance (CEP) and Department of Psychological and Behavioural Science, Houghton Street, London WC2A 2AE, UK.

## 1. Introduction

Whether and how volunteering improves wellbeing has important implications for economics and policy.[1] In the UK, more than four in ten adults (38%) reported to volunteer at least once during the last twelve months in 2019, and two thirds of them at least monthly, with a median of eight hours (NCVO, 2019). This makes more than 1.6 billion hours of unpaid, voluntary work per year in the UK alone.[2] This large scale of volunteering in society is also reflected in GDP figures. In the UK, volunteering is valued at about 2.5% of annual GDP. In the US, it is even higher, estimated to be about 3.7% (OECD, 2015).

Traditionally, voluntary work enters national accounts via time use surveys, by multiplying the number of volunteering hours with the hourly wages in complementary, paid work, which then yields the economic value of volunteering. However, if volunteering causally affects volunteers' wellbeing, and if wellbeing returns are positive and sizeable, this traditional method of accounting may underestimate, potentially largely, the true value of volunteering to society, by neglecting an important component of its private returns.[3]

Yet, whether volunteering causally improves wellbeing is not *ex-ante* clear. Standard economic theory suggests that giving away time for free that could otherwise be used as inputs into labour or leisure leaves agents on a lower utility level, arguably reducing rather than raising wellbeing. On the other hand, theories of warm-glow giving (Andreoni, 1990) and growing evidence from lab experiments and field trials on pro-social spending (Dunn et al.,

---

[1] Volunteering is defined as helping others by voluntarily giving time without compulsion and expectation of direct monetary returns.

[2] 66,700,000 x 0.38 x 0.67 x 8 x 12 = 1,630,254,710.

[3] Private economic returns have also been studied. For example, Freeman (1997) finds that volunteering is associated with a raise in paid work hours by between 3% and 7%. There may also be wider social returns.

2008; Aknin et al., 2020) suggests that agents may be better off after giving donations, at least for a while (Falk and Graeber, 2020).[4]

We seek to understand whether and in what ways volunteering affects wellbeing, by taking advantage of data uniquely equipped to do so. In March 2020, England's National Health Service (NHS) and the then Secretary of State for Health issued a mass call for volunteers via the *NHS Volunteer Responders* (NHSVR) programme.[5] Its goal was to support clinically high-risk people self-isolating in their homes during the Covid-19 lockdown and to ease pressures on NHS staff. In this novel, digital micro-volunteering programme, a smartphone app ('GoodSAM') flexibly allocated low-commitment, small ('micro') tasks directly from those in need to those who wanted to help. It had two types of services. First, *Transport* and *Community Response*, which were based on volunteers' locations, were concerned with logistics; tasks involved running errands for local NHS sites, pharmacies, or private persons in need, for example transporting equipment, assisting with medication delivery, or collecting groceries and delivering these to people who were self-isolating. Second, *Check In And Chat*, which was independent of volunteers' locations, provided phone support to people who were at risk of loneliness as a consequence of self-isolation.

Three quarters of a million people registered their interest to volunteer in just four days (NHS, 2020), exceeding the target of 250,000 within just 24 hours and resulting in the largest volunteer mobilisation in England since World War II. The benefits to vulnerable

[4] In his book *Social Interest: A Challenge to Mankind*, Austrian psychologist Alfred Adler stresses as early as 1938 that people are fundamentally striving to work towards a goal larger than their self-interest, and that satisfying this need produces positive cognition, which may be directly related to warm-glow giving (Adler, 1938).

[5] The call was issued on March 24, 2020. The programme was open to those 18 and above and without current Covid-19 symptoms. Those self-isolating could also volunteer but were constrained to phone services.

communities were considerable: around 165,000 people were helped at home during the pandemic from April 2020 to April 2021, with more than 1.8 million tasks completed.[6]

To estimate causal wellbeing returns to volunteering, we exploit two unique features of the NHSVR programme: the oversubscription of volunteers to the programme and the random allocation of tasks via the app. We examine responses from 4,400 volunteers who were surveyed three months after they signed up to the programme, combined with detailed administrative data on the history, type, location, and timing of their volunteering.

We find that volunteering significantly increased volunteers' overall life satisfaction, their sense of purpose in life, and their feelings of belongingness and social connectedness to their local neighbourhood. Impacts are sizeable: the effect on life satisfaction, for example, (+0.21 on a zero-to-ten scale) is about 30% of the effect of being employed as opposed to being unemployed (+0.68, cf. Clark et al., 2018), 35% of the effect of being partnered as opposed to being single (+0.59, cf. ibid), or 20% of the effect of local-community interventions aimed at raising the wellbeing and pro-sociality of the general population (+1.04, cf. Krekel et al., 2021; +1.1, cf. Heintzelman et al., 2020). Our effects are in line with quasi-experimental studies on volunteering and life satisfaction (which range between +0.14 and +0.3, cf. Binder and Freytag, 2013; Borgonovi, 2008), and are in the middle of that range.

We find that impacts on wellbeing are increasing in the number of tasks, with diminishing returns, and with some outcomes pointing towards an inverse U-shape relationship. Volunteers generate stronger benefits in terms of overall life satisfaction and sense of purpose in life when volunteering in phone services that provide more social interaction with the benefits of their volunteering. On the contrary, volunteers generate stronger benefits in terms of feelings of belongingness and social connectedness to their local neighbourhood when locally

[6] Figures provided by the NHS and the Royal Voluntary Service.

helping local NHS sites, pharmacies, or private persons with logistics and errands.

We conduct welfare analyses, including a cost-benefit analysis which compares the monetised wellbeing benefits of the programme with its costs, a cost-effectiveness analysis which compares its benefit-cost ratio with that of other interventions (including psychological therapies by the NHS), and a more traditional analysis which is based on the number of volunteering hours and the minimum wage rate and which neglects private wellbeing returns. Irrespective of the type of analysis, we find that the benefits of the programme exceeded its costs by a multiple. Importantly, this is a lower-bound as it does not account for the benefits to the beneficiaries of volunteering, which are likely to be substantial.

We add to the literature in three ways. First, although there is a strong suggestion that volunteering can improve wellbeing (Borgonovi, 2008; Meier and Stutzer, 2008; Binder and Freytag, 2013; Jenkinson et al., 2013; Son and Wilson, 2015; Tabassum et al., 2016; Russel et al., 2018; Huang et al., 2019; Lawton et al., 2020), most evidence is correlational. The few studies that use longitudinal methods such as fixed effects (Russel et al., 2018; Lawton et al., 2020) or quasi-experimental methods such as matching (Binder and Freytag, 2013), instrumental variables (Borgonovi, 2008), or difference-in-differences (Meier and Stutzer, 2008) report higher life satisfaction of volunteers compared to non-volunteers. However, the only field trial that looked at wellbeing as the main outcome – a waitlist RCT amongst college students – did not find any effect of weekly volunteering in a community service-learning programme (Whillans et al., 2016). In contrast, Schreier et al. (2013) find that randomly assigning high-school students to weekly volunteering with younger students did reduce their cardiovascular risk, especially amongst those who showed increases in empathy and altruistic behavior, and who showed decreases in negative mood. We add to this literature, by estimating the causal effect of volunteering in a large-scale, nationwide volunteering programme and by studying its wider welfare implications for society. By looking at the wellbeing benefits of *giving time*, we complement existing evidence in the adjacent area of pro-social spending and its modalities (Dunn et al., 2008, 2014; Aknin et al., 2013a, 2013b; 2015, 2020; Falk and Graeber, 2020), in particular field trials on the wellbeing returns from *giving money* in form of donations (Dunn et al., 2008; Aknin et al., 2020; Falk and Graeber, 2020), to paint a more complete picture of the wellbeing benefits of pro-social behaviour in general.

Second, there is a debate about the ways in which volunteering may improve wellbeing, for example by generating a “warm glow” from helping others, by giving people a sense of purpose in life, or by connecting them to others in their local community (Andreoni, 1989; Meier and Stutzer, 2008; Son and Wilson, 2015). Volunteering can arguably affect different dimensions of wellbeing, yet few studies have looked at them jointly (Son and Wilson (2015) is an exception). We add to the literature, by not only looking at a range of outcomes but also by looking at which types of tasks bring about which wellbeing benefits. Moreover, beyond the extensive margin, our unique data and setting also allow us to tease out the intensive margin of volunteering and potential non-linearities in its relationship with wellbeing.

More generally, we add to the literature in economics on pro-social behaviour (e.g. Ariely et al., 2009; Feldman, 2010; Al-Ubaydli and Lee, 2011; Stutzer et al., 2011; Mujcic and Leibbrand, 2018; Cassar and Meier, 2021), and in particular, to the few studies looking at the returns to volunteering (e.g. Freeman, 1997; Hackl et al., 2007; Sauer, 2015; Baert and Vujic, 2018), especially in terms of wellbeing (e.g. Borgonovi, 2008; Meier and Stutzer, 2008; Binder and Freytag, 2013). We also add to growing literature that uses wellbeing data to monetarily value non-market goods and activities (e.g. van Praag and Baarsma, 2005; Luechinger, 2009a, 2009b; Levinson, 2012; Dolan et al., 2019).

## 2. Experimental Design

To join the NHSVR programme, volunteers had to register online, by submitting their date of birth, proof of identity, contact information, location, and preferences for tasks. After their identity had been verified, they had to download the app, which they had to switch to 'on duty' and which alerted them of nearby tasks. Our experimental design relies on the oversubscription of volunteers to the programme and the random allocation of tasks via the app.

***Oversubscription.*** Of the 750,000 volunteers who came forward in March 2020, 590,633 provided a proof of identity and were approved, 491,813 downloaded and logged onto the app, and 366,482 switched it 'on duty' at least once by mid July 2020 (i.e. when we collected our survey data). However, only 225,069 completed at least one task by that time, yielding an oversubscription by about 366,482 / 225,069 = 1.6. The 590,633 volunteers who were approved were recruited within a four-day window (i.e. March 25 to 28, 2020), after which the web site was closed due to too many registrations and not reopened until after our survey data collection. There should thus be little systematic differences between volunteers, as they all registered during the same closely spaced time window.

Exploiting precise geographical coordinates on the universe of volunteers and tasks from administrative records, Appendix Figure A1 plots the geographical distribution of the number of volunteers (i.e. the supply of voluntary work) during our observation period at the level of Lower Layer Super Output Areas (LSOAs), of which there are 32,844 in England. These areas encompass, on average, 1,600 residents and 670 households. Similarly, Figure A2 plots the geographical distribution of the number of tasks (i.e. the demand for voluntary work) per LSOA during our observation period. Figure A3 shows the over-supply, by plotting the number of tasks less the number of volunteers per LSOA. As seen, there is huge variation in demand and supply, with broad over-supply. The median number of volunteers per

LSOA is 14 and the median number of tasks nine, which yields an over-supply by about 14 / 9 = 1.6, in line with over-subscription from aggregate numbers.

***Randomisation.*** Amongst the pool of potential volunteers, the allocation of tasks is based on a randomisation algorithm inside the app and unrelated to individual characteristics. For both location-based (*Transport* and *Community Response*) and phone-based services (*Check In And Chat*), help is primarily requested via the app. Alternatively, a person in need can either call the NHS Support Centre to make a self-referral or a professional (e.g. a GP) can make a referral on their behalf. Help can include both one-off tasks or more regular support (recorded on a task-by-task basis).

For location-based services, a request is registered and assigned to a pool of volunteers who signed up for this type of task in a 25km radius to the origin. The app then picks the volunteer who is geographically closest (measured by the Euclidean distance) and sends this volunteer an alert. In case that two volunteers are at exactly the same distance, the app picks one at random. If the volunteer who has been sent the alert does not accept the task within fifteen minutes, or rejects it, the next geographically closest volunteer is picked and sent the alert, and so on. If no volunteer is found within a 25km radius, the app automatically increases the radius to 30km. When unanswered, a task will automatically time out after five days. For location-based services, the allocation of tasks to volunteers is, therefore, random conditional on local characteristics and demand for volunteers.

For phone-based services, the request is registered and assigned to a pool of *all* volunteers who signed up for this type of task, regardless of location. The app then picks one at random. If the volunteer who has been sent the alert does not accept the task within fifteen minutes, or rejects it, another volunteer is picked at random and sent the alert, and so on. For phone-based services, the allocation of tasks to volunteers is, therefore, completely random.

Note that, for location-based services, volunteers are informed at registration that the allocation of tasks is random and depending on local demand, whereas for phone-based services, they are informed that the allocation of tasks is random and depending on overall demand. Moreover, the way the app is designed should not lead volunteers to believe that whether they are allocated a task or not depends on their individual characteristics. These features reduce concern that volunteers interpret not having been allocated a task as a signal of personal deficiency. For both location-based and phone-based services, the app prioritises volunteers who have not completed any task yet since registration.[7]

A source of residual selection may be that volunteers, when being allocated a task, can decide whether to accept it or not. It may be that tasks which are perceived as unpleasurable are less likely to be accepted, which would inflate our average treatment effect as 'active' volunteers in our treatment group would be more likely to accumulate more pleasurable tasks relative to 'passive' volunteers in our control group. Although we cannot fully exclude this, we note that volunteers had to indicate their preferences for tasks at registration. A strong mismatch between preferences and tasks is thus unlikely.

## 3. Data & Empirical Strategy

### 3.1. Data

We use survey data on individuals who were interviewed three months after they signed up and were admitted to the NHSVR programme (during the initial recruitment window from March 26 to 28, 2020), combined with detailed administrative data on the history, type, location, and timing of their volunteering.

[7] Appendix Figure A6 illustrates the allocation of tasks to volunteers, Figures A7 to A9 show some of the functionalities of the app.

***Survey Data.*** Our first data source is an online survey which was embedded as a link in a newsletter sent out to the universe of individuals who have signed up and were admitted (due to valid proof of identity) to become an *NHS Volunteer Responder* (590,633 individuals). The newsletter was sent on July 6 and a separate reminder two weeks later, on July 20, 2020.[8] The response rate was, with 12,056 respondents, about 2%.

The survey asked several questions about wellbeing, which we use as outcomes. These are *life satisfaction* ("Overall, how satisfied are you with your life nowadays?") and *sense of purpose in life* ("Overall, to what extent do you feel that the things you do in your life are worthwhile?"), each on a scale from zero ("not at all") to ten ("completely"). These questions have been validated and are routinely asked by the Office for National Statistics (ONS) in the UK to measure personal wellbeing (cf. Dolan and Metcalfe, 2012). Due to survey length, we limited ourselves to these evaluative measures, as these (especially life satisfaction) are routinely used for monetarily valuing intangibles and are advocated for policy analysis by HM Treasury (2021).[9] The survey also asked about *feelings of belongingness* ("How strongly do you feel you belong to your immediate neighbourhood? Please think of the area within a few minutes walking distance from your home", on a four-point scale where zero means "not at all strongly" and three "very strongly") and *feelings of social connectedness* ("Do you feel more or less connected to your immediate neighbourhood and your neighbours since the Coronavirus (COVID-19) outbreak?", on a three-point scale where zero means "less" and two "more"). We dichotomise these items such that *feelings of*

[8] The Online Appendix includes links to these materials.

[9] The ONS also asks respondents whether they felt happy or anxious on the previous day. We postulate that more frequent experiences of happiness, e.g. due to warm-glow effects, would also be reflected in a higher overall life satisfaction, and *vice versa*.

*belongingness* takes on one for “very strongly” and “fairly strongly” (and zero otherwise), and *feelings of social connectedness* takes on one for “more” (and zero otherwise).[10]

Apart from these outcomes, our survey asked questions about individual characteristics, which we use as controls. These are age, gender, health, whether respondents are shielding or self-isolating, employment, religion, their previous volunteering experience, whether they were involved in other volunteering activities besides the NHSVR programme (and if yes, where), their motivations for joining, and the tasks they preferred to do when signing up. The survey also asked whether respondents had already completed a task (and if yes, the the number of tasks) or not (and if not, why not, including not yet been given a task, unable to accept a task due to constraints like time or distance, and issues with setting up the app).

***Administrative Data.*** Our second data source are administrative data from the app, which exist for the 366,482 individuals who downloaded the app and switched it ‘on duty’ at least once by mid July 2020 (i.e. when we collected our survey data). While sparse on individual characteristics, they contain detailed information on the history of each task that has ever been registered via the app. In particular, the data include the type of task (i.e. whether a task was in any of the location-based services *Transport* and *Community Response* or in the phone-based service *Check In And Chat*), the precise geographical coordinates of the origin of a task, and the timestamp when a task was allocated to a volunteer and, if any, the timestamps when a task was accepted (or rejected) and when it was completed.

***Samples and Group Allocation.*** We merge our survey with our administrative data based on volunteers’ e-mail addresses, which are unique person identifiers. As we have e-mail addresses for only a subset of volunteers in our survey data, we obtain a combined dataset of 4,898 volunteers (out of 12,056 survey respondents). Based on detailed information

[10] The results remain qualitatively the same when using the full rage without dichotomisation.

on each task from the administrative data, we then assign volunteers to our *treatment group* if they have been allocated, have accepted, and have completed at least one task, and to our *control group* if they have not been allocated a task yet, at the time when they completed their survey. The zero-category excludes volunteers who have rejected a task. Our estimation sample is, therefore, a cross-section of 4,358 volunteers in July 2020 who can be assigned to our treatment or control group according to this definition (180 cannot be assigned, e.g. because they have rejected a task) and who have no missings on either outcomes or controls (360 have missings). Of these, 3,214 (73.8%) are in the treatment and 1,144 (26.3%) are in the control group. This is our main estimation sample, as it reduces concern about bias from the self-reporting of volunteering and its extent.

Besides our main estimation sample, we use, whenever it adds value, a larger, extended estimation sample based solely on survey data. Using their self-reports, we can assign volunteers to our *treatment group* if they reported to have been given a task and to have volunteered, and to our *control group* if they reported to not have volunteered because they had not been given a task yet, at the time when they completed their survey. This yields a cross-section of 9,163 volunteers (out of 12,056 survey respondents) who can be assigned to our treatment or control group according to this definition (2,374 cannot be assigned, e.g. because they have rejected a task, because they had issues with the app, or because of other reasons) and who have no missings on either outcomes or controls (519 have missings). Of these, 6,375 (69.6%) are in the treatment and 2,788 (30.4%) are in the control group.[11, 12]

***Internal and External Validity.*** To check for internal validity, we compare treatment

[11] When correlating our treatment dummy generated from the administrative data with the self-reported dummy, we find a correlation of 0.90, which is significant at the 1% level.

[12] A detailed description of our data can be found in Online Appendix W4.

and control group within our sample. Table 1 shows means and standard deviations by group, simple differences in means, and normalised differences (which are scale-free, i.e. independent of group sizes, and thus more informative about the degree of covariate imbalance, if any, between larger groups, cf. Imbens and Rubin, 2015) for our main estimation sample. As seen, none of the normalised differences exceeds 0.25, which Imbens and Wooldridge (2009) suggest as a threshold above which covariates can be considered unbalanced. Covariates seem well balanced between groups within our sample, and treatment and control group, therefore, well comparable.[13]

A concern may be selection into the survey (or attrition from the universe of volunteers in the administrative records to those in the sample), and in particular the differential response rate by group (roughly two treated to one controlled). Such selection may be driving our results if the response rate was correlated with our outcomes, i.e. happier volunteers, presumably due to being treated, were more likely to respond and unhappier ones less likely, which may inflate or even entirely explain our identified difference between treated and controlled. We argue that this is less of a concern here, for several reasons. As shown below, the shares of treated and controlled in our samples (both main and extended) are quite similar to the 'true' shares of treated and controlled in the universe of volunteers. Moreover, our sample compares well with the universe when it comes to observable characteristics from the app, especially task behaviour, and observable characteristics in two external datasets, on average and by group. Our identified treatment effect is also similar in size to the wedge between volunteers and non-volunteers found in these datasets. Finally, in a formal robustness check in Section 5.4, we predict survey response from having been treated in the administrative

[13] We arrive at a similar conclusion when comparing treatment and control group within our larger, extended estimation sample based on survey data alone. The results are available upon request.

records, showing that treatment does not seem to be a strong predictor of response. We also calculate Lee (2009) bounds around our average treatment effects, showing that these exclude zero (with the exception of belongingness).

We now look at external validity, by looking at response rates and their balancedness between treatment and control group in our sample relative to the universe of volunteers in the administrative data. Our main estimation sample includes 73.7% treated and 26.3% controlled. Recall that 366,482 individuals downloaded the app and switched it 'on duty' (and hence chose to be available for task allocation and, thereby, treatment or control group assignment) and 225,069 completed at least one task. This yields a ratio of (225,069 / 366,482) x 100 = 61.4% treated to 38.6% controlled, which is quite similar.[14] As expected, our sample includes a larger share of the treated, most likely because they had more interaction with the programme and were, therefore, more receptive to the newsletter and its embedded survey.

Next, we compare our sample with the universe of volunteers in the administrative data based on observable characteristics from the app. Table 2 shows means and standard deviations by sample, simple differences in means, and scale-free normalised differences. In line with our findings on response rates, our main estimation sample includes a larger share of the treated, in total and across services, though none of the normalised differences exceeds the suggested threshold of 0.25 except *Check In And Chat*. Going on, individuals in our sample have, on average, been allocated more tasks, both in total and across services, though again none of the normalised differences exceeds 0.25. Importantly, volunteers in our sample and those in the universe show a similar task behaviour when it comes to the number of rejected tasks (3.5 vs. 2.4) and the number of timed out tasks (4.3 vs. 6.2). They also have a

[14] In our extended sample, we have a ratio of 69.6% treated to 30.4% controlled, which is very similar to our main estimation sample and, likewise, similar to the universe of volunteers in the administrative data.

similar waiting time for the first task being allocated (31 vs. 28 days). Volunteers in our sample and those in the universe, therefore, seem to behave in similar ways. Exploiting their precise geographical coordinates from administrative records, Appendix Figure A4 plots the geographical distribution of tasks in our estimation sample, Figure A5 that of a 1% random sample of tasks in the universe: the distributions are similar. Taken together, our survey attracted volunteers who had more interaction with the programme. They are also slightly older. Note, however, that the treated and controlled seem well balanced in terms of age (and other observables) *within* our sample (cf. Table 1).

Finally, we compare our sample with two external datasets: the nationally representative Understanding Society Covid-19 Wave (USC19) and the UCL Covid-19 Social Study (UCL19). We restrict observation periods to England in July 2020, construct covariates that match ours (if possible), and calculate scale-free normalised differences. We compare samples on average and by group, i.e. our treatment group with volunteers and our control group with non-volunteers. Appendix Tables A5 and A6 show that there are few differences, except that individuals in our sample seem less often full-time employed and slightly younger and healthier. This is not surprising: those who are older and less healthy are more vulnerable to Covid-19, and hence less likely to volunteer. Besides that, our sample seems very similar to the general population at the time, on average and by group.[15]

### 3.2. Estimation and Identification

We estimate the following regression equation:

[15] We arrive at similar results when using our extended sample (available upon request).

$$y_i = \alpha + \delta Treatment_i + \beta_1{}'X_i + \beta_2{}'C + p + d + \varepsilon_i \qquad (1)$$

where $y_i$ is the wellbeing of individual $i$; $Treatment_i$ is a dummy that is one if the individual has been assigned to our treatment group, and zero if assigned to our control group; $X_i$ and $C$ are individual and regional controls; and $p$ and $d$ are postcode and interview date fixed effects.[16] We estimate models using OLS, which in case of *feelings of belongingness* and *social connectedness* yields linear probability models. As *life satisfaction* and *sense of purpose in life* are discrete and ordinal, linear models may yield measurement error. However, it has been shown to be minor in most applications (cf. Ferrer-i-Carbonell & Frijters, 2004). When re-estimating our models using ordered logit models, our results remain qualitatively the same. This is shown in Online Appendix Section W2.1, where we also address the finding by Bond and Lang (2019) on the reversibility of results from ordered models when using wellbeing data if the models are heteroskedastic, by using heteroskedastic ordered logit models.

$X_i$ are individual controls, including age, gender, ethnicity, religion, whether the individual has a long-term physical or mental health condition, whether the individual is shielding or self-isolating, employment status, whether the individual has volunteered before, and whether the individual is currently volunteering elsewhere, and if so, where. We also control for self-reported motivations for joining the NHSVR programme[17], which service they joined (*Transport*, *Community Response*, or *Check In And Chat*), and for a respondent's fidelity in

[16] See Section 3.1 for the precise treatment and control group definitions in our estimation samples.

[17] Online Appendix Section W3 includes the precise definitions of these motivations (including purely altruistic, impurely altruistic, time use, and skills or career-related motivations, amongst others).

completing the survey (measured in terms of time taken to complete the survey).[18]

*C* are regional Covid-19 controls, including the daily new and cumulative number of people with at least one lab-confirmed positive Covid-19 test result; the daily new and cumulative number of Covid-19 patients admitted to hospital; the daily new and cumulative number of deaths of people who had a positive test result for Covid-19 and died within 28 days of the first positive test; the daily number of confirmed Covid-19 patients in hospital at midnight the preceding night; and the daily number of confirmed Covid-19 patients in mechanical ventilation beds. These originate from NHS and UK Government administrative data (UK Government, 2020a) and are recorded at the level of NHS regions (i.e. East of England, London, Midlands, North East and Yorkshire, North West, South East, and South West).

Our empirical strategy rests on the comparison of volunteers who signed up, were admitted, and got to volunteer at any point in time until taking our survey (treatment group) with those who signed up, were admitted, but did not get to volunteer because they had not been given a task (control group). Our comparison is, therefore, *between* individuals who selected into the programme. This eliminates selection into volunteering based on observable or unobservable characteristics. Oversubscription of volunteers to the programme and the random allocation of tasks via the app further ensure that being allocated a task is independent of such individual characteristics.

***Identifying Assumptions for Location-Based Services.*** For the location-based services *Transport* and *Community Response*, the allocation of tasks to volunteers is random conditional on local characteristics and demand for volunteers. To the extent that local

[18] These controls are time-invariant and, in most cases, pre-treatment. Excluding shielding and self-isolating, employment status, or volunteering elsewhere including where (which may have changed between joining the programme and data collection) leaves our results unchanged.

characteristics and demand are independent of wellbeing, or that controlling for them renders them conditionally independent, $\delta$ can be interpreted as the (sample) average treatment effect on the treated (ATT), and hence as causal. That is, $Treatment_i \perp \{0, 1\} \mid C, p$. In addition, we require that joining the programme does not by itself constitute a positive treatment for our control group, for example by inducing warm-glow effects (Andreoni, 1989, 1990) or bestowing social recognition, thereby deflating $\delta$. Likewise, waiting for a task does not constitute a negative treatment, by inducing disappointment effects, thereby inflating $\delta$. That is, for $\delta$ to reflect the true effect of volunteering, outcomes in our control group must remain unaffected and on the same underlying trend in wellbeing as our treatment group.

***Identifying Assumptions for Phone-Based Service.*** For the phone-based service *Check In And Chat*, the allocation of tasks to volunteers is independent of location and hence completely random: $Treatment_i \perp \{0, 1\}$. Thus, $\delta$ can be directly interpreted as the (sample) ATT. We still require that being in the programme does not by itself constitute a positive or negative treatment for our control group, thereby deflating or inflating $\delta$.

Regarding conditional independence, we routinely include regional Covid-19 controls $C$ (netting out potential differences in local demand for volunteers) and postcode fixed effects $p$ (netting out local characteristics and unobserved heterogeneity at a precise geographical level) throughout our regressions to ensure exogeneity.[19] Beyond that, we routinely include individual controls $X_i$ (netting out potential differences in individual characteristics between treatment and control group). Omitting these leaves our results unchanged. Table 1 shows that covariates are well balanced between treatment and control group.

Regarding a potential treatment of our control group, we are concerned about a

[19] We include 124 postcode area fixed effects. In Section 5.1, we explore alternatives, including postcode area and district as well as local-authority district (LAD) fixed effects. These leave our results unchanged.

negative treatment (as opposed to a positive treatment, which would yield a lower-bound), in particular disappointment. Note that our treatment group could also be subject to disappointment from not being called upon more recently or frequently, which would, however, yield a lower-bound. We argue that disappointment effects are rather unlikely, for several reasons.

First, recall that volunteers are made aware of the random assignment of tasks during the registration process, and that the way the app is set up should not lead them to believe that whether they receive a task or not depends on 'how good they are' but only on local demand for volunteers. Such expectation management should minimise disappointment. One might also argue that it is rather unlikely that people experience a significant decrease in their overall quality of life by not being called upon (arguably, it may even work the other way around, because the crisis may become more salient when being called upon).

Second, in a robustness check in Section 5.1, we exploit the timestamps of the first and last tasks completed by each volunteer from the administrative data, and calculate the time elapsed between the last task completed and the interview date to control for disappointment effects (which we assume to be linearly increasing in waiting time). As we will see, controlling for the time elapsed since the last task leaves our results unchanged. This also reduces concern about disappointment effects in our treatment group.

Finally, in Appendix Table A6, we compare our ATT with the nationally representative Understanding Society Covid-19 (USC19) Wave and the UCL Covid-19 Social Study (UCL19), each restricted to England in July 2020, which include volunteers who should not suffer from disappointment effects. We find that volunteers in each dataset score higher in life satisfaction than non-volunteers, by about 0.2 points on a zero-to-ten scale, which is almost exactly the same as our ATT of about 0.21 points (cf. Table 3).[20] Compared to the quasi-experimental literature, our ATT is in line with the range of effect sizes of volunteering on life satisfaction, as discussed in more detail in Section 6.

## 4. Results

### 4.1. Average Treatment Effects

Table 3 compares the wellbeing of 'active' volunteers who have been allocated, have accepted, and have completed at least one task (our treatment group, for whom $Treatment_i$ takes on one) to 'passive' volunteers who have not been allocated a task yet (our control group, for whom $Treatment_i$ takes on zero), at the time when they completed their survey.[21]

[Table 3 about here]

We find that volunteering has strong, positive effects on wellbeing, raising overall life satisfaction and sense of purpose in life by about 0.21 and 0.23 points on a zero-to-ten scale (0.10σ and 0.12σ), respectively, in our preferred model with individual controls. Likewise, it raises feelings of belongingness and social connectedness to volunteers' immediate

[20] Our ATT is about 0.21 points on a zero-to-ten scale. In the USC19 sample, volunteers score, on average, 0.23 points higher in life satisfaction than non-volunteers (8.1 vs. 7.87). In the UCL19 sample, this amounts to 0.22 points (6.23 vs. 6.01). Interestingly, there is an average difference of about one point in life satisfaction between the USC19 and the UCL19 samples. There may be various reasons this, for example interview mode.

[21] Recall that the zero-category excludes volunteers who have rejected a task.

neighbourhood and their local community by about three and six percentage points, respectively.[22] Impacts are similar regardless of whether we control for individual characteristics or not, which reinforces our identifying assumptions.[23]

Online Appendix Table W1.1 estimates Table 3 using our extended estimation sample consisting of 9,163 volunteers (6,375 treated and 2,788 controlled): our results are similar, though effect sizes slightly weaker.

### 4.2. Treatment Effect Intensity

Table 4 looks at volunteers separately by their position in the overall task frequency distribution. We obtain the exact number of tasks that have been allocated, have been accepted, and have been completed by each volunteer by the time when they completed their survey from administrative records. On average, volunteers completed 15 tasks (SD of 27), with a median of seven. We allocate volunteers into different segments of the distribution. The dummy *25% to 50%*, for example, takes on one for those volunteers in our treatment group who have completed between 25% and 50% of tasks in the overall task frequency distribution, and zero for volunteers in our control group. On average, volunteers with less than 10% of tasks completed one task; volunteers in the 10% to 25% range two tasks; in the 25% to 50% range three to seven tasks; in the 50% to 75% range eight to 16 tasks; in the 75% to 90% range 17 to 35 tasks; and volunteers with more than 90% completed 36 and more tasks.

---

[22] We also find that 'active' volunteers are about eight percentage points more likely to expect that other people are doing more to help others since the Covid-19 outbreak. The results are available upon request.

[23] When restricting our treatment dummy to volunteers who completed their last task in one of the three months preceding their survey interview, we find significant, positive, and quantitatively similarly large effects for each of the preceding periods. The results are available upon request.

[Table 4 about here]

For overall life satisfaction and sense of purpose in life, wellbeing returns are generated from eight or more tasks onwards. For feelings of social connectedness to the local community, however, wellbeing returns are generated already at lower levels, from three or more tasks onwards. Impacts on feelings of belongingness to the immediate neighbourhood, which have been weaker, turn insignificant in this specification. Note that belongingness already has a high baseline level: about 70% of volunteers feel that they belong to their neighbourhood.

Except for sense of purpose in life, our results suggest that wellbeing returns to volunteering are diminishing, and in case of volunteers' overall life satisfaction and feelings of social connectedness to their local community even show an inverse U-shape. A possible reason could be overexposure to negative experiences of Covid-19, or a growing (time) commitment that could become emotionally straining, whereby highly active volunteers fail to draw the boundary between their own wellbeing and that of others (Heldman and Israel-Trummel, 2012; Jones and Williamson, 2014). However, for both outcomes, we cannot statistically reject diminishing returns (as opposed to an inverse U-shape).[24]

Assuming an inverse U-shape exists, we can calculate the life-satisfaction maximising amount of volunteering. In particular, we find that the strongest effect is generated for 17 to 35 tasks during our observation period (i.e. about 12 weeks). Taking the midpoint (i.e. 26 tasks) and assuming that tasks are equally spaced, this yields 26 / 12 = 2.2 tasks per week. Assuming each task lasts about one hour, this yields 2.2 hours of volunteering per week.

[24] F-tests for equality of coefficients between *75% to 90%* and *>90%* yield F(1, 4112) = 0.03 and F(1, 4112) = 0.04 for life satisfaction and for feelings of social connectedness, respectively.

### 4.3. Heterogeneous Treatment Effects by Type of Task

When signing up, volunteers could state their preferences for (multiple) services, and thereby for the types of tasks they wanted to do. *Transport* involves transporting equipment, supplies, or medication between NHS services and sites, including assisting pharmacies with medication delivery. *Community Response* involves collecting shopping, medication, or other essential supplies for individuals who are self-isolating, to deliver these supplies to their homes. Besides these location-based services, the phone-based service *Check In And Chat* provides phone support to individuals who are at risk of loneliness as a consequence of self-isolation. Selection into services and tasks is, therefore, not random.

To study heterogeneous treatment effects by type of task, we thus re-estimate our average treatment effects in Table 3 separately for each service.[25] In particular, we compare volunteers in our treatment group who signed up to a particular service and volunteered at any point in time within that service to those who signed up to *the same service* but did not get to volunteer. We take whether a volunteer has been allocated, has accepted, and has completed at least one task in a particular service by the time when they completed their survey from administrative records (as opposed to self-report, which is also available). Note that, because volunteers could choose several services to join, the resulting sub-samples are not independent of each other. To avoid small control group sizes, we merge the two location-based services *Transport* and *Community Response* into one service category which shares the same identifying assumptions. Table 5 shows our findings.

[25] In addition to type of task, we also estimated heterogeneous treatment effects by self-reported motivation for joining the programme. We did not find strong evidence that volunteers who reported different motivations for joining show systematically different returns to wellbeing, perhaps because motivations of joining were quite uniformly distributed. Online Appendix Table W3 shows this analysis.

[Table 5 about here]

We find a clear pattern: effects on overall life satisfaction and sense of purpose in life are strongest in *Check In And Chat*, arguably the service that allows for most social interaction between volunteers and their beneficiaries. On the contrary, effects on feelings of belongingness to the immediate neighbourhood and social connectedness to the local community are, though positive, smaller and insignificant. This makes sense, as the phone-based service *Check In And Chat* is nationwide and independent of the location of volunteers. In contrast, effects on overall life satisfaction and sense of purpose in life are smaller (though still significant) in the location-based services *Transport* and *Community Response*. Here, feelings of social connectedness to the local community show the strongest effects.[26]

While returns in terms of overall life satisfaction and sense of purpose in life may be increasing in the degree of social interaction, the observed pattern can also be explained in terms of differential costs of participation: arguably, volunteering in *Check In And Chat* is, from a volunteer's perspective, less costly than volunteering in *Transport* or *Community Response*, which is logistically more burdensome and may entail greater personal risks.[27]

[26] Online Appendix Table W1.2 re-estimates Table 4 using our extended estimation sample, which, due to its larger sample size, allows us to look at the two location-based services *Transport* and *Community Response* separately. A caveat is that services are obtained from self-report. We find a similar pattern where personal wellbeing is increasing in social interaction, i.e. from *Transport* to *Community Response* to *Check In And Chat*.

[27] We also looked at heterogeneous treatment effects by age, gender, and whether a respondent has volunteered prior to joining the programme, which refer to fixed or pre-treatment characteristics and are thus exogenous. However, we could not detect any convincing heterogeneities.

### 4.4. Heterogeneous Treatment Effects by Type of Task and Its Intensity

As a final exercise, we bring Tables 5 and 4 together, by estimating heterogeneous treatment effects by type of task and its intensity. As before, we obtain the exact number of tasks that have been allocated, have been accepted, and have been completed by each volunteer by the time when they completed their survey from administrative records, yet this time separately *in each service*. The dummy *25% to 50%*, for example, takes on one for those volunteers in our treatment group who have completed between 25% and 50% of the tasks in the overall task frequency distribution, and zero for volunteers in our control group, *in a particular service*. Note that the frequency distribution is service-specific. Table 6 shows our findings.

[Table 6 about here]

We find very similar patterns as before: effects on overall life satisfaction and sense of purpose in life are strongest in the phone-based service *Check In And Chat*, and these are generated already from three or more tasks onwards. On the contrary, effects on feelings of belongingness and social connectedness to the immediate neighbourhood and local community are smaller, and are generated only much later, from 17 or more tasks onwards. There is again evidence for diminishing returns, and even an inverse U-shape for overall life satisfaction and feelings of social connectedness in our preferred model with individual controls.[28] In contrast, effects on feelings of belongingness and social connectedness are generated much earlier in the location-based services *Transport* and *Community Response*, from two or more tasks

[28] Again, we cannot reject the null hypothesis of coefficient equality between *75% to 90%* and *>90%* for neither life satisfaction nor feelings of social connectedness: F(1, 2524) = 0.03 and F(1, 2524) = 0.01.

onwards, with diminishing returns or even an inverse U-shape.[29] However, effects on overall life satisfaction and sense of purpose in life are generated only later, from 21 or more tasks onwards.

## 5. Robustness

### 5.1. Extended Controls

We exploit the timestamps of tasks from administrative records and variation in survey dates, and calculate the time elapsed between the last task completed and the interview to control for waiting time. If present, disappointment effects for either the treatment group (from not being called upon more recently or frequently) or the control group (from not being called upon ever) should be captured by waiting time. Appendix Table A1 re-estimates our baseline specification while additionally controlling for waiting time. As seen, our results remain robust, suggesting that disappointment effects may play only a minor role.

Next, we test the sensitivity of our results to the choice of geographical fixed effects. In our baseline specification, we are controlling for 124 postcode area fixed effects to capture differences local characteristics. As an alternative, in Appendix Table A2, we replace these with 1,630 postcode area and district fixed effects (Panel A) and 309 local-authority district (LAD) fixed effects (Panel B), to net out local characteristics and unobserved heterogeneity at an even more precise geographical level. As seen, our results remain robust.

Finally, we test how well our geographical fixed effects capture local characteristics. We obtained data on a range of local characteristics from the Office for National Statistics (ONS)'s Indices of Multiple Deprivation (IMD) at the local-authority district (LAD) level for

[29] $F(1, 2644) = 0.00$.

the pre-treatment years 2015 and 2019. In Appendix Table A3 Column 1, we regress the number of tasks per LAD on these time-varying local characteristics in a kitchen-sink regression (pooled OLS, with robust standard errors). As expected, many of them are significantly associated with the number of tasks per LAD. Then, in Column 2, we estimate the same regression while additionally controlling for local-authority district (LAD) fixed effects. As seen, all of the local-area characteristics are (strongly) moderated downwards and only few still significant. Finally, Columns 3 to 6 show our baseline results when controlling for LAD fixed effects. As seen, our results remain robust, suggesting that our geographical fixed effects should net out a wide range of local characteristics that explain differences in local demand for volunteers.

### 5.2. Alternative Estimators

We estimated simple ordered logit models as an alternative to our linear models as well as heteroskedastic ordered logit models to address the finding by Bond and Lang (2019) on the reversibility of results from ordered models if these are heteroskedastic. Our results are found in Online Appendix W2. Both show significant, positive effects of volunteering on life satisfaction and sense of purpose in life.

### 5.3. Multiple Hypotheses Testing

To account for multiple hypotheses testing, we applied the stepdown multiple testing procedure by Romano and Wolf (2005a, 2005b) to our estimates, with the four-step algorithm outlined by Romano and Wolf (2016). Our stepdown-adjusted P values shown in Tables 2 to 5 continue to indicate significance at conventional levels for most of our estimates.

### 5.4. Selection Into Survey (Attrition)

In addition to comparing the shares of treated and controlled in our sample to the 'true' shares in the universe of volunteers as well as comparing our sample to the universe and external datasets when it comes to observable characteristics, we conduct a formal robustness check on selection into the survey (or attrition from the universe of volunteers to those in the sample), by predicting survey response from having been treated in the administrative records. In particular, we regress a dummy that is one if a volunteer has responded to our survey on a dummy that is one if a volunteer has been treated, as shown in the administrative records, using the same definition of treatment and control as in our baseline specification. We run this regression for our main estimation sample.[30] Table 7 presents our findings.

[Table 7 about here]

We find that having been treated increases response by, on average, 2.5 percentage points, without controls. Controlling for tasks and task behaviour as well as postcode fixed effects (as in our baseline specification) reduces the effect size further, to well below one percentage point. In other words, an individual who has actively volunteered is less than one percentage point more likely to have responded to our survey than a volunteer who has not been allocated a task yet, at the time of our survey. The small R Squared suggests that the predictive power of each model is low, and the large N of 331,521 is likely to yield a high statistical

[30] Unfortunately, we cannot run this regression for our extended estimation sample because, by definition, it does not include the administrative variables to allocate volunteers into treatment and control. When comparing our main to our extended estimation sample in terms of observable characteristics in Appendix Table A7, we find that simple differences are small and that none of the normalised differences exceeds the suggested threshold of 0.25. The similarity between both samples could be suggestive that treatment may be a similar predictor of response in each.

significance even for small differences between treated and control.[31] Hence, treatment does not seem to be a strong predictor of response.[32]

Finally, we calculate Lee (2009) bounds around our average treatment effects. Besides exogeneity of treatment, which is satisfied if our identifying assumptions hold, Lee (2009) bounds additionally assume that treatment affects attrition in only one direction (monotonicity). This implies that individuals in our control group who responded would have also responded if they had been treated, and that there are some additional individuals (in our treatment group) who only responded because they were treated. In our case, this is justified if we lose 'passive' volunteers primarily due to lack of engagement with the programme, which seems reasonable. Lee (2009) bounds then trim differential attritors by assuming that they all come from the very top or the very bottom of the outcome distribution, while bounds can additionally be tightened by baseline (pre-treatment) characteristics. We tighten by simple age categories and use bootstrapped standard errors with 100 repetitions. As shown in Tables 3 and 5, we obtain bounds around our average as well as heterogeneous treatment effects by type of task that exclude zero, with the exception of belongingness, which was only marginally significant at the 10% level. The lower bound for life satisfaction is higher than the calculated break-even effect necessary to make the programme worthwhile from a social welfare perspective, as shown below.

[31] Note that our sample size is slightly lower than 366,482 (the sample of volunteers who downloaded the app and switched it 'on duty' at least once) as we do not have administrative controls for all observations.

[32] We also exploited our estimated unconditional and conditional probabilities from Table 7 Columns 1 and 2, respectively, to construct inverse-probability weights and weigh observations in our main specification. Appendix Table A8 shows that our results remain qualitatively the same.

## 6. Welfare Implications

The costs of running the NHSVR programme during the period from April to July 2020 were about GBP 3.1 million.[33] Was it worth it?

To answer this question, we conduct three types of welfare analysis, each of which treats benefits in a different way: first, we conduct a wellbeing cost-benefit analysis (CBA) in which we compare monetised wellbeing benefits with costs. Second, we conduct a wellbeing cost-effectiveness analysis (CEA) in which we divide wellbeing benefits by costs and then compare the resulting benefit-cost ratio with alternative interventions. A wellbeing CEA does not convert wellbeing benefits into money, and hence does not rely on an unbiased estimate of the marginal utility of income. Third, we conduct a more traditional analysis based on the number of volunteering hours and the wage rate. Note that all three analyses are lower bounds: they neither take into account the wellbeing benefits to the beneficiaries of volunteering, which are likely to be substantial, nor knock-on effects, for example intra-household wellbeing spillovers from volunteers to those living with them.[34, 35]

---

[33] These are direct and indirect administrative costs of running the programme and do not include personal costs to volunteers such as time, effort, or direct expenses (e.g. phone bills).

[34] By July 2020, 92,120 beneficiaries have been helped. Based on a survey of 548, the Royal Voluntary Service finds that beneficiaries score 0.3 points higher in life satisfaction than a comparable sample of the UK population with underlying health conditions (Royal Voluntary Service, 2020). Applying the same approach as for volunteers below, whilst caveating a lack of causality, this would yield a total monetised wellbeing benefit for beneficiaries of (74 x 0.3) / 0.0196) x 92,120 = GBP 104.3 million.

[35] There is also sickness avoided, though this is more difficult to quantify: on the one hand, volunteers helped the vulnerable to not expose themselves to risk and fall sick. On the other, volunteers exposed themselves to risk. The programme did not keep track of how many volunteers fell sick, but anecdotal evidence suggests that this was a quantitatively minor issue.

Turning to our wellbeing CBA first, we estimated that volunteering increased life satisfaction by 0.21 points, on average. For the effect of log income on life satisfaction, we use a very conservative estimate of 1.96 points, which comes from official HM Treasury (2021) guidelines which use this coefficient as an upper bound to monetarily value wellbeing.[36] Median annual gross household income in England in 2019 was GBP 29,600 (ONS, 2020), or GBP 7,400 during the period from April to July 2020. Calculating the marginal rate of substitution between volunteering and income, we find that volunteers would have to be compensated with, on average, GBP (74 x 0.21) / 0.0196 = 790 to reach the same wellbeing level in the counterfactual case in which they had not volunteered. With 225,069 volunteers by July 2020, this yields total monetised wellbeing benefits of GBP 178 million. After subtracting costs, this yields net benefits of GBP 174.9 million. Note that the break-even effect of volunteering on life satisfaction to make the programme worthwhile would be 0.0036 (less than 1% of a SD), which is is a very small effect size compared to the literature (cf. Dolan and Peasgood, 2008; Clark et al., 2018; Frijters et al., 2020).[37]

Turning to our wellbeing CEA next, we arrive at a benefit-cost ratio of (0.21 x 225,069) / 3,100,000 = 0.0152, which can serve as a benchmark for future interventions in the area of volunteering. “Exploring What Matters”, a local-community intervention that has been conducted throughout England and that aims at raising wellbeing and pro-sociality in

[36] This is a large estimate, which is conservative as it leads to a lower willingness-to-pay. Lindqvist et al. (2020) report a causal estimate of 0.35, exploiting exogenous lottery wins, with auto-enrolment into lotteries. Using this estimate, we would arrive at total monetised wellbeing benefits of GBP 810 million. This shows the sensitivity of this type of analysis to the choice of the income coefficient, which varies in the literature. Sachs et al. (2010) report an estimate of 0.7, Kahneman and Deaton (2010) of 0.64, Stevenson and Wolfers (2008) of 0.3, and Clark et al. (2018) and De Neve et al. (2018) of 0.2.

[37] (3,100,000 / 225,069) / (74 / 0.0196) = 0.0036. The SD of life satisfaction in our sample is about 2.

the general population, has been found to increase life satisfaction by 1.04 points at a cost of GBP 90 per participant (Krekel et al., 2021). This yields a benefit-cost ratio of 1.04 / 90 = 0.0116, very similar to the NHSVR programme. The *Improving Access to Psychological Therapies (IAPT)* programme of NHS England, aimed at treating patients with mild depression and anxiety using cognitive behavioural therapy, has been found to increase life satisfaction by 2.7 points at an estimated cost of GBP 650 per patient (Gyani et al., 2013; Clark and Layard, 2014). This yields a benefit-cost ratio of 2.7 / 650 = 0.0042, somewhat more cost-effective than the NHSVR programme.

Finally, turning to a more traditional analysis which does not consider private wellbeing returns, recall that the median number of tasks completed was seven. At an assumed duration of one hour per task, this yields seven volunteering hours per volunteer. The UK minimum wage was GBP 8.72 per hour in April 2020 (UK Government, 2020b). With 225,069 volunteers by July 2020, this yields a total market value of GBP 13.7 million, After subtracting costs, this yields a net value of GBP 10.6 million.

## 7. Discussion and Conclusion

We estimated the causal wellbeing returns to volunteering in the NHSVR programme, by far the largest volunteer mobilisation in England since World War II. Volunteering had strong, positive effects on volunteers' wellbeing, raising their overall life satisfaction and sense of purpose in life as well as their feelings of belongingness to their immediate neighbourhood and social connectedness to their local community. We found sizeable impacts: for example, overall life satisfaction increased by about 0.21 points on a zero-to-ten scale, about 30% of the effect of being employed as opposed to being unemployed (+0.68, cf. Clark et al., 2018) or about 20% of the effect of local-community interventions aimed at raising the wellbeing and pro-sociality of the general population (+1.04, cf. Krekel et al., 2021; +1.1, cf. Heintzelman et al., 2020). The NHSVR programme can be seen as a scheme that provides purposeful volunteering activities in local communities in times of need.

Our identified effect for life satisfaction turns out to be in line with those reported in the quasi-experimental literature. Using matching, Binder and Freytag (2013) report effects between 0.14 and 0.18 points (volunteering: at least once during the past twelve months) and Borgonovi (2008), using selection on observables as well as instrumental variable estimation with state-level religious fragmentation as instrument, of up to 0.3 points (volunteering: monthly but less than weekly), rescaled to a zero-to-ten scale. Meier and Stutzer (2008) find an effect of 0.26 (volunteering: weekly or monthly) using a difference-in-differences design that exploits the exogenous shock to volunteering opportunities in East Germany due to the German reunification in the early 1990s. Together with the observed differences in mean life satisfaction between volunteers and non-volunteers in the nationally representative Understanding Society Covid-19 (USC19) Wave and the UCL Covid-19 Social Study (UCL19) (which are about 0.2 points, cf. Appendix Tables A6 and A7), a growing body of evidence, therefore, points towards an effect of volunteering on life satisfaction of about 0.2 points.

We find that wellbeing returns to volunteering are increasing in the number of tasks, with diminishing returns. Some measures, notably volunteer’s overall life satisfaction and their feelings of social connectedness to their local community, point towards a potential inverse U-shape, although we cannot statistically rule out diminishing returns. This may suggest that, at least for some wellbeing dimensions, there could to be an optimal amount of volunteering that is located neither at the lower nor at the upper end of the task frequency distribution. If so, this has practical implications for the optimal bunching or spacing out of tasks allocated to a volunteer in a given period of time, and can be an important insight to address concerns such as volunteer burnout (Bakker et al., 2006). Apps like GoodSAM may then be programmed such that they allocate tasks not only depending on distance but also depending on task history. As we cannot rule out diminishing returns, the potential inverse U-shape along the intensive margin is a promising area for future research.

There are other practical implications. The most important comes from our finding that wellbeing returns are stronger in environments where volunteers have more social interaction with the beneficiaries of their volunteering, a finding that resonates well with evidence from pro-social spending, in particular Aknin et al. (2013b)'s observation that making impact salient increases the wellbeing benefits to those who give. Alternatively, our result can also be explained in terms of lower entry costs to volunteering. To the extent that higher returns, in turn, attract more volunteers or make them supply more hours, environments could be designed in a way that allows for more social interaction and that makes participating in volunteering easy. To the extent that volunteering is a credence good and people mispredict the wellbeing benefits they may generate from volunteering (cf. Wilson and Gilbert, 2003; Stutzer and Odermatt, 2019), communication in recruitment and outreach should highlight these to potential volunteers. In a supplementary analysis, we found some evidence that volunteers generate higher wellbeing returns when being motivated by imperfectly altruistic reasons.[38] It may thus be interesting to experiment with open acknowledgement of volunteers (cf. Laffan and Dolan, 2020).

Our findings have several implications for policy. As its benefits outweigh its costs, the NHSVR programme could be seen as a model to learn from and to replicate in other countries, during the current and future crises (Churchill, 2020). It could also be run in normal times (for example, as a system to help the elderly or support vulnerable people in their

[38] See Online Appendix Table W3 for this analysis.

local community), not only directly benefiting the volunteers and their beneficiaries but also indirectly contributing to higher social cohesion. In fact, the NHS, UK Department for Health and Social Care, and Royal Voluntary Service, in June 2023, rolled out a new programme – the *NHS Care and Volunteer Responders (NHSCVR)* – aimed at bringing together volunteers and people in need for small health and social care tasks in their local communities. It was heavily inspired by the original NHSVR programme. The returns to wellbeing from volunteering – especially from a highly cost-effective and scalable scheme like the NHSVR programme – may also have important implications for *social prescribing* in public health, i.e. the referral of patients by GPs to non-medical, local-community interventions to improve their health-related behaviours and, thereby, their health and wellbeing. The notion of social prescribing has recently gained traction in research and policy in the UK (see the NHS Long Term Plan (2019), for example). As it is net-social-welfare-enhancing, a case can be made for public subsidies to organisations that promote volunteering, especially those with high benefits to beneficiaries. Our paper is the first to provide evidence on the benefits and effectiveness of such a large-scale, national volunteering programme.

There are several shortcomings to our study, some of which present themselves as promising avenues for future research. Our study suggests that the wellbeing returns from volunteering last for three months, when we collected our survey data. It is unclear, however, what the long-run impacts of volunteering on wellbeing are, not only in our context but also in the literature more generally. Do volunteers who continue to volunteer hedonically adapt at some point and stop generating wellbeing returns? So far, there is no evidence on this point. Do volunteers who stop volunteering continue to generate wellbeing returns or is there a return to baseline? Magnani and Zhu (2018), using nationally representative longitudinal data in Australia, find that stopping to volunteer yields a return to baseline within one year, yet more evidence is needed in other contexts. Moreover, it would be interesting to study whether higher wellbeing, in itself, has a causal effect on volunteering. That is, is there a dynamic relationship between wellbeing and volunteering whereby higher wellbeing at time $t$ leads to more volunteering at time $t+1$, which leads to even higher wellbeing at time $t+2$, and so on? Evidence in behavioural economics suggests that experimentally induced happiness leads to less selfish behaviour (Drouvelis and Powdthavee, 2015), which may point towards such a relationship, though Drouvelis and Grosskopf (2016) find that induced happiness has less impact on voluntary contributions in a public goods game. Our results on the intensive margin, however, cast some doubt on such a dynamic relationship, at least for lower levels of volunteering. Finally, once a crisis is over, do volunteers continue to volunteer, by substituting to alternative activities elsewhere? We know very little about wellbeing as antecedents and precedents over the volunteering life cycle. Lastly, it would be interesting to study whether volunteering (in our context or in general) has spillovers (cf. Dolan and Galizzi, 2015) on behaviours or attitudes in other life domains, for example pro-social spending.

Notwithstanding some limitations and unanswered questions, the results presented in this paper are strongly suggestive of significant and sizeable wellbeing returns to volunteering. They further highlight the need for policy-makers to not only encourage volunteering for the benefit of others but also to make salient the considerable personal benefits that come from pro-sociality.

## Acknowledgements

For helpful comments and suggestions, we are thankful to Richard Layard, Andrew Clark, Andrew Oswald, Nicolas Ziebarth, Maria Cotofan, Ekaterina (Katya) Oparina, Nils Mallock, Clemens Hetschko, Canishk Naik, and Matt Whittaker; conference participants at the IAREP-SABE 2021 conference; and seminar participants at the London School of Economics, University of Leeds, and various other places. Niall Maher provided excellent research assistance. A special thanks goes to the NHSVR programme staff, the NHS, the Royal Voluntary Service (especially Georgina Williams), GoodSAM, and the many thousand volunteers in the programme whose dedication, commitment, and effort made this research possible in the first place. We are truly grateful for the very valuable comments and suggestions of two anonymous referees, as well as for the inputs, guidance, and patience by the editor (Tavneet Suri).

We are indebted to Daisy Fancourt and the University College London Covid-19 Social Study team for providing access to their dataset. The University College London Covid-19 Social Study was funded by the Nuffield Foundation [WEL/FR-000022583], but the views expressed are those of the authors and not necessarily the Foundation. The study was also supported by the MARCH Mental Health Network funded by the Cross-Disciplinary Mental Health Network Plus initiative supported by UK Research and Innovation [ES/S002588/1], and by the Wellcome Trust [221400/Z/20/Z]. The researchers are grateful for the support of a number of organisations with their recruitment efforts including: the UKRI Mental Health Networks, Find Out Now, UCL BioResource, HealthWise Wales, SEO Works, FieldworkHub, and Optimal Workshop. The funders had no final role in the study design; in the collection, analysis and interpretation of data; in the writing of the report; or in the decision to submit the paper for publication.

For this research, ethics approval has been obtained from the LSE Research Division.

## References


Adler, A. (1938). *Social Interest: A Challenge to Mankind*. London: Faber & Faber.

Aknin, L. B., Barrington-Leigh, C. P., Dunn, E. W., Helliwell, J. F., Burns, J., Biswas-Diener, R., Kemeza, I., Nyende, P., Ashton-James, C. E., & Norton, M. I. (2013a). Prosocial Spending and Well-Being: Cross-Cultural Evidence for a Psychological Universal. *Journal of Personality and Social Psychology*, *104*(4), 635-652.

Aknin, L. B., Broesch, T., Hamlin, J. K., & Van de Vondervoort, J. (2015). Prosocial Behavior Leads to Happiness in a Small-Scale Rural Society. *Journal of Experimental Psychology: General*, *144*(4), 788-795.

Aknin, L. B., Dunn, E. W., Proulx, J., Lok, I., & Norton, M. I. (2020). Does Spending Money on Others Promote Happiness?: A Registered Replication Report. *Journal of Personality and Social Psychology*, *119*(2), e15-26.

Aknin, L. B., Dunn, E. W., Whillans, A. V., Grant, A. M., & Norton, M. I. (2013b). Making a difference matters: Impact unlocks the emotional benefits of prosocial spending. *Journal of Economic Behavior & Organization*, *88*, 90-95.

Al-Ubaydli, O., & Lee, M. (2011). Can Tailored Communications Motivate Environmental Volunteers? A Natural Field Experiment. *American Economic Review*, *101*(3), 323-328.

Andreoni, J. (1989). Giving with Impure Altruism: Applications to Charity and Ricardian Equivalence. *Journal of Political Economy*, *97*(6), 1447-1458.

Andreoni, J. (1990). Impure Altruism and Donations to Public Goods: A Theory of Warm-Glow Giving. *Economic Journal*, *100*(401), 464-477.

Ariely, D., Bracha, A., & Meier, S. (2009). Doing Good or Doing Well? Image Motivation and Monetary Incentives in Behaving Prosocially. *American Economic Review, 99*(1), 544-555.

Baert, S., & Vujic, S. (2018). Does it pay to care? Volunteering and employment opportunities. *Journal of Population Economics, 31*, 819-836.

Bakker, A. B., Van Der Zee, K. I., Lewig, K. A., & Dollard, M. F. (2006). The Relationship Between the Big Five Personality Factors and Burnout: A Study Among Volunteer Counselors. *Journal of Social Psychology*, *146*(1), 31-50.

Binder, M., & Freytag, A. (2013). Volunteering, subjective well-being and public policy. *Journal of Economic Psychology*, *34*, 97-119.

Bond, T. N., & Lang, K. (2019). The Sad Truth about Happiness Scales. *Journal of Political Economy*, *127*(4), 1629-1640.

Borgonovi, F. (2008). Doing well by doing good. The relationship between formal volunteering and self-reported health and happiness. *Social Science & Medicine*, *66*, 2321-2334.

Cassar, L., & Meier, S. (2021). Intentions for Doing Good Matter for Doing Well: The Negative Effects of Prosocial Incentives. *Economic Journal*, *131*(637), 1988-2017.

Chen, L.-Y., Oparina, E., Powdthavee, N., & Srisuma, S. (2022). *Robust Ranking of Happiness Outcomes: A Median Regression Perspective*. *Journal of Economic Behavior & Organization*, *200*, 672-686.

Churchill, N. (2020). Micro-volunteering at scale can help health systems respond to emergencies, such as the Covid-19 pandemic. *Patient Experience Journal*, *7(2)*, 160-164.

Clark, A. E., Flèche, S., Layard, R., Powdthavee, N., & Ward, G. (2018). *The Origins of Happiness: The Science of Well-Being Over the Life Course*. Princeton, NJ: Princeton University Press.

De Neve, J.-E., Ward, G., De Keulenaer, F., Van Landeghem, B., Kavetsos, G., & Norton, M. I. (2018). The Asymmetric Experience of Positive and Negative Economic Growth: Global Evidence Using Subjective Well-Being Data. *Review of Economics and Statistics*, *100*(2), 362-375.

Dolan, P., & Galizzi, M. M. (2015). Like ripples on a pond: Behavioral spillovers and their implications for research and policy. *Journal of Economic Psychology*, *47*, 1-16.

Dolan, P., Peasgood, T., & White, M. (2008). Do we really know what makes us happy? A review of the economic literature on the factors associated with subjective well-being. *Journal of Economic Psychology, 29*(1), 94-122.

Dolan, P., & Metcalfe, R. (2012). Measuring Subjective Wellbeing: Recommendations on Measures for use by National Governments. *Journal of Social Policy, 41*(2), 409-427.

Dolan, P., Kavetsos, G., Krekel, C., Mavridis, D., Metcalfe, R., Senik, C., Szymanski, S., & Ziebarth, N. (2019). Quantifying the intangible impact of the Olympics using subjective well-being data. *Journal of Public Economics, 177,* 104043.

Drouvelis, M., & Grosskopf, B. (2016). The effects of induced emotions on pro-social behaviour. *Journal of Public Economics, 134*, 1-8.

Drouvelis, M., & Powdthavee, N. (2015). Are happier people less judgmental of other people's selfish behaviors? Experimental survey evidence from trust and gift exchange games. *Journal of Behavioral and Experimental Economics, 58*, 111-123.

Dunn, E. W., Aknin, L. B., & Norton, M. I. (2008). Spending Money on Others Promotes Happiness. *Science, 319*(5870), 1687-1688.

Dunn, E. W., Aknin, L. B., & Norton, M. I. (2014). Prosocial Spending and Happiness: Using Money to Benefit Others Pays Off. *Current Directions in Psychological Science*, *23*(1), 41-47.

Falk, A., & Graeber, T. (2020). Delayed negative effects of prosocial spending on happiness. *Proceedings of the National Academy of Sciences*, *117*(2), 6463-6468.

Feldman, N. E. (2010). Time Is Money: Choosing between Charitable Activities. *American Economic Journal: Economic Policy*, *2*(1), 103-130.

Ferrer-i-Carbonell, A., & Frijters, P. (2004). How Important is Methodology for the Estimates of the Determinants of Happiness? *Economic Journal*, *114*(497), 641-659.

Freeman, R. B. (1997). Working for Nothing: The Supply of Volunteer Labor. *Journal of Labor Economics*, *15*(1), S140-S166.

Frijters, P., Clark, A. E., Krekel, C., & Layard, R. (2020). A happy choice: wellbeing as the goal of government. *Behavioural Public Policy*, *4*(2), 126-165.

Gyani, A., Shafran, R., Layard, R., & Clark, D. M. (2013). Enhancing recovery rates: Lessons from year one of IAPT. *Behaviour Research and Therapy*, *51*(9), 597-606.

Hackl, F., Halla, M., & Pruckner, G. J. (2007). Volunteering and Income – The Fallacy of the Good Samaritan? *Kyklos*, *60*(1), 77-104.

Heintzelman, S. J., Kushlev, K., Lutes, L. D., Wirtz, D., Kanippayoor, J. M., Leitner, D., Oishi, S., & Diener, E. (2020). ENHANCE: Evidence for the Efficacy of a Comprehensive Intervention Program to Promote Subjective Well-Being. *Journal of Experimental Psychology: Applied*, *26*(2), 360-383.

Heldman, C., & Israel-Trummel, M. (2012). The Double-Edged Sword of "Disaster Volunteerism": A Study of New Orleans Rebirth Movement Participants. *Journal of Political Science Education, 8*(4), 311-332.

HM Treasury (2021). *Green Book supplementary guidance: wellbeing*, Online: https://www.gov.uk/government/publications/green-book-supplementary-guidance-wellbeing, last accessed 14/11/2022.

Huang, L. H. (2019). Well-being and volunteering: Evidence from aging societies in Asia. *Social Science & Medicine*, *229*, 172-180.

Imbens, G. W., & Wooldridge, J. M. (2009). Recent Developments in the Econometrics of Program Evaluation. *Journal of Economic Literature*, *47*(1), 5-86.

Imbens, G. W., & Rubin, D. B. (2015). *Causal Inference for Statistics, Social, and Biomedical Sciences*. Cambridge University Press.

Jenkinson, C. E., Dickens, A. P., Jones, K., Thompson-Coon, J., Taylor, R. S., Rogers, M., Bambra, C. L., Lang, I., & Richards, S. H. (2013). Is volunteering a public health intervention? A systematic review and meta-analysis of the health and survival of volunteers. *BMC Public Health*, *13*(1), 773.

Jones, C., & Williamson, A. E. (2014). Volunteers working to support migrants in Glasgow: a qualitative study. *International Journal of Migration, Health, and Social Care*, *10*(4), 193-206.

Kahneman, D., & Deaton, A. (2010). High income improves evaluation of life but not emotional well-being. *Proceedings of the National Academy of Sciences*, *107*(38), 16489-16493.

Krekel, C., De Neve, J.-E., Fancourt, D., & Layard, R. (2021). A Local Community Course That Raises Wellbeing and Pro-Sociality: Evidence From a Randomised Controlled Trial. *Journal of Economic Behavior & Organization*, *188*, 322-336.

Laffan, K. M., and Dolan, P. (2020). In defence of charity which benefits both giver and receiver. *Nature Human Behaviour*, *4*, 670-672.

Lawton, R. N., Gramatki, I., Watt, W., & Fujiwara, D. (2020). Does Volunteering Make Us Happier, or Are Happier People More Likely to Volunteer? Addressing the Problem of Reverse Causality When Estimating the Wellbeing Impacts of Volunteering. *Journal of Happiness Studies*, *22*, 599-624.

Layard, R., & Clark, D. M. (2014). *Thrive: The Power of Psychological Therapy*. London: Penguin Books.

Lee, D. S. (2009). Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects. *Review of Economic Studies*, *76*(3), 1071-1102.

Levinson, A. (2012). Valuing public goods using happiness data: The case of air quality. *Journal of Public Economics*, *96*(9-10), 869-880.

Lindqvist, E., Östling R., & Cesarini, D. (2020). Long-run Effects of Lottery Wealth on Psychological Well-being. *Review of Economic Studies*, *87(6)*, 2703-2726.

Luechinger, S. (2009a). Valuing Air Quality Using the Life Satisfaction Approach. *Economic Journal*, *119*(536), 482-515.

Luechinger, S., & Raschky, P. (2009b). Valuing flood disasters using the life satisfaction approach. *Journal of Public Economics*, *93*(3-4), 620-633.

Magnani, E., & Zhu, R. (2018). Does kindness lead to happiness? Voluntary activities and subjective well-being. *Journal of Behavioral and Experimental Economics*, *77*, 20-28.

Meier, S., & Stutzer, A. (2008). Is Volunteering Rewarding in Itself? *Economica*, *75*, 39-59.

Mujcic, R., & Leibbrand, A. (2018). Indirect Reciprocity and Prosocial Behaviour: Evidence from a Natural Field Experiment. *Economic Journal*, *128*(611), 1683-1699.

NCVO (2019). *Time well spent: A national survey on the volunteering experience*. Online: https://www.ncvo.org.uk/policy-and-research/volunteering-policy/research/time-well-spent, last accessed 24/04/2021.

NHS Long Term Plan (2019). *The NHS Long Term Plan 2019*. Online: https://www.long-termplan.nhs.uk/wp-content/uploads/2019/08/nhs-long-term-plan-version-1.2.pdf, last accessed 20/04/2021.

NHS (2020). *NHS army of volunteers to start protecting vulnerable from coronavirus in England*. Online: https://www.england.nhs.uk/2020/04/nhs-volunteer-army-now-ready-to-support-even-more-people/, last accessed 22/04/2021.

Odermatt, R., & Stutzer, A. (2019). (Mis-)Predicted Subjective Well-Being Following Life Events. *Journal of the European Economic Association*, *17*(1), 245-283.

OECD (2015). *How's Life? Measuring Well-Being*. Paris: OECD Publishing.

ONS (2020). *Average household income, UK: financial year ending 2019*. Online: https://www.ons.gov.uk/peoplepopulationandcommunity/personalandhouseholdfinances/incomeandwealth/bulletins/householddisposableincomeandinequality/financialyearending2019, last accessed January 11, 2021.

Romano, J. P., & Wolf, M. (2005a). Stepwise Multiple Testing as Formalized Data Snooping. *Econometrica*, *73*(4), 1237-1282.

Romano, J. P., & Wolf, M. (2005b). Exact and Approximate Stepdown Methods for Multiple Hypothesis Testing. *Journal of the American Statistical Association*, *469*(100), 94-108.

Romano, J. P., & Wolf, M. (2016). Efficient computation of adjusted p-values for resampling-based stepdown multiple testing. *Statistics & Probability Letters*, *113*, 38-40.

Royal Voluntary Service (2020). *Findings: Patients Supported by the NHS Volunteer Responder Programme During Covid-19 – April to August 2020*. Online: https://www.royalvoluntaryservice.org.uk/media/p2him5vk/nhsvr_working_paper_one_patient_findings.pdf, last accessed 14/11/2022.

Russell, A. R., Nyame-Mensah, A., de Wit, A., & Handy, F. (2019). Volunteering and Well-being Among Ageing Adults: A Longitudinal Analysis. *VOLUNTAS: International Journal of Voluntary and Nonprofit Organizations*, *30*(1), 115-128.

Sauer, R. M. (2015). Does It Pay For Women to Volunteer? *International Economic Review*, *56*(2), 537-564.

Sacks, D. W., Stevenson, B., & Wolfers, J. (2010). Subjective Well-Being, Income, Economic Development and Growth. *NBER Working Paper*, 16441.

Schreier, H. M. C., Schonert-Reichl, K. A., & Chen, E. (2013). Effect of Volunteering on Risk Factors for Cardiovascular Disease in Adolescents: A Randomized Controlled Trial. *JAMA Pediatrics*, *167*(4), 327-332.

Son, J., & Wilson, J. (2015). The Psycho-Social Processes Linking Income and Volunteering: Chronic Financial Strain and Well-Being. *Sociological Forum*, *30(4)*, 1059-1081.

Stevenson, B., & Wolfers, J. (2008). Economic Growth and Subjective Well-Being: Reassessing the Easterlin Paradox. *Brookings Papers on Economic Activity*, *39*(1), 1-102.

Stutzer, A., Goette, L., & Zehnder, M. (2011). Active Decisions and Prosocial Behaviour: A Field Experiment on Blood Donation. *Economic Journal*, *121*(556), F476-F493.

Tabassum, F., Mohan, J., & Smith, P. (2016). Association of volunteering with mental well-being: a lifecourse analysis of a national population-based longitudinal study in the UK. *BMJ Open*, *6*(8), e011327.

UK Government (2020a). *Coronavirus (COVID-19) in the UK: UK Summary*. Online: https://coronavirus.data.gov.uk/, last accessed November 3, 2020.

UK Government (2020b). *National Minimum Wage and National Living Wage rates*. Online: https://www.gov.uk/national-minimum-wage-rates, last accessed January 11, 2021.

van Praag, B. M. S., & Baarsma, B. E. (2005). Using Happiness Surveys to Value Intangibles: The Case of Airport Noise. *Economic Journal*, *115*(500), 224-246.

Whillans, A. V., Seider, S. C., Chen, L., Dwyer, R. J., Novick, S., Gramigma, K. J., Mitchell, B. A., Savalei, V., Dickerson, S. S., and Dunn, E. W. (2016). Does volunteering improve well-being? *Comprehensive Results in Social Psychology*, *1(1-3)*, 35-50.

Wilson, T. D., & Gilbert, D. T. (2003). Affective forecasting. *Advances in Experimental Social Psychology*, *35*, 345-411.

## Tables

Table 1: Summary Statistics for Treatment and Control Group

| | Control<br>*Not Given Task*<br>Mean (SD) | Treatment<br>*Volunteered*<br>Mean (SD) | Diff.<br>Control - Treatment | P-Value | Normalised Diff.<br>Control - Treatment |
|---|---|---|---|---|---|
| *Individual Controls* | | | | | |
| Age: 16 to 24 | 0.0079 (0.0884) | 0.0096 (0.0978) | -0.0018 | 0.5882 | 0.0135 |
| 25 to 34 | 0.0402 (0.1965) | 0.0420 (0.2006) | -0.0018 | 0.7940 | 0.0064 |
| 35 to 44 | 0.0953 (0.2937) | 0.1095 (0.3123) | -0.0142 | 0.1787 | 0.0332 |
| 45 to 54 | 0.2159 (0.4116) | 0.2386 (0.4263) | -0.0227 | 0.1181 | 0.0384 |
| 55 to 64 | 0.3846 (0.4867) | 0.3948 (0.4889) | -0.0102 | 0.5433 | 0.0148 |
| 65 to 74 | 0.2386 (0.4264) | 0.1910 (0.3932) | 0.0476 | 0.0006 | 0.0821 |
| 75 to 84 | 0.0149 (0.1210) | 0.0118 (0.1081) | 0.0030 | 0.4295 | 0.0187 |
| Prefer Not to Say | 0.0026 (0.0512) | 0.0025 (0.0498) | 0.0001 | 0.9385 | 0.0019 |
| Gender: Male | 0.4152 (0.4930) | 0.3124 (0.4635) | 0.1028 | 0.0000 | 0.1520 |
| Female | 0.5795 (0.4938) | 0.6851 (0.4645) | -0.1056 | 0.0000 | 0.1557 |
| Other | 0.0009 (0.0296) | 0.0006 (0.0249) | 0.0003 | 0.7804 | 0.0065 |
| Prefer Not to Say | 0.0044 (0.0660) | 0.0019 (0.0432) | 0.0025 | 0.1473 | 0.0317 |
| Ethnicity: Asian / Asian British - Bangladeshi | 0.0052 (0.0723) | 0.0025 (0.0498) | 0.0028 | 0.1573 | 0.0314 |
| Asian / Asian British - Chinese | 0.0009 (0.0296) | 0.0050 (0.0704) | -0.0041 | 0.0558 | 0.0538 |
| Asian / Asian British - Indian | 0.0201 (0.1404) | 0.0268 (0.1614) | -0.0067 | 0.2160 | 0.0311 |
| Asian / Asian British - Pakistani | 0.0087 (0.0931) | 0.0075 (0.0861) | 0.0013 | 0.6742 | 0.0100 |
| Asian / Asian British - Other | 0.0061 (0.0780) | 0.0047 (0.0682) | 0.0015 | 0.5519 | 0.0140 |
| Black / African / Carib. / Black British - African | 0.0105 (0.1019) | 0.0106 (0.1023) | -0.0001 | 0.9798 | 0.0006 |
| Black / African / Carib. / Black British - Carib. | 0.0026 (0.0512) | 0.0034 (0.0584) | -0.0008 | 0.6814 | 0.0103 |
| Black / African / Carib. / Black British - Other | 0.0017 (0.0418) | 0.0019 (0.0432) | -0.0001 | 0.9359 | 0.0020 |
| Mixed / Multiple - White and Asian | 0.0026 (0.0512) | 0.0050 (0.0704) | -0.0024 | 0.2991 | 0.0271 |

| | | | | | |
|---|---|---|---|---|---|
| Mixed / Multiple - White and Black African | 0.0009 (0.0296) | 0.0009 (0.0305) | -0.0001 | 0.9547 | 0.0014 |
| Mixed / Multiple - White and Black Caribb. | 0.0000 (0.0000) | 0.0016 (0.0394) | -0.0016 | 0.1820 | 0.0395 |
| Mixed / Multiple - Other | 0.0044 (0.0660) | 0.0031 (0.0557) | 0.0013 | 0.5324 | 0.0146 |
| White - British / Engl. / Irish / Scottish / Welsh | 0.8514 (0.3559) | 0.8273 (0.3780) | 0.0241 | 0.0604 | 0.0464 |
| White - Gypsy or Irish Traveller | 0.0009 (0.0296) | 0.0003 (0.0176) | 0.0006 | 0.4453 | 0.0164 |
| White - Irish | 0.0149 (0.1210) | 0.0149 (0.1213) | -0.0001 | 0.9858 | 0.0004 |
| White - Other | 0.0621 (0.2414) | 0.0775 (0.2674) | -0.0154 | 0.0862 | 0.0428 |
| Other Ethnic Group - Arab | 0.0017 (0.0418) | 0.0025 (0.0498) | -0.0007 | 0.6530 | 0.0114 |
| Other Ethnic Group - Other | 0.0052 (0.0723) | 0.0047 (0.0682) | 0.0006 | 0.8086 | 0.0058 |
| Religion: None | 0.3549 (0.4787) | 0.3108 (0.4629) | 0.0441 | 0.0062 | 0.0662 |
| Buddhist | 0.0105 (0.1019) | 0.0106 (0.1023) | -0.0001 | 0.9798 | 0.0006 |
| Christian | 0.5533 (0.4974) | 0.5930 (0.4913) | -0.0397 | 0.0193 | 0.0568 |
| Hindu | 0.0052 (0.0723) | 0.0143 (0.1188) | -0.0091 | 0.0153 | 0.0652 |
| Jewish | 0.0026 (0.0512) | 0.0128 (0.1122) | -0.0101 | 0.0032 | 0.0822 |
| Muslim | 0.0271 (0.1624) | 0.0162 (0.1262) | 0.0109 | 0.0203 | 0.0531 |
| Sikh | 0.0052 (0.0723) | 0.0072 (0.0843) | -0.0019 | 0.4948 | 0.0172 |
| Other | 0.0201 (0.1404) | 0.0184 (0.1343) | 0.0017 | 0.7088 | 0.0090 |
| Prefer Not to Say | 0.0210 (0.1434) | 0.0168 (0.1285) | 0.0042 | 0.3602 | 0.0217 |
| Physical or Mental Health Condition: No | 0.7587 (0.4280) | 0.7321 (0.4429) | 0.0266 | 0.0782 | 0.0432 |
| Yes | 0.2212 (0.4152) | 0.2470 (0.4314) | -0.0259 | 0.0784 | 0.0432 |
| Don’t Know | 0.0114 (0.1060) | 0.0087 (0.0929) | 0.0027 | 0.4251 | 0.0188 |
| Prefer Not to Say | 0.0087 (0.0931) | 0.0121 (0.1095) | -0.0034 | 0.3500 | 0.0236 |
| Self-Isolating: No | 0.9274 (0.2595) | 0.9088 (0.2879) | 0.0186 | 0.0542 | 0.0480 |
| Yes | 0.0673 (0.2507) | 0.0856 (0.2798) | -0.0183 | 0.0517 | 0.0486 |
| Don’t Know | 0.0052 (0.0723) | 0.0056 (0.0746) | -0.0004 | 0.8890 | 0.0034 |
| Employment: Full-Time Employed | 0.2605 (0.4391) | 0.2647 (0.4413) | -0.0042 | 0.7807 | 0.0068 |
| Part-Time Employed | 0.1565 (0.3635) | 0.1457 (0.3529) | 0.0107 | 0.3815 | 0.0212 |
| Furloughed | 0.0656 (0.2476) | 0.0987 (0.2983) | -0.0332 | 0.0008 | 0.0855 |

| | | | | | |
|---|---|---|---|---|---|
| In Education | 0.0087 (0.0931) | 0.0103 (0.1009) | -0.0015 | 0.6520 | 0.0112 |
| Unemployed | 0.0524 (0.2230) | 0.0517 (0.2214) | 0.0008 | 0.9218 | 0.0024 |
| Disabled | 0.0122 (0.1100) | 0.0206 (0.1419) | -0.0083 | 0.0721 | 0.0463 |
| Retired | 0.3269 (0.4693) | 0.2946 (0.4559) | 0.0323 | 0.0412 | 0.0494 |
| Looking After Family | 0.0533 (0.2248) | 0.0564 (0.2307) | -0.0030 | 0.6994 | 0.0095 |
| Doing Something Else | 0.0638 (0.2445) | 0.0573 (0.2325) | 0.0065 | 0.4226 | 0.0193 |
| Motivation: Altruistic (i.e. Helping in Crisis) | 0.9851 (0.1210) | 0.9832 (0.1285) | 0.0019 | 0.6561 | 0.0110 |
| Thought It Was Expected | 0.0839 (0.2774) | 0.0719 (0.2583) | 0.0120 | 0.1843 | 0.0318 |
| Like Telling Family, Friends | 0.0393 (0.1945) | 0.0781 (0.2684) | -0.0388 | 0.0000 | 0.1170 |
| Was Asked | 0.0157 (0.1245) | 0.0134 (0.1149) | 0.0024 | 0.5604 | 0.0139 |
| Like Helping People | 0.6189 (0.4859) | 0.7576 (0.4286) | -0.1387 | 0.0000 | 0.2141 |
| Wanted to Meet People | 0.0542 (0.2265) | 0.0548 (0.2275) | -0.0006 | 0.9425 | 0.0018 |
| Gain Skills | 0.1337 (0.3405) | 0.1724 (0.3778) | -0.0386 | 0.0023 | 0.0760 |
| Pursue Career | 0.0428 (0.2026) | 0.0557 (0.2294) | -0.0129 | 0.0934 | 0.0420 |
| Had Some Time | 0.1040 (0.3054) | 0.1185 (0.3233) | -0.0145 | 0.1857 | 0.0327 |
| Other | 0.0315 (0.1747) | 0.0436 (0.2041) | -0.0121 | 0.0745 | 0.0450 |
| Volunteered Before: No | 0.2185 (0.4134) | 0.2122 (0.4089) | 0.0063 | 0.6537 | 0.0109 |
| Yes | 0.7815 (0.4134) | 0.7878 (0.4089) | -0.0063 | 0.6537 | 0.0109 |
| Volunteering Elsewhere: No | 0.6337 (0.4820) | 0.6658 (0.4718) | -0.0321 | 0.0495 | 0.0476 |
| Yes, One Other Group | 0.2203 (0.4146) | 0.1923 (0.3942) | 0.0280 | 0.0419 | 0.0489 |
| Yes, More Than One Other Group | 0.1460 (0.3532) | 0.1419 (0.3490) | 0.0041 | 0.7338 | 0.0083 |
| If Elsewhere: Informal Local Group | 0.0542 (0.2265) | 0.0448 (0.2069) | 0.0094 | 0.1987 | 0.0306 |
| Organised Local Group | 0.1206 (0.3258) | 0.0983 (0.2978) | 0.0223 | 0.0339 | 0.0505 |
| National Charity | 0.0385 (0.1924) | 0.0411 (0.1985) | -0.0026 | 0.7004 | 0.0094 |
| Public Sector | 0.0306 (0.1723) | 0.0314 (0.1745) | -0.0008 | 0.8897 | 0.0034 |
| Sports Group | 0.0201 (0.1404) | 0.0168 (0.1285) | 0.0033 | 0.4665 | 0.0174 |
| Faith Group | 0.0411 (0.1986) | 0.0448 (0.2069) | -0.0037 | 0.5977 | 0.0130 |
| Other | 0.0612 (0.2398) | 0.0569 (0.2318) | 0.0043 | 0.5976 | 0.0127 |

| | | | | | |
|---|---|---|---|---|---|
| *Regional Covid-19 Controls* | | | | | |
| New Covid-19 Hospital Admissions | 12.6565 (6.8428) | 12.4051 (6.7985) | 0.2514 | 0.2837 | 0.0261 |
| Cumulative Covid-19 Hospital Admissions | 15,350 (5,491) | 16,079 (5,471) | -728.5621 | 0.0001 | 0.0940 |
| Current Covid-19 Hospital Cases | 209.1635 (106.8508) | 215.1842 (105.7360) | -6.0207 | 0.0991 | 0.0401 |
| Occupied Medical Ventilation Beds | 18.8103 (11.3749) | 19.6739 (11.3913) | -0.8636 | 0.0277 | 0.0536 |
| New Covid-19 Cases | 63.7063 (41.2581) | 68.4157 (40.9520) | -4.7094 | 0.0009 | 0.0810 |
| Cumulative Covid-19 Cases | 34,873 (11,359) | 36,121 (11,053) | -1,248.0149 | 0.0011 | 0.0787 |
| New Covid-19 Deaths | 3.4030 (2.8203) | 3.1033 (2.5458) | 0.2997 | 0.0009 | 0.0789 |
| Cumulative Covid-19 Deaths | 5,111 (1,607) | 5,302 (1,573) | -190.7463 | 0.0005 | 0.0848 |
| N | 1,144 | 3,214 | - | - | - |

*Notes:* Normalised differences are calculated as $\Delta x = (\bar{x}_t - \bar{x}_c) / \sqrt{(\sigma_t^2 + \sigma_c^2)}$, where $\bar{x}_t$ and $\bar{x}_c$ is the sample mean of the covariate in the treatment and control group, respectively. $\sigma^2$ denotes the respective variance. A normalised difference greater than 0.25 suggests a non-balanced covariate (Imbens and Wooldridge, 2009; Imbens and Rubin, 2015).
*Source:* NHSVR Survey Data, Administrative Data, July 2020; own calculations.

Table 2: Universe of Volunteers vs. Main Estimation Sample

| | Universe<br>Mean (SD) | Main<br>Mean (SD) | Diff.<br>Universe - Main | P-Value | Normalised Diff.<br>Universe - Main |
|---|---|---|---|---|---|
| *Groups*<br>*i.e. Treated (Allocated, Accepted, and Completed Task) vs. Controlled (Not Allocated Task)* | | | | | |
| Share of Treated: Total | 0.6135 (0.4892) | 0.7375 (0.4400) | -0.1240 | 0.0000 | 0.2037 |
| Transport | 0.2125 (0.4091) | 0.3434 (0.4750) | -0.1309 | 0.0000 | 0.2088 |
| Community Response | 0.5711 (0.4949) | 0.7071 (0.4552) | -0.1360 | 0.0000 | 0.2022 |
| Check In And Chat | 0.6191 (0.4856) | 0.8025 (0.3982) | -0.1834 | 0.0000 | 0.2921 |
| Share of Controlled: Total | 0.3855 (0.4892) | 0.2625 (0.4400) | 0.1230 | 0.0000 | 0.2037 |
| Transport | 0.7875 (0.4091) | 0.6566 (0.4750) | 0.1309 | 0.0000 | 0.2088 |
| Community Response | 0.4289 (0.4949) | 0.2929 (0.4552) | 0.1360 | 0.0000 | 0.2022 |
| Check In And Chat | 0.3809 (0.4856) | 0.1975 (0.3982) | 0.1834 | 0.0000 | 0.2921 |
| *Tasks and Task Behaviour* | | | | | |
| Number of Allocated Tasks: Total | 10.9073 (37.0730) | 19.1480 (37.1981) | -8.2407 | 0.0000 | 0.1569 |
| Transport | 0.3441 (3.7379) | 0.5670 (4.8050) | -0.2229 | 0.0001 | 0.0366 |
| Community Response | 6.8339 (28.2475) | 11.6184 (28.3870) | -4.7845 | 0.0000 | 0.1195 |
| Check In And Chat | 3.7293 (14.8690) | 6.9626 (18.0604) | -3.2333 | 0.0000 | 0.1382 |
| Number of Rejected Tasks | 2.4352 (7.5921) | 3.5106 (9.7447) | -1.0754 | 0.0000 | 0.0871 |
| Number of Timed Out Tasks | 6.2393 (32.2777) | 4.3148 (14.5944) | 1.9245 | 0.0001 | 0.0543 |
| Waiting Time for First Task | 28.4762 (25.7956) | 30.8502 (26.7874) | -2.3740 | 0.0000 | 0.0638 |
| *Volunteers* | | | | | |
| Share of Volunteers: Transport | 0.6071 (0.4884) | 0.5156 (0.4998) | 0.0915 | 0.0000 | 0.1309 |
| Community Response | 0.6985 (0.4589) | 0.5959 (0.4908) | 0.1026 | 0.0000 | 0.1527 |

| | | | | | |
|---|---|---|---|---|---|
| Check In And Chat | 0.5642 (0.4959) | 0.6498 (0.4771) | -0.0857 | 0.0000 | 0.1245 |
| Age: 16 to 24 | 0.0845 (0.2782) | 0.0094 (0.0967) | 0.0751 | 0.0000 | 0.2550 |
| 25 to 34 | 0.2207 (0.4147) | 0.0387 (0.1928) | 0.1820 | 0.0000 | 0.3980 |
| 35 to 44 | 0.1923 (0.3941) | 0.1038 (0.3050) | 0.0885 | 0.0000 | 0.1777 |
| 45 to 54 | 0.2317 (0.4219) | 0.2209 (0.4149) | 0.0108 | 0.0920 | 0.0183 |
| 55 to 64 | 0.2079 (0.4058) | 0.3921 (0.4883) | -0.1842 | 0.0000 | 0.2901 |
| 65 to 74 | 0.0597 (0.2369) | 0.2195 (0.4140) | -0.1598 | 0.0000 | 0.3351 |
| 75 to 84 | 0.0031 (0.0559) | 0.0156 (0.1241) | -0.0125 | 0.0000 | 0.0920 |
| N | 366,482 | 4,358 | - | - | - |

*Notes:* Normalised differences are calculated as $\Delta x = (\bar{x}_t - \bar{x}_c) / \sqrt{(\sigma_t^2 + \sigma_c^2)}$, where $\bar{x}_t$ and $\bar{x}_c$ is the sample mean of the covariate in the treatment and control group, respectively (here: different samples). $\sigma^2$ denotes the respective variance. A normalised difference greater than 0.25 suggests a non-balanced covariate (Imbens and Wooldridge, 2009; Imbens and Rubin, 2015).
*Source:* NHSVR Survey Data, Administrative Data, July 2020; own calculations.

Table 3: Average Treatment Effects

| | Life Satisfaction | | Sense of Purpose in Life | | Belongingness | | Connectedness | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| $Treatment_i$ | 0.1814** | 0.2092*** | 0.2672*** | 0.2325*** | 0.0270* | 0.0272* | 0.0700*** | 0.0646*** |
| *(Volunteered Vs. Not Given Task)* | (0.0746) | (0.0726) | (0.0725) | (0.0720) | (0.0163) | (0.0164) | (0.0178) | (0.0181) |
| Stepdown P-Value ($Treatment_i$) | 0.0297 | 0.0198 | 0.0594 | 0.0099 | 0.0594 | 0.0396 | 0.0099 | 0.0099 |
| Lee (2009) Lower Bounds | 0.1367** | | 0.2153*** | | 0.0211 | | 0.0632*** | |
| | (0.0684) | | (0.0658) | | (0.0146) | | (0.0191) | |
| Upper Bounds | 0.3125*** | | 0.4084*** | | 0.0447*** | | 0.0876*** | |
| | (0.0706) | | (0.0718) | | (0.0146) | | (0.0181) | |
| Individual Controls | No | Yes | No | Yes | No | Yes | No | Yes |
| Regional Covid-19 Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Postcode Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Date Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Scaling | 0-10 | 0-10 | 0-10 | 0-10 | 0-1 | 0-1 | 0-1 | 0-1 |
| Mean | 7.3 | 7.3 | 7.6 | 7.6 | 0.7 | 0.7 | 0.5 | 0.5 |
| σ | 2.1 | 2.1 | 2.0 | 2.0 | 0.5 | 0.5 | 0.5 | 0.5 |
| Number of Observations | 4,358 | 4,358 | 4,358 | 4,358 | 4,358 | 4,358 | 4,358 | 4,358 |
| Number of Treated | 3,214 | 3,214 | 3,214 | 3,214 | 3,214 | 3,214 | 3,214 | 3,214 |
| Number of Controlled | 1,144 | 1,144 | 1,144 | 1,144 | 1,144 | 1,144 | 1,144 | 1,144 |
| R Squared | 0.0414 | 0.1276 | 0.0426 | 0.1132 | 0.0487 | 0.0875 | 0.0595 | 0.0909 |

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

*Notes:* $Treatment_i$ takes on one if a volunteer has been allocated, has accepted, and has completed at least one task, and zero if a volunteer has not been allocated a task yet, at the time of the survey. The zero-category excludes individuals who have rejected a task. It is constructed using the archive of all tasks and their timestamps in administrative records. See Section 3.1 for a description of our data and Table 1 for summary statistics.
*Source:* NHSVR Survey Data, Administrative Data, July 2020; own calculations.

Table 4: Average Treatment Effect Intensity (% of Task Distribution)

| Treatment$_i$ *(Volunteered Vs. Not Given Task)* | Life Satisfaction (1) | (2) | Sense of Purpose in Life (3) | (4) | Belongingness (5) | (6) | Connectedness (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| <10% *(1 Task)* | 0.1030 | 0.0960 | 0.1663 | 0.1428 | 0.0225 | 0.0269 | -0.0113 | -0.0097 |
| | (0.1455) | (0.1409) | (0.1474) | (0.1436) | (0.0316) | (0.0311) | (0.0341) | (0.0338) |
| 10% to 25% *(2 Tasks)* | -0.0212 | 0.0622 | 0.1974 | 0.2177* | 0.0292 | 0.0353 | 0.0339 | 0.0308 |
| | (0.1362) | (0.1329) | (0.1250) | (0.1235) | (0.0293) | (0.0291) | (0.0327) | (0.0326) |
| 25% to 50% *(3 to 7 Tasks)* | 0.0557 | 0.1341 | 0.1007 | 0.1009 | 0.0202 | 0.0257 | 0.0594*** | 0.0585*** |
| | (0.0907) | (0.0893) | (0.0885) | (0.0880) | (0.0199) | (0.0200) | (0.0218) | (0.0220) |
| 50% to 75% *(8 to 16 Tasks)* | 0.2525** | 0.2511*** | 0.3666*** | 0.2951*** | 0.0286 | 0.0226 | 0.0887*** | 0.0804*** |
| | (0.1004) | (0.0975) | (0.0938) | (0.0931) | (0.0218) | (0.0220) | (0.0240) | (0.0243) |
| 75% to 90% *(17 to 35 Tasks)* | 0.4188*** | 0.4180*** | 0.4269*** | 0.3689*** | 0.0352 | 0.0324 | 0.1200*** | 0.1131*** |
| | (0.1144) | (0.1101) | (0.1079) | (0.1054) | (0.0257) | (0.0258) | (0.0285) | (0.0287) |
| >90% *(36+ Tasks)* | 0.4677*** | 0.3927*** | 0.6335*** | 0.5079*** | 0.0399 | 0.0274 | 0.1227*** | 0.1054*** |
| | (0.1376) | (0.1331) | (0.1313) | (0.1286) | (0.0301) | (0.0303) | (0.0331) | (0.0331) |
| | | | | | | | | |
| Stepdown P-Value (<10%) | 0.8218 | 0.7822 | 0.5446 | 0.7129 | 0.8218 | 0.7228 | 0.9109 | 0.8020 |
| Stepdown P-Value (10% to 25%) | 0.9802 | 0.5347 | 0.4158 | 0.2079 | 0.4455 | 0.5149 | 0.4455 | 0.5347 |
| Stepdown P-Value (25% to 50%) | 0.6535 | 0.3366 | 0.6535 | 0.3762 | 0.6535 | 0.3762 | 0.0198 | 0.0396 |
| Stepdown P-Value (50% to 75%) | 0.0396 | 0.0198 | 0.0099 | 0.0099 | 0.1089 | 0.2376 | 0.0099 | 0.0099 |
| Stepdown P-Value (75% to 90%) | 0.0099 | 0.0198 | 0.0099 | 0.0198 | 0.2079 | 0.1980 | 0.0099 | 0.0198 |
| Stepdown P-Value (>90%) | 0.0198 | 0.0198 | 0.0099 | 0.0099 | 0.1782 | 0.3069 | 0.0099 | 0.0099 |
| | | | | | | | | |
| Individual Controls | No | Yes | No | Yes | No | Yes | No | Yes |
| Regional Covid-19 Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Postcode Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Date Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Number of Observations | 4,358 | 4,358 | 4,358 | 4,358 | 4,358 | 4,358 | 4,358 | 4,358 |
| Number of Treated | 3,214 | 3,214 | 3,214 | 3,214 | 3,214 | 3,214 | 3,214 | 3,214 |
| Number of Controlled | 1,144 | 1,144 | 1,144 | 1,144 | 1,144 | 1,144 | 1,144 | 1,144 |
| R Squared | 0.0456 | 0.1299 | 0.0478 | 0.1162 | 0.0488 | 0.0875 | 0.0634 | 0.0939 |

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

*Notes:* $\text{Treatment}_i$ reflects the number of tasks in different categories of the overall frequency distribution that have been allocated to, have been accepted by, and have been completed by each volunteer (which is zero for those who have not been allocated a task yet) at the time of the survey. The dummy *25% to 50%*, for example, takes on one for those volunteers in our treatment group who have completed between 25% and 50% of the tasks in the overall task frequency distribution, and zero for volunteers in our control group. The zero-category excludes individuals who have rejected a task. It is constructed using the archive of all tasks and their timestamps in administrative records. See Section 3.1 for a description of our data and Table 1 for summary statistics.
*Source:* NHSVR Survey Data, Administrative Data, July 2020; own calculations.

Table 5: Heterogeneous Treatment Effects By Type of Task

| | Life Satisfaction | | Sense of Purpose in Life | | Belongingness | | Connectedness | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Transport and Community Response* | | | | | | | | |
| Treatment$_i$ | 0.1557* | 0.1564* | 0.2364*** | 0.1798** | 0.0243 | 0.0215 | 0.1054*** | 0.0922*** |
| *(Volunteered Vs. Not Given Task)* | (0.0903) | (0.0889) | (0.0863) | (0.0865) | (0.0195) | (0.0196) | (0.0216) | (0.0220) |
| Stepdown P-Value (Treatment$_i$) | 0.1386 | 0.1089 | 0.0198 | 0.0891 | 0.1584 | 0.2376 | 0.0099 | 0.0099 |
| Lee (2009) Lower Bounds | 0.1590* | | 0.2241*** | | 0.0277 | | 0.1067*** | |
| | (0.0871) | | (0.0839) | | (0.0181) | | (0.0201) | |
| Upper Bounds | 0.2964*** | | 0.3815*** | | 0.0481** | | 0.1286*** | |
| | (0.0942) | | (0.0856) | | (0.0190) | | (0.0202) | |
| Number of Observations | 2,879 | 2,879 | 2,879 | 2,879 | 2,879 | 2,879 | 2,879 | 2,879 |
| Number of Treated | 2,053 | 2,053 | 2,053 | 2,053 | 2,053 | 2,053 | 2,053 | 2,053 |
| Number of Controlled | 826 | 826 | 826 | 826 | 826 | 826 | 826 | 826 |
| R Squared | 0.0606 | 0.1469 | 0.0667 | 0.1432 | 0.0714 | 0.1086 | 0.0860 | 0.1211 |
| *Check In and Chat* | | | | | | | | |
| Treatment$_i$ | 0.3390*** | 0.3395*** | 0.3480*** | 0.3159*** | 0.0383* | 0.0373 | 0.0288 | 0.0280 |
| *(Volunteered Vs. Not Given Task)* | (0.1051) | (0.1017) | (0.1010) | (0.0992) | (0.0231) | (0.0231) | (0.0253) | (0.0254) |
| Stepdown P-Value (Treatment$_i$) | 0.0198 | 0.0099 | 0.0099 | 0.0099 | 0.2673 | 0.2277 | 0.2673 | 0.2871 |
| Lee (2009) Lower Bounds | 0.2292** | | 0.2429*** | | 0.0205 | | 0.0149 | |
| | (0.0909) | | (0.0913) | | (0.0209) | | (0.0237) | |
| Upper Bounds | 0.4917*** | | 0.5199*** | | 0.0536** | | 0.0490** | |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (0.1028) | | (0.0892) | | (0.0209) | | (0.0228) | |
| Number of Observations | 2,760 | 2,760 | 2,760 | 2,760 | 2,760 | 2,760 | 2,760 | 2,760 |
| Number of Treated | 2,215 | 2,215 | 2,215 | 2,215 | 2,215 | 2,215 | 2,215 | 2,215 |
| Number of Controlled | 545 | 545 | 545 | 545 | 545 | 545 | 545 | 545 |
| R Squared | 0.0677 | 0.1517 | 0.0661 | 0.1328 | 0.0665 | 0.1121 | 0.0825 | 0.1261 |
| Individual Controls | No | Yes | No | Yes | No | Yes | No | Yes |
| Regional Covid-19 Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Postcode Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Date Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

*Notes:* Treatment$_i$ takes on one if a volunteer has been allocated, has accepted, and has completed at least one task in a specific service, and zero if a volunteer has not been allocated a task yet, at the time of the survey. The zero-category excludes individuals who have rejected a task. It is constructed using the archive of all tasks and their timestamps in administrative records. See Section 3.1 for a description of our data and Table 1 for summary statistics.
*Source:* NHSVR Survey Data, Administrative Data, July 2020; own calculations.

Table 6: Heterogeneous Treatment Effect Intensity (% of Task Distribution) By Type of Task

| | Life Satisfaction | | Sense of Purpose in Life | | Belongingness | | Connectedness | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Transport and Community Response* | | | | | | | | |
| <10% *(1 Task)* | 0.1738 | 0.1524 | 0.1994 | 0.2028 | 0.0144 | 0.0266 | -0.0087 | -0.0110 |
| | (0.1978) | (0.1914) | (0.1837) | (0.1790) | (0.0436) | (0.0436) | (0.0472) | (0.0470) |
| 10% to 25% *(2 to 4 Tasks)* | 0.0711 | 0.1254 | 0.0974 | 0.0909 | 0.0484* | 0.0473* | 0.1140*** | 0.1044*** |
| | (0.1325) | (0.1302) | (0.1238) | (0.1226) | (0.0279) | (0.0274) | (0.0319) | (0.0319) |
| 25% to 50% *(5 to 10 Tasks)* | 0.1235 | 0.1255 | 0.1722 | 0.1047 | 0.0047 | 0.0035 | 0.0953*** | 0.0791*** |
| | (0.1182) | (0.1164) | (0.1125) | (0.1122) | (0.0262) | (0.0265) | (0.0288) | (0.0292) |
| 50% to 75% *(11 to 20 Tasks)* | 0.0577 | 0.0392 | 0.2292* | 0.1529 | 0.0354 | 0.0317 | 0.1173*** | 0.1068*** |
| | (0.1313) | (0.1286) | (0.1209) | (0.1204) | (0.0272) | (0.0276) | (0.0305) | (0.0308) |
| 75% to 90% *(21 to 43 Tasks)* | 0.4106*** | 0.3996*** | 0.4068*** | 0.3352*** | 0.0110 | 0.0035 | 0.1352*** | 0.1223*** |
| | (0.1370) | (0.1328) | (0.1295) | (0.1268) | (0.0312) | (0.0313) | (0.0342) | (0.0346) |
| >90% *(44+ Tasks)* | 0.2608 | 0.2268 | 0.5352*** | 0.4244*** | 0.0296 | 0.0114 | 0.1385*** | 0.1211*** |
| | (0.1676) | (0.1621) | (0.1569) | (0.1554) | (0.0363) | (0.0367) | (0.0404) | (0.0407) |
| | | | | | | | | |
| Stepdown P-Value (<10%) | 0.6436 | 0.7624 | 0.6040 | 0.5842 | 0.9010 | 0.7624 | 0.9010 | 0.8515 |
| Stepdown P-Value (10% to 25%) | 0.5446 | 0.5149 | 0.5446 | 0.5149 | 0.1386 | 0.2079 | 0.0099 | 0.0099 |
| Stepdown P-Value (25% to 50%) | 0.5050 | 0.6040 | 0.2376 | 0.6931 | 0.9307 | 0.9208 | 0.0099 | 0.0495 |
| Stepdown P-Value (50% to 75%) | 0.6040 | 0.7723 | 0.1386 | 0.4752 | 0.3564 | 0.4752 | 0.0099 | 0.0099 |
| Stepdown P-Value (75% to 90%) | 0.0099 | 0.0198 | 0.0099 | 0.0198 | 0.7921 | 0.9109 | 0.0099 | 0.0198 |
| Stepdown P-Value (>90%) | 0.1782 | 0.2277 | 0.0099 | 0.0099 | 0.4356 | 0.6931 | 0.0099 | 0.0099 |
| | | | | | | | | |
| Number of Observations | 2,879 | 2,879 | 2,879 | 2,879 | 2,879 | 2,879 | 2,879 | 2,879 |
| Number of Treated | 2,053 | 2,053 | 2,053 | 2,053 | 2,053 | 2,053 | 2,053 | 2,053 |
| Number of Controlled | 826 | 826 | 826 | 826 | 826 | 826 | 826 | 826 |
| R Squared | 0.0629 | 0.1489 | 0.0698 | 0.1453 | 0.0723 | 0.1095 | 0.0892 | 0.1239 |

| *Check In and Chat* | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| <10% *(1 Task)* | 0.2854 | 0.2893 | 0.2333 | 0.2440 | 0.0161 | 0.0193 | -0.0457 | -0.0334 |
| | (0.1920) | (0.1901) | (0.1995) | (0.1966) | (0.0418) | (0.0414) | (0.0453) | (0.0449) |
| 10% to 25% *(2 Tasks)* | 0.2285 | 0.2633 | 0.2249 | 0.2372 | 0.0313 | 0.0357 | -0.0619 | -0.0550 |
| | (0.1689) | (0.1643) | (0.1585) | (0.1557) | (0.0371) | (0.0374) | (0.0408) | (0.0411) |
| 25% to 50% *(3 to 7 Tasks)* | 0.2051* | 0.2668** | 0.1931* | 0.2015* | 0.0368 | 0.0426 | 0.0203 | 0.0240 |
| | (0.1199) | (0.1172) | (0.1161) | (0.1146) | (0.0266) | (0.0265) | (0.0292) | (0.0293) |
| 50% to 75% *(8 to 16 Tasks)* | 0.3946*** | 0.3441*** | 0.4783*** | 0.3885*** | 0.0357 | 0.0246 | 0.0509 | 0.0422 |
| | (0.1328) | (0.1289) | (0.1234) | (0.1226) | (0.0293) | (0.0294) | (0.0321) | (0.0322) |
| 75% to 90% *(17 to 34 Tasks)* | 0.5999*** | 0.5501*** | 0.5464*** | 0.4882*** | 0.0606* | 0.0583* | 0.1000*** | 0.0988*** |
| | (0.1544) | (0.1497) | (0.1423) | (0.1392) | (0.0344) | (0.0344) | (0.0382) | (0.0383) |
| >90% *(35+ Tasks)* | 0.6288*** | 0.5191*** | 0.7173*** | 0.5967*** | 0.0550 | 0.0414 | 0.1138*** | 0.0948** |
| | (0.1836) | (0.1752) | (0.1724) | (0.1688) | (0.0395) | (0.0401) | (0.0428) | (0.0428) |
| | | | | | | | | |
| Stepdown P-Value (<10%) | 0.5149 | 0.3663 | 0.5545 | 0.4950 | 0.7228 | 0.6733 | 0.7228 | 0.6733 |
| Stepdown P-Value (10% to 25%) | 0.4752 | 0.3267 | 0.4752 | 0.3267 | 0.4752 | 0.3663 | 0.4752 | 0.3366 |
| Stepdown P-Value (25% to 50%) | 0.2673 | 0.0495 | 0.3267 | 0.2376 | 0.3762 | 0.2376 | 0.4059 | 0.3762 |
| Stepdown P-Value (50% to 75%) | 0.0396 | 0.0495 | 0.0099 | 0.0099 | 0.2277 | 0.3960 | 0.2178 | 0.3267 |
| Stepdown P-Value (75% to 90%) | 0.0099 | 0.0099 | 0.0099 | 0.0099 | 0.0891 | 0.0792 | 0.0198 | 0.0198 |
| Stepdown P-Value (>90%) | 0.0297 | 0.0396 | 0.0198 | 0.0099 | 0.1386 | 0.2574 | 0.0297 | 0.0594 |
| | | | | | | | | |
| Number of Observations | 2,760 | 2,760 | 2,760 | 2,760 | 2,760 | 2,760 | 2,760 | 2,760 |
| Number of Treated | 2,215 | 2,215 | 2,215 | 2,215 | 2,215 | 2,215 | 2,215 | 2,215 |
| Number of Controlled | 545 | 545 | 545 | 545 | 545 | 545 | 545 | 545 |
| R Squared | 0.0720 | 0.1536 | 0.0723 | 0.1361 | 0.0670 | 0.1126 | 0.0902 | 0.1318 |
| | | | | | | | | |
| Individual Controls | No | Yes | No | Yes | No | Yes | No | Yes |
| Regional Covid-19 Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Postcode Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Date Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

*Notes:* $Treatment_i$ reflects the number of tasks in different categories of the task-specific overall frequency distribution that have been allocated to, have been accepted by, and have been completed by each volunteer in a specific service (which is zero for those who have not been allocated a task yet) at the time of the survey. The dummy *25% to 50%*, for example, takes on one for those volunteers in our treatment group who have completed between 25% and 50% of the tasks in the overall task frequency distribution, and zero for volunteers in our control group. The zero-category excludes individuals who have rejected a task. It is constructed using the archive of all tasks and their timestamps in administrative records. See Section 3.1 for a description of our data and Table 1 for summary statistics.
*Source:* NHSVR Survey Data, Administrative Data, July 2020; own calculations.

Table 7: Predicting Survey Response From Having Been Treated in Administrative Records (Response in Combined Dataset of Survey and Administrative Data)

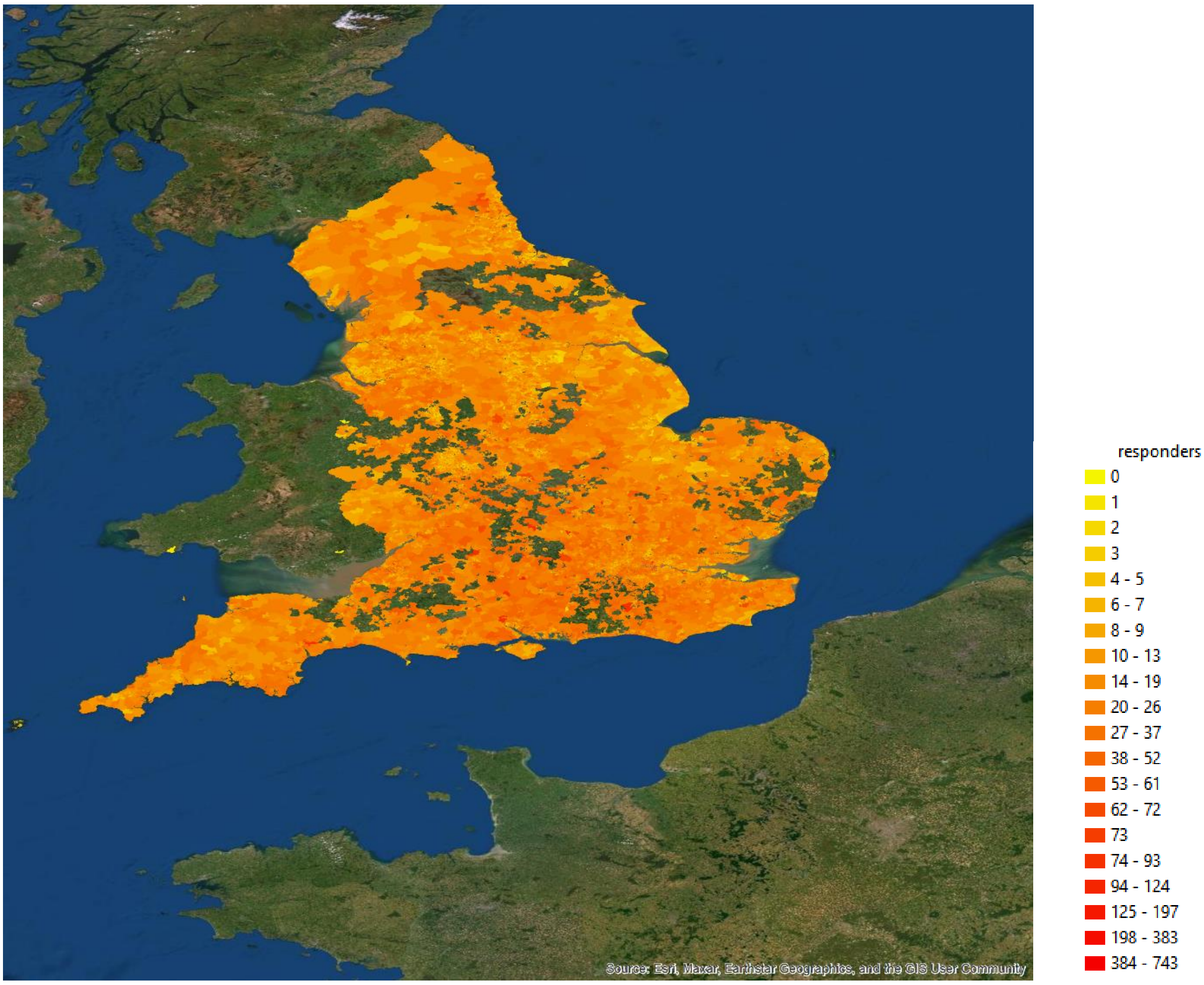
| | Response (0-1) | |
|---|---|---|
| | (1) | (2) |
| $Treatment_i$ | 0.0254*** | 0.0063*** |
| *(Volunteered Vs. Not Given Task)* | (0.0005) | (0.0015) |
| | | |
| Controls | | |
| Tasks and Task Behaviour | No | Yes |
| Postcode Fixed Effects | No | Yes |
| | | |
| N | 331,521 | 331,521 |
| R Squared | 0.0105 | 0.0332 |

*Notes:* $Treatment_i$ takes on one if a volunteer has been allocated, has accepted, and has completed at least one task in a specific service, and zero if a volunteer has not been allocated a task yet, at the time of the survey. The zero-category excludes individuals who have rejected a task. It is constructed using the archive of all tasks and their timestamps in administrative records. The controls include administrative variables on tasks and task behaviour (i.e. the services of volunteering and the number of tasks in each service); the postcode fixed effects are 124 postcode area fixed effects, as in our baseline specification.
*Source:* NHSVR Survey Data, Administrative Data, July 2020; own calculations.

## Appendix

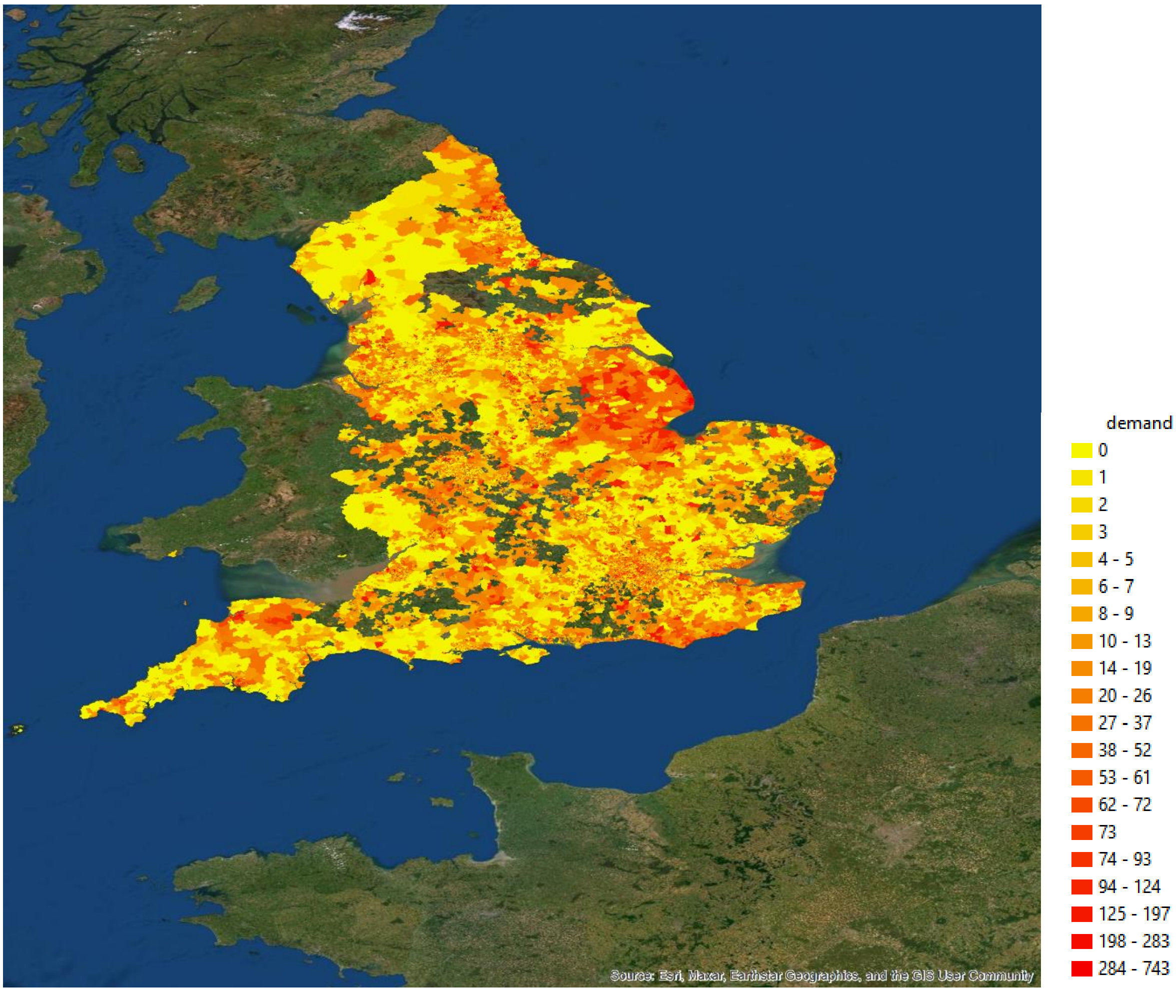
### Figures

Figure A1: Number of Volunteers at Lower Level Super Output Area (LSOA)



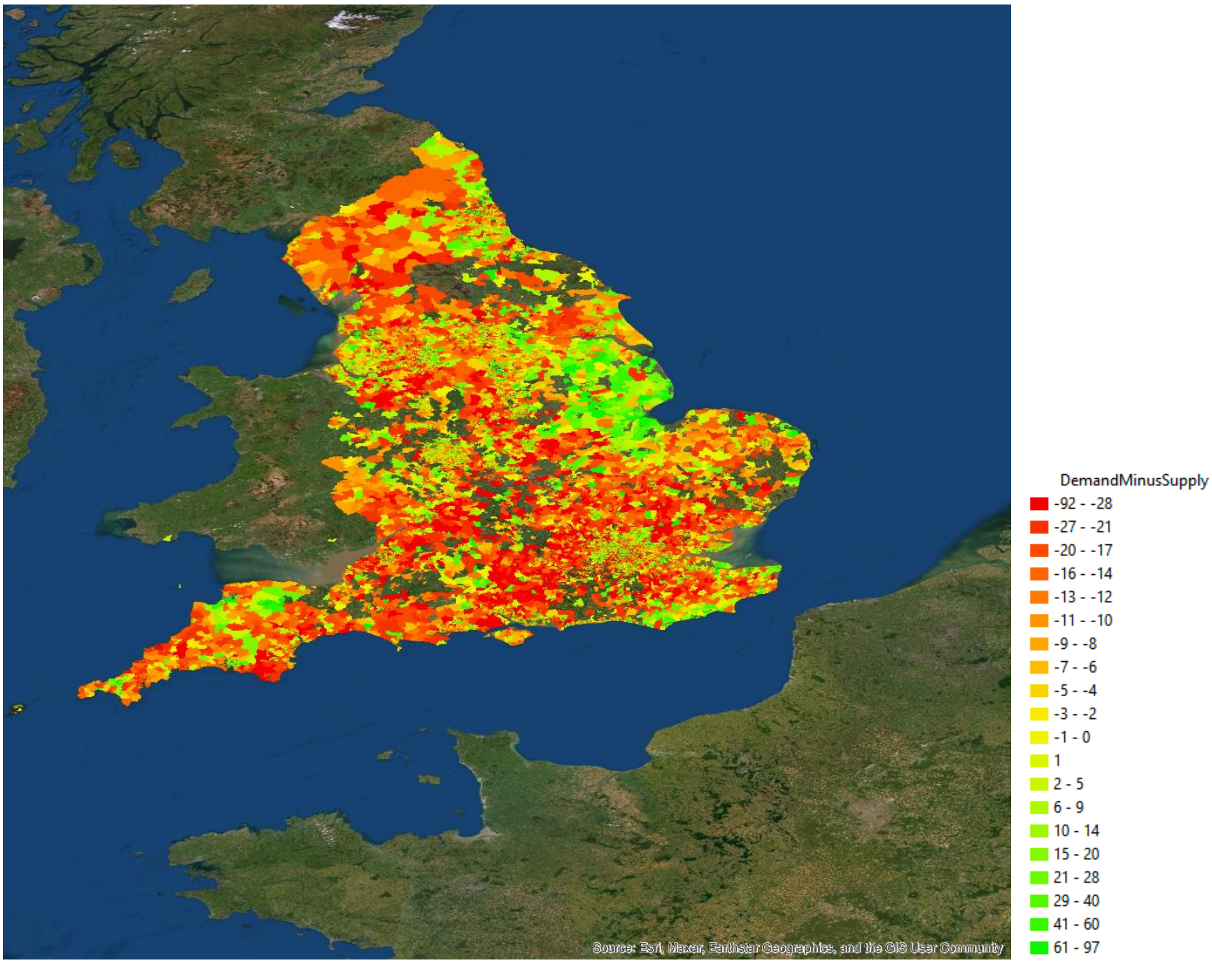

*Source:* NHSVR Administrative Data, July 2020; own calculations.

Figure A2: Number of Tasks at Lower Level Super Output Area (LSOA)



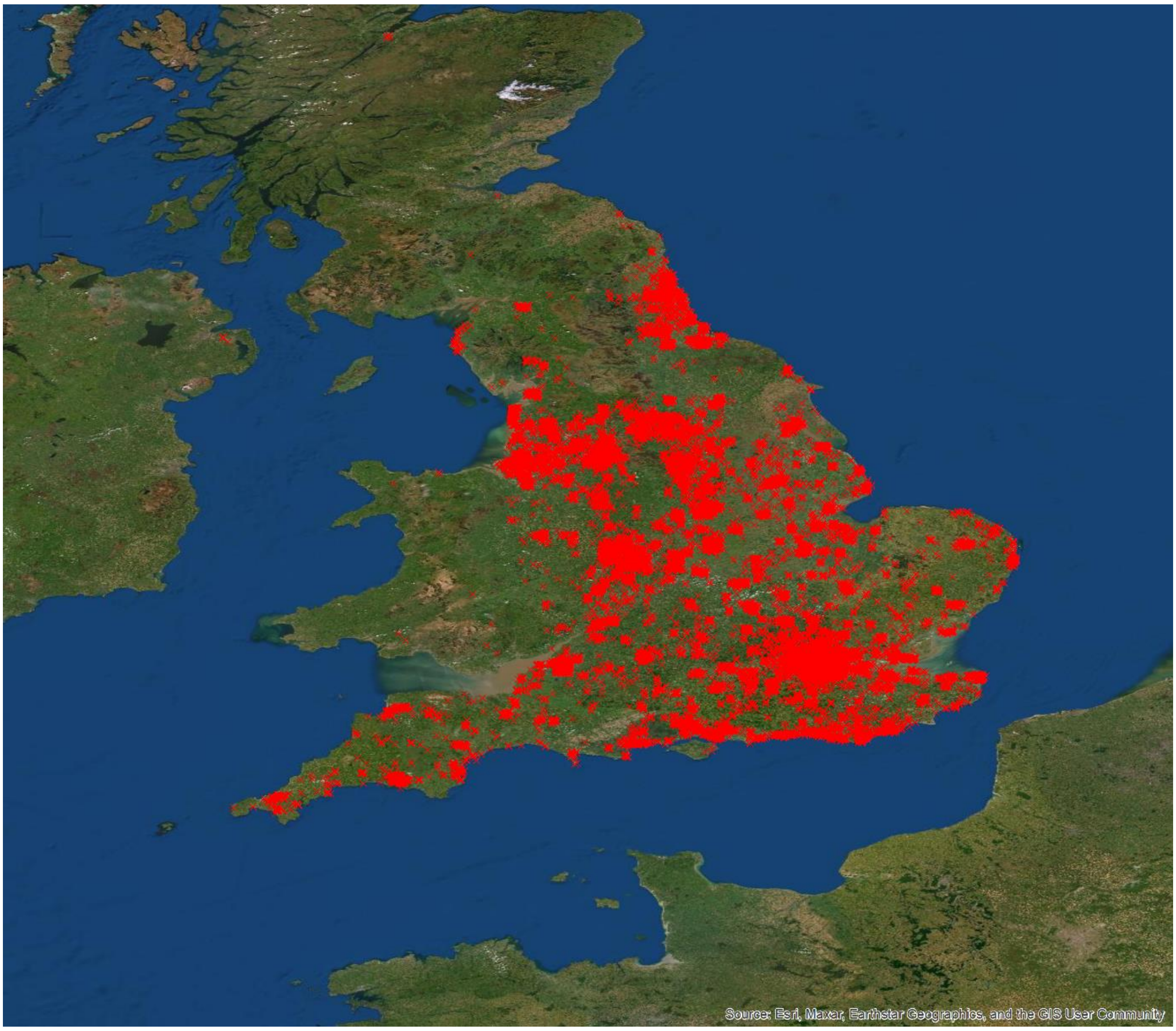

*Source:* NHSVR Administrative Data, July 2020; own calculations.

Figure A3: Number of Tasks Less Number of Volunteers at Lower Level Super Output Area (LSOA)



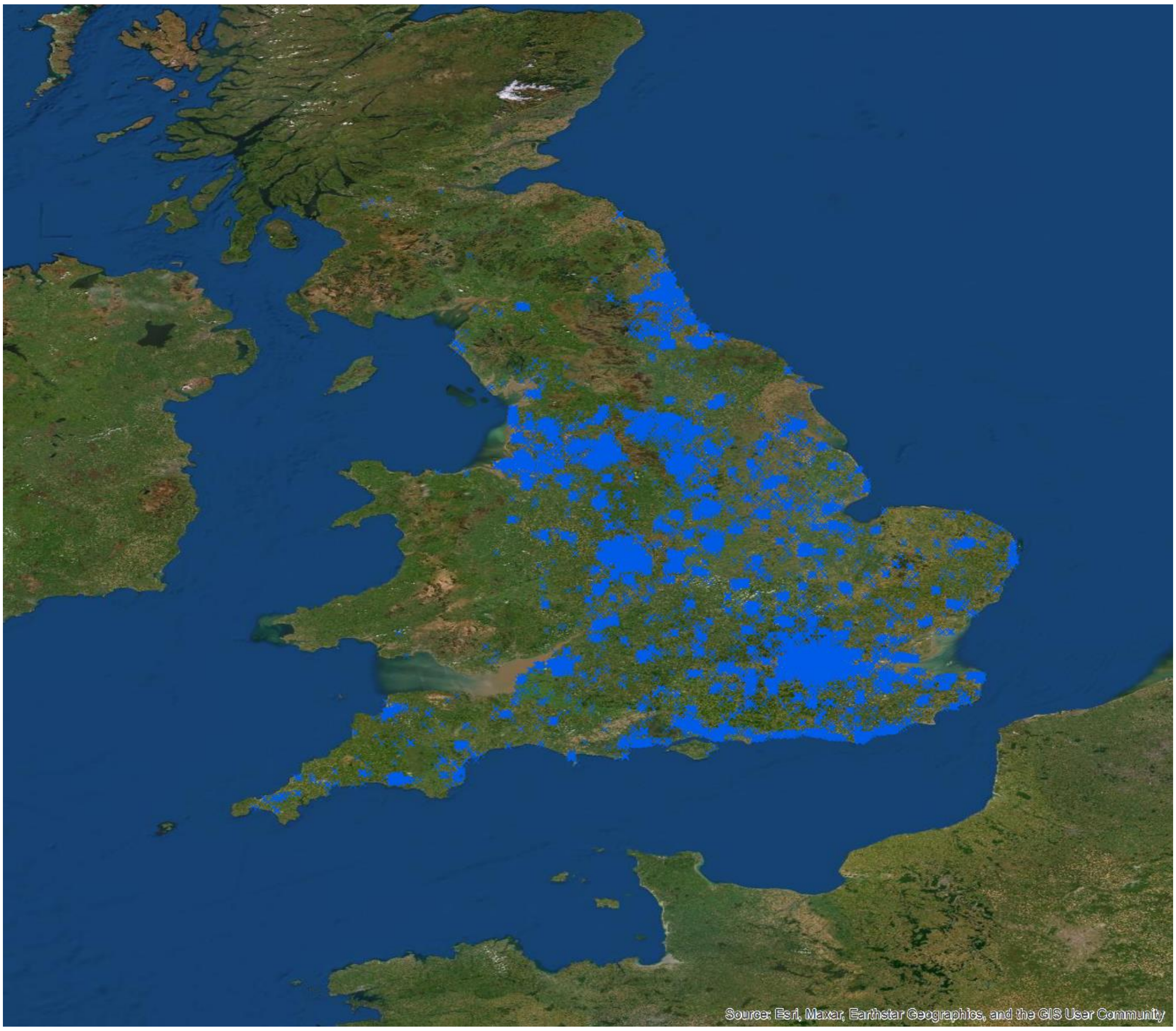

*Source:* NHSVR Administrative Data, July 2020; own calculations.

Figure A4: Distribution of Tasks in Main Estimation Sample



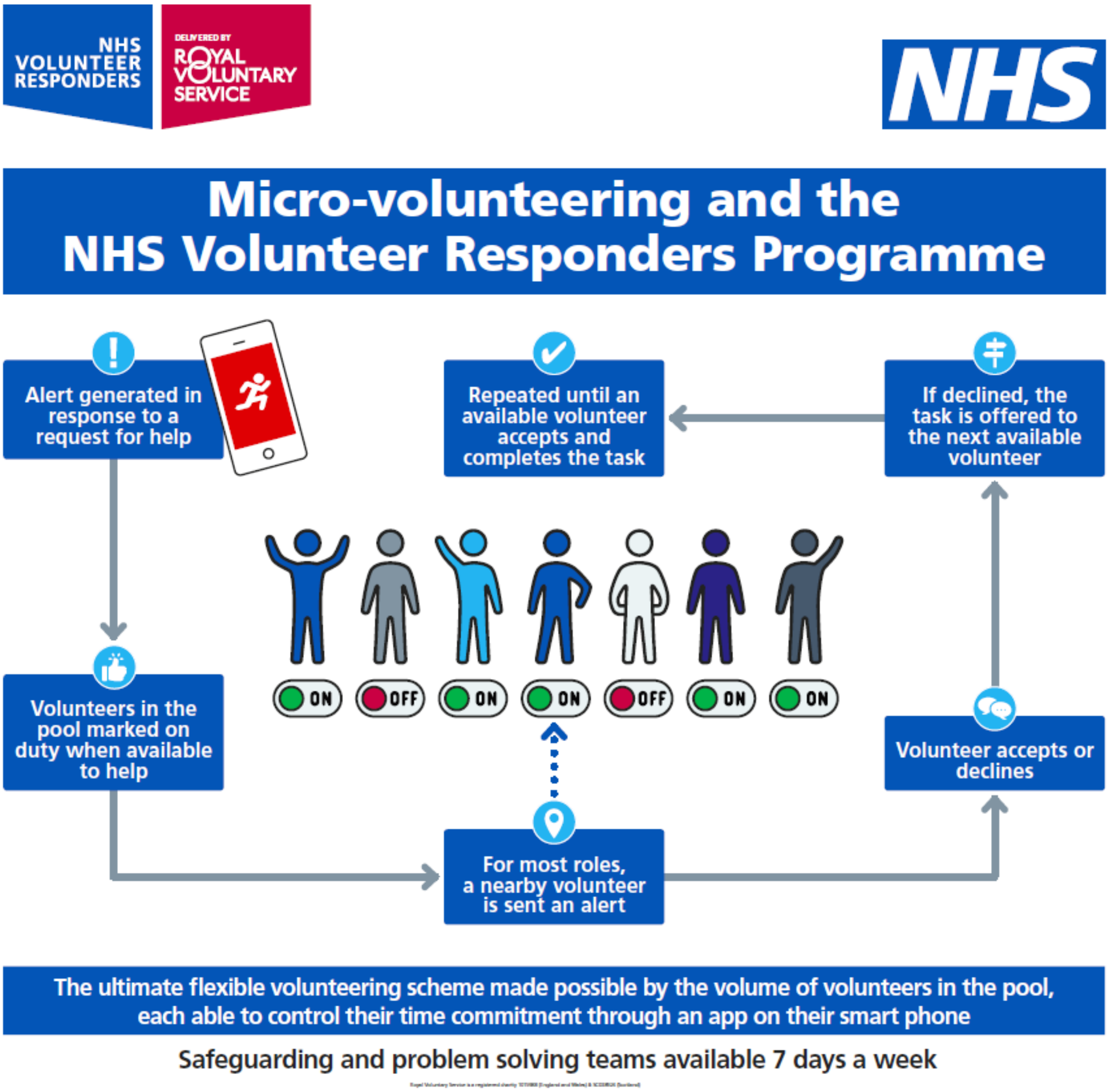

*Source:* NHSVR Administrative Data, July 2020; own calculations.

Figure A5: Distribution of Tasks in 1% Random Sample in Administrative Records



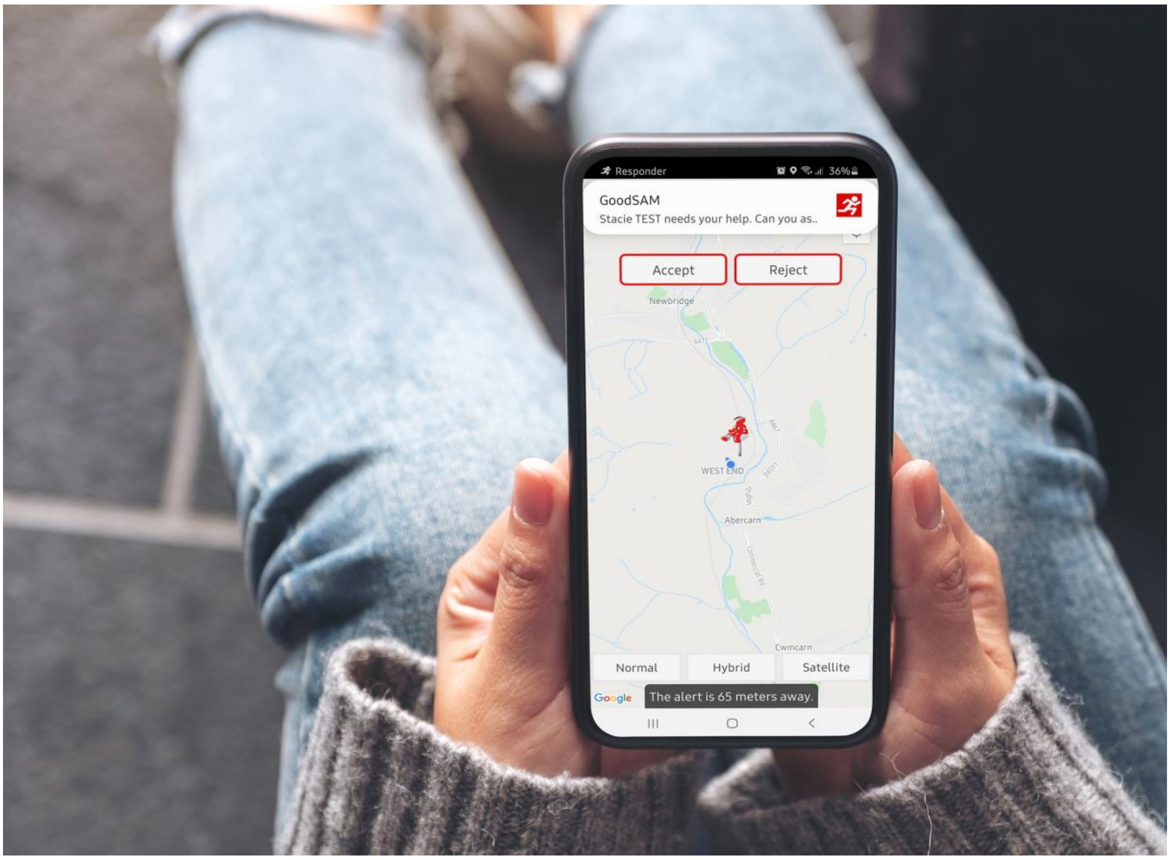

*Source:* NHSVR Administrative Data, July 2020; own calculations.

Figure A6: Allocation of Tasks to Volunteers Via Smartphone App



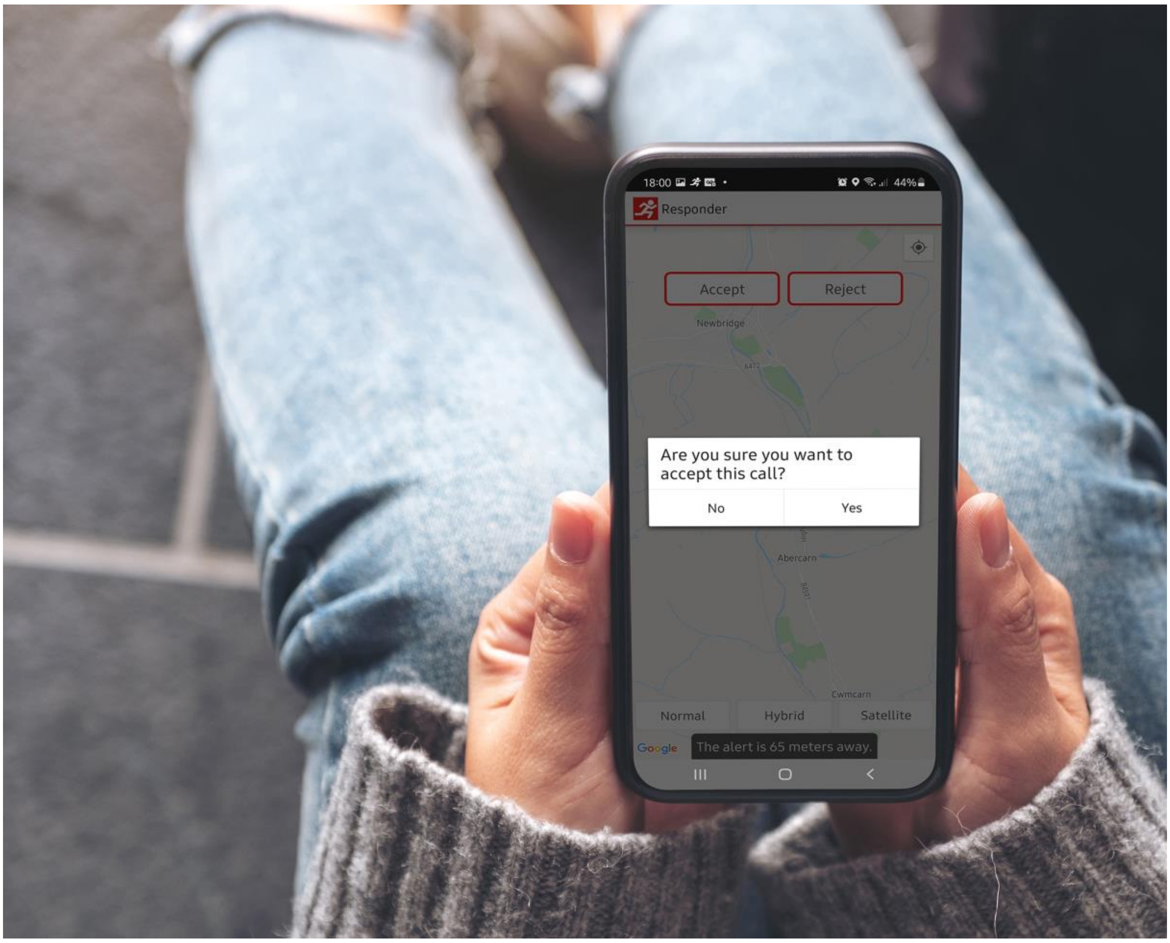

*Source:* NHSVR programme.

Figure A7: Smartphone App – Volunteer Receiving Nearby Task



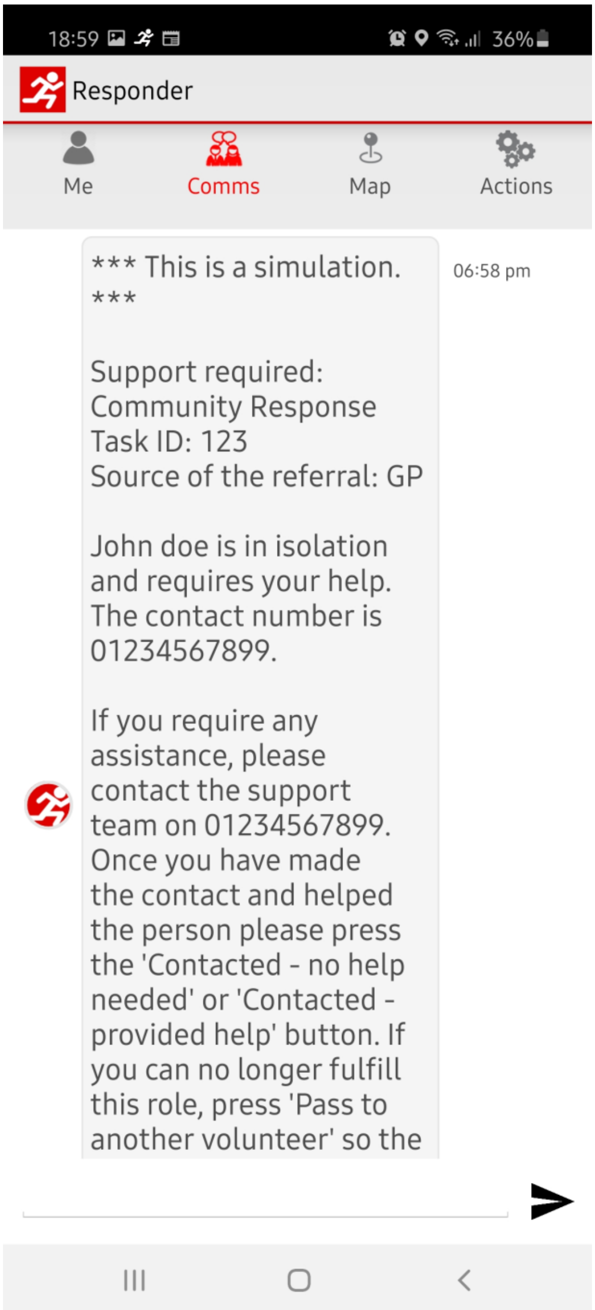

*Source:* NHSVR programme.

Figure A8: Smartphone App – Volunteer Accepting or Rejecting a Task




*Source:* NHSVR programme.

Figure A9: Smartphone App – Connecting Volunteer and Person in Need




*Source:* NHSVR programme.

## Tables

Table A1: Additionally Controlling for Waiting Time

| | Life Satisfaction | | Sense of Purpose in Life | | Belongingness | | Connectedness | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Treatment$_i$ | 0.2206** | 0.2311*** | 0.2653*** | 0.2230** | 0.0266 | 0.0291 | 0.0773*** | 0.0737*** |
| *(Volunteered Vs. Not Given Task)* | (0.0905) | (0.0883) | (0.0884) | (0.0871) | (0.0197) | (0.0197) | (0.0217) | (0.0217) |
| Individual Controls | No | Yes | No | Yes | No | Yes | No | Yes |
| Regional Covid-19 Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Postcode Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Date Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Scaling | 0-10 | 0-10 | 0-10 | 0-10 | 0-1 | 0-1 | 0-1 | 0-1 |
| Mean | 7.3 | 7.3 | 7.6 | 7.6 | 0.7 | 0.7 | 0.5 | 0.5 |
| σ | 2.1 | 2.1 | 2.0 | 2.0 | 0.5 | 0.5 | 0.5 | 0.5 |
| Number of Observations | 4,358 | 4,358 | 4,358 | 4,358 | 4,358 | 4,358 | 4,358 | 4,358 |
| Number of Treated | 3,214 | 3,214 | 3,214 | 3,214 | 3,214 | 3,214 | 3,214 | 3,214 |
| Number of Controlled | 1,144 | 1,144 | 1,144 | 1,144 | 1,144 | 1,144 | 1,144 | 1,144 |
| R Squared | 0.0415 | 0.1276 | 0.0426 | 0.1132 | 0.0487 | 0.0875 | 0.0596 | 0.0910 |

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

*Notes:* Treatment$_i$ takes on one if a volunteer has been allocated, has accepted, and has completed at least one task, and zero if a volunteer has not been allocated a task yet, at the time of the survey. The zero-category excludes individuals who have rejected a task. It is constructed using the archive of all tasks and their timestamps in administrative records. See Section 3.1 for a description of our data and Table 1 for summary statistics.
*Source:* NHSVR Survey Data, Administrative Data, July 2020; own calculations.

Table A2: Alternative Geographical Controls

| | Life Satisfaction | | Sense of Purpose in Life | | Belongingness | | Connectedness | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Postcode Area and District Fixed Effects (1,630)* | | | | | | | | |
| Treatment$_i$ | 0.2734*** | 0.2762*** | 0.2864*** | 0.2388** | 0.0350 | 0.0304 | 0.0744*** | 0.0708*** |
| *(Volunteered Vs. Not Given Task)* | (0.1032) | (0.0997) | (0.0994) | (0.0989) | (0.0226) | (0.0229) | (0.0244) | (0.0249) |
| Number of Observations | 4,358 | 4,358 | 4,358 | 4,358 | 4,358 | 4,358 | 4,358 | 4,358 |
| Number of Treated | 3,214 | 3,214 | 3,214 | 3,214 | 3,214 | 3,214 | 3,214 | 3,214 |
| Number of Controlled | 1,144 | 1,144 | 1,144 | 1,144 | 1,144 | 1,144 | 1,144 | 1,144 |
| R Squared | 0.3739 | 0.4340 | 0.3734 | 0.4281 | 0.3803 | 0.4099 | 0.3806 | 0.4041 |
| *Local-Authority District (LAD) Fixed Effects (309)* | | | | | | | | |
| Treatment$_i$ | 0.1936** | 0.2055*** | 0.2497*** | 0.2001*** | 0.0316* | 0.0317* | 0.0639*** | 0.0570*** |
| *(Volunteered Vs. Not Given Task)* | (0.0797) | (0.0778) | (0.0775) | (0.0769) | (0.0173) | (0.0176) | (0.0189) | (0.0192) |
| Number of Observations | 4,033 | 4,033 | 4,033 | 4,033 | 4,033 | 4,033 | 4,033 | 4,033 |
| Number of Treated | 2,968 | 2,968 | 2,968 | 2,968 | 2,968 | 2,968 | 2,968 | 2,968 |
| Number of Controlled | 1,065 | 1,065 | 1,065 | 1,065 | 1,065 | 1,065 | 1,065 | 1,065 |
| R Squared | 0.0886 | 0.1671 | 0.0920 | 0.1645 | 0.1010 | 0.1389 | 0.1070 | 0.1374 |
| Individual Controls | No | Yes | No | Yes | No | Yes | No | Yes |
| Regional Covid-19 Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Date Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

*Notes:* Treatment$_i$ takes on one if a volunteer has been allocated, has accepted, and has completed at least one task, and zero if a volunteer has not been allocated a task yet, at the time of the survey. The zero-category excludes individuals who have rejected a task. It is constructed using the archive of all tasks and their timestamps in administrative records. See Section 3.1 for a description of our data and Table 1 for summary statistics.
*Source:* NHSVR Survey Data, Administrative Data, July 2020; own calculations.

Table A3: Testing Predictive Power of Local-Area Characteristics for Number of Tasks at Local-Authority District (LAD) Level

| | Number of Tasks | | Life Satisfaction | Sense of Purpose | Belongingness | Connectedness |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treatment$_i$ | | | 0.2055*** | 0.2001*** | 0.0317* | 0.0570*** |
| *(Volunteered Vs. Not Given Task)* | | | (0.0778) | (0.0769) | (0.0176) | (0.0192) |
| Income Score | 2.0304*** | -0.0013* | | | | |
| | (0.1490) | (0.0007) | | | | |
| Employment Score | 0.1788 | 0.0019* | | | | |
| | (0.1653) | (0.0010) | | | | |
| Education Score | -0.3195*** | -0.0006 | | | | |
| | (0.0828) | (0.0005) | | | | |
| Health Score | -0.9663*** | -0.0000 | | | | |
| | (0.0938) | (0.0006) | | | | |
| Crime Score | -0.8814*** | -0.0006 | | | | |
| | (0.0596) | (0.0004) | | | | |
| Housing Score | -0.6765*** | -0.0002 | | | | |
| | (0.0661) | (0.0005) | | | | |
| Living Environment Score | 0.6670*** | -0.0010** | | | | |
| | (0.0593) | (0.0004) | | | | |
| Income Rank | 0.0011* | -0.0000 | | | | |
| | (0.0005) | (0.0000) | | | | |
| Income Decile | -5.5061*** | -0.0048 | | | | |
| | (1.4250) | (0.0135) | | | | |
| Employment Rank | 0.0049*** | -0.0000 | | | | |
| | (0.0005) | (0.0000) | | | | |
| Employment Decile | -8.9613*** | 0.0231 | | | | |
| | (1.4328) | (0.0141) | | | | |
| Education Rank | 0.0052*** | 0.0000 | | | | |
| | (0.0005) | (0.0000) | | | | |
| Education Decile | -5.2946*** | -0.0013 | | | | |
| | (1.4241) | (0.0139) | | | | |

| | | | | | | |
|---|---|---|---|---|---|---|
| Health Rank | -0.0034*** | -0.0000*** | | | | |
| | (0.0005) | (0.0000) | | | | |
| Health Decile | -4.9303*** | 0.0021 | | | | |
| | (1.4508) | (0.0139) | | | | |
| Crime Rank | -0.0041*** | -0.0000 | | | | |
| | (0.0005) | (0.0000) | | | | |
| Crime Decile | 0.7731 | 0.0049 | | | | |
| | (1.4365) | (0.0140) | | | | |
| Housing Rank | -0.0026*** | 0.0000 | | | | |
| | (0.0005) | (0.0000) | | | | |
| Housing Decile | 0.7372 | -0.0189 | | | | |
| | (1.4327) | (0.0132) | | | | |
| Living Environment Rank | 0.0010** | -0.0000 | | | | |
| | (0.0005) | (0.0000) | | | | |
| Living Environment Decile | -3.8272*** | -0.0008 | | | | |
| | (1.4436) | (0.0144) | | | | |
| IMD Rank | 0.0017*** | 0.0000* | | | | |
| | (0.0007) | (0.0000) | | | | |
| IMD Decile | -0.8151 | -0.0035 | | | | |
| | (1.4331) | (0.0141) | | | | |
| | | | | | | |
| Individual Controls | No | No | Yes | Yes | Yes | Yes |
| Regional Covid-19 Controls | No | No | Yes | Yes | Yes | Yes |
| Local Authority Fixed Effects | No | Yes | Yes | Yes | Yes | Yes |
| Date Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| | | | | | | |
| Number of Observations | 65,688 | 65,688 | 4,033 | 4,033 | 4,033 | 4,033 |
| Number of Treated | - | - | 2,968 | 2,968 | 2,968 | 2,968 |
| Number of Controlled | - | - | 1,065 | 1,065 | 1,065 | 1,065 |
| R Squared | 0.1658 | 0.9999 | 0.1671 | 0.1645 | 0.1389 | 0.1374 |

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

*Source:* NHSVR Survey Data, Administrative Data, July 2020; ONS Indices of Multiple Deprivation, 2015, 2019; own calculations.

Table A4: Understanding Society Covid-19 Wave (USC19) vs. Main Estimation Sample

| | USC19 | | | Main Estimation Sample | | | Normalised Difference | | |
|---|---|---|---|---|---|---|---|---|---|
| | All Mean | Volunteers Mean | Non-Volunteers Mean | All Mean | Treatment Mean | Control Mean | All - All | Volunteers - Treatment | Non-Volunteers - Control |
| | (1) | (2) | (3) | (4) | (5) | (6) | (1) - (4) | (2) - (5) | |
| Age: 16 to 24 | - | - | - | 0.0092 | 0.0096 | 0.0079 | - | - | - |
| 25 to 34 | 0.0644 | 0.0443 | 0.0664 | 0.0415 | 0.0420 | 0.0402 | 0.0723 | 0.0080 | 0.0825 |
| 35 to 44 | 0.0934 | 0.0483 | 0.0985 | 0.1058 | 0.1095 | 0.0953 | -0.0292 | -0.1617 | 0.0076 |
| 45 to 54 | 0.1448 | 0.1116 | 0.1469 | 0.2327 | 0.2386 | 0.2159 | -0.1599 | -0.2398 | -0.1272 |
| 55 to 64 | 0.1962 | 0.1978 | 0.1952 | 0.3922 | 0.3948 | 0.3846 | -0.3114 | -0.3124 | -0.3018 |
| 65 to 74 | 0.2191 | 0.2524 | 0.2165 | 0.2035 | 0.1910 | 0.2386 | 0.0269 | 0.1047 | -0.0374 |
| 75 to 84 | 0.1957 | 0.2579 | 0.1899 | 0.0126 | 0.0118 | 0.0149 | 0.4442 | 0.5459 | 0.4264 |
| Prefer Not to Say | 0.0097 | 0.0047 | 0.0101 | 0.0025 | 0.0025 | 0.0026 | 0.0654 | 0.0266 | 0.0664 |
| Gender: Male | 0.4142 | 0.4283 | 0.4130 | 0.3394 | 0.3124 | 0.4152 | 0.1095 | 0.1710 | -0.0032 |
| Female | 0.5858 | 0.5717 | 0.5870 | 0.6574 | 0.6851 | 0.5795 | -0.1047 | -0.1672 | 0.0107 |
| Other | - | - | - | 0.0007 | 0.0006 | 0.0009 | - | - | - |
| Prefer Not to Say | - | - | - | 0.0025 | 0.0019 | 0.0044 | - | - | - |
| Ethnicity: Asian / Asian British - Bangladeshi | 0.0076 | 0.0089 | 0.0094 | 0.0032 | 0.0025 | 0.0052 | 0.0427 | 0.0602 | 0.0346 |
| Asian / Asian British - Chinese | 0.0046 | 0.0057 | 0.0054 | 0.0039 | 0.0050 | 0.0009 | 0.0073 | 0.0066 | 0.0569 |
| Asian / Asian British - Indian | 0.0283 | 0.0283 | 0.0338 | 0.0250 | 0.0268 | 0.0201 | 0.0146 | 0.0065 | 0.0599 |
| Asian / Asian British - Pakistani | 0.0169 | 0.0081 | 0.0210 | 0.0078 | 0.0075 | 0.0087 | 0.0585 | 0.0049 | 0.0716 |
| Asian / Asian British - Other | 0.0067 | 0.0081 | 0.0073 | 0.0050 | 0.0047 | 0.0061 | 0.0157 | 0.0303 | 0.0100 |
| Black / African / Carib. / Black British - African | 0.0097 | 0.0113 | 0.0108 | 0.0106 | 0.0106 | 0.0105 | -0.0057 | 0.0050 | 0.0022 |
| Black / African / Carib. / Black British - Carib. | 0.0106 | 0.0065 | 0.0134 | 0.0032 | 0.0034 | 0.0026 | 0.0629 | 0.0306 | 0.0855 |
| Black / African / Carib. / Black British - Other | 0.0011 | 0.0008 | 0.0012 | 0.0018 | 0.0019 | 0.0017 | -0.0131 | -0.0205 | -0.0106 |

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Mixed / Multiple - White and Asian | 0.0046 | 0.0073 | 0.0048 | 0.0044 | 0.0050 | 0.0026 | 0.0030 | 0.0208 | 0.0255 |
| Mixed / Multiple - White and Black African | - | - | - | 0.0009 | 0.0009 | 0.0009 | - | - | - |
| Mixed / Multiple - White and Black Caribb. | 0.0062 | 0.0032 | 0.0079 | 0.0011 | 0.0016 | 0.0000 | 0.0593 | 0.0242 | 0.0893 |
| Mixed / Multiple - Other | 0.0034 | 0.0048 | 0.0040 | 0.0034 | 0.0031 | 0.0044 | 0.0001 | 0.0195 | -0.0045 |
| White - British / Engl. / Irish / Scottish / Welsh | 0.8503 | 0.8522 | 0.8371 | 0.8336 | 0.8273 | 0.8514 | 0.0323 | 0.0479 | -0.0279 |
| White - Gypsy or Irish Traveller | - | - | - | 0.0005 | 0.0003 | 0.0009 | - | - | - |
| White - Irish | 0.0150 | 0.0121 | 0.0068 | 0.0149 | 0.0149 | 0.0149 | 0.0004 | -0.0172 | -0.0547 |
| White - Other | 0.0298 | 0.0363 | 0.0318 | 0.0734 | 0.0775 | 0.0621 | -0.1400 | -0.1260 | -0.1015 |
| Other Ethnic Group - Arab | 0.0016 | 0.0016 | 0.0020 | 0.0023 | 0.0025 | 0.0017 | -0.0103 | -0.0136 | 0.0046 |
| Other Ethnic Group - Other | 0.0032 | 0.0048 | 0.0033 | 0.0048 | 0.0047 | 0.0052 | -0.0178 | 0.0018 | -0.0209 |
| Religion: None | - | - | - | 0.3224 | 0.3108 | 0.3549 | - | - | - |
| Buddhist | - | - | - | 0.0106 | 0.0106 | 0.0105 | - | - | - |
| Christian | - | - | - | 0.5826 | 0.5930 | 0.5533 | - | - | - |
| Hindu | - | - | - | 0.0119 | 0.0143 | 0.0052 | - | - | - |
| Jewish | - | - | - | 0.0101 | 0.0128 | 0.0026 | - | - | - |
| Muslim | - | - | - | 0.0190 | 0.0162 | 0.0271 | - | - | - |
| Sikh | - | - | - | 0.0067 | 0.0072 | 0.0052 | - | - | - |
| Other | - | - | - | 0.0188 | 0.0184 | 0.0201 | - | - | - |
| Prefer Not to Say | - | - | - | 0.0179 | 0.0168 | 0.0210 | - | - | - |
| Physical or Mental Health Condition: No | 0.4882 | 0.4612 | 0.4891 | 0.7391 | 0.7321 | 0.7587 | -0.3771 | -0.4061 | -0.4097 |
| Yes | 0.5118 | 0.5388 | 0.5109 | 0.2402 | 0.2470 | 0.2212 | 0.4130 | 0.4424 | 0.4459 |
| Don’t Know | - | - | - | 0.0094 | 0.0087 | 0.0114 | - | - | - |
| Prefer Not to Say | - | - | - | 0.0112 | 0.0121 | 0.0087 | - | - | - |
| Self-Isolating: No | 0.8869 | 0.8519 | 0.8799 | 0.9137 | 0.9088 | 0.9274 | -0.0633 | -0.1232 | -0.1143 |
| Yes | 0.1105 | 0.1481 | 0.1201 | 0.0808 | 0.0856 | 0.0673 | 0.0716 | 0.1368 | 0.1285 |
| Don’t Know | 0.0026 | 0.0000 | 0.0000 | 0.0055 | 0.0056 | 0.0052 | -0.0327 | -0.0750 | -0.0726 |
| Employment: Full-Time Employed | 0.5666 | 0.5023 | 0.5747 | 0.2636 | 0.2647 | 0.2605 | 0.4568 | 0.3562 | 0.4751 |

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Part-Time Employed | - | - | - | 0.1486 | 0.1457 | 0.1565 | - | - | - |
| Furloughed | 0.1199 | 0.0946 | 0.1233 | 0.0900 | 0.0987 | 0.0656 | 0.0691 | -0.0099 | 0.1403 |
| In Education | - | - | - | 0.0099 | 0.0103 | 0.0087 | - | - | - |
| Unemployed | - | - | - | 0.0519 | 0.0517 | 0.0524 | - | - | - |
| Disabled | - | - | - | 0.0184 | 0.0206 | 0.0122 | - | - | - |
| Retired | - | - | - | 0.3031 | 0.2946 | 0.3269 | - | - | - |
| Looking After Family | - | - | - | 0.0556 | 0.0564 | 0.0533 | - | - | - |
| Doing Something Else | - | - | - | 0.0590 | 0.0573 | 0.0638 | - | - | - |
| Motivation: Altruistic (i.e. Helping in Crisis) | - | - | - | 0.9837 | 0.9832 | 0.9851 | - | - | - |
| Thought It Was Expected | - | - | - | 0.0750 | 0.0719 | 0.0839 | - | - | - |
| Like Telling Family, Friends | - | - | - | 0.0679 | 0.0781 | 0.0393 | - | - | - |
| Was Asked | - | - | - | 0.0140 | 0.0134 | 0.0157 | - | - | - |
| Like Helping People | - | - | - | 0.7212 | 0.7576 | 0.6189 | - | - | - |
| Wanted to Meet People | - | - | - | 0.0546 | 0.0548 | 0.0542 | - | - | - |
| Gain Skills | - | - | - | 0.1622 | 0.1724 | 0.1337 | - | - | - |
| Pursue Career | - | - | - | 0.0523 | 0.0557 | 0.0428 | - | - | - |
| Had Some Time | - | - | - | 0.1147 | 0.1185 | 0.1040 | - | - | - |
| Other | - | - | - | 0.0404 | 0.0436 | 0.0315 | - | - | - |
| Volunteered Before: No | - | - | - | 0.2139 | 0.2122 | 0.2185 | - | - | - |
| Yes | - | - | - | 0.7861 | 0.7878 | 0.7815 | - | - | - |
| Volunteering Elsewhere: No | - | - | - | 0.6574 | 0.6658 | 0.6337 | - | - | - |
| Yes, One Other Group | - | - | - | 0.1996 | 0.1923 | 0.2203 | - | - | - |
| Yes, More Than One Other Group | - | - | - | 0.1430 | 0.1419 | 0.1460 | - | - | - |
| If Elsewhere: Informal Local Group | - | - | - | 0.0473 | 0.0448 | 0.0542 | - | - | - |
| Organised Local Group | - | - | - | 0.1042 | 0.0983 | 0.1206 | - | - | - |
| National Charity | - | - | - | 0.0404 | 0.0411 | 0.0385 | - | - | - |
| Public Sector | - | - | - | 0.0312 | 0.0314 | 0.0306 | - | - | - |
| Sports Group | - | - | - | 0.0177 | 0.0168 | 0.0201 | - | - | - |

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Faith Group | - | - | - | 0.0438 | 0.0448 | 0.0411 | - | - | - | |
| Other | - | - | - | 0.0581 | 0.0569 | 0.0612 | - | - | - | |
| N | 10,892 | 1,264 | 9,628 | 4,358 | 3,214 | 1,144 | - | - | - | |

*Notes:* Normalised differences are calculated as $\Delta x = (\bar{x}_t - \bar{x}_c) / \sqrt{(\sigma_t^2 + \sigma_c^2)}$, where $\bar{x}_t$ and $\bar{x}_c$ is the sample mean of the covariate for the treatment and control group, respectively. $\sigma^2$ denotes the respective variance. As a suggested threshold value, a normalised difference greater than 0.25 suggests a non-balanced covariate (Imbens and Wooldridge, 2009; Imbens and Rubin, 2015).
*Sources:* NHSVR Survey Data, Administrative Data, July 2020; Understanding Society Covid-19 Wave, July 2020; own calculations.

Table A5: UCL Covid-19 Social Study (UCL19) vs. Main Estimation Sample

| | UCL19 | | | Main Estimation Sample | | | Normalised Difference | | |
|---|---|---|---|---|---|---|---|---|---|
| | All | Volun-teers | Non-Vol-unteers | All | Treat-ment | Control | | | |
| | Mean | Mean | Mean | Mean | Mean | Mean | All - All | Volunteers - Treatment | Non-Volunteers - Control |
| | (1) | (2) | (3) | (4) | (5) | (6) | (1) - (4) | (2) - (5) | (3) - (6) |
| Age: 16 to 24 | 0.0216 | 0.0180 | 0.0229 | 0.0092 | 0.0096 | 0.0079 | 0.0716 | 0.0505 | 0.0867 |
| 25 to 34 | 0.1030 | 0.0860 | 0.1090 | 0.0415 | 0.0420 | 0.0402 | 0.1689 | 0.1276 | 0.1868 |
| 35 to 44 | 0.1670 | 0.1472 | 0.1741 | 0.1058 | 0.1095 | 0.0953 | 0.1266 | 0.0798 | 0.1644 |
| 45 to 54 | 0.2193 | 0.2145 | 0.2210 | 0.2327 | 0.2386 | 0.2159 | -0.0226 | -0.0408 | 0.0087 |
| 55 to 64 | 0.2489 | 0.2660 | 0.2428 | 0.3922 | 0.3948 | 0.3846 | -0.2196 | -0.1955 | -0.2186 |
| 65 to 74 | 0.1928 | 0.2172 | 0.1840 | 0.2035 | 0.1910 | 0.2386 | -0.0190 | 0.0459 | -0.0948 |
| 75 to 84 | 0.0440 | 0.0479 | 0.0427 | 0.0126 | 0.0118 | 0.0149 | 0.1345 | 0.1506 | 0.1180 |
| Prefer Not to Say | - | - | - | 0.0025 | 0.0025 | 0.0026 | - | - | - |
| Gender: Male | 0.2398 | 0.2417 | 0.2391 | 0.3394 | 0.3124 | 0.4152 | -0.1562 | -0.1120 | -0.2702 |
| Female | 0.7563 | 0.7540 | 0.7572 | 0.6574 | 0.6851 | 0.5795 | 0.1546 | 0.1087 | 0.2716 |
| Other | - | - | - | 0.0007 | 0.0006 | 0.0009 | - | - | - |
| Prefer Not to Say | - | - | - | 0.0025 | 0.0019 | 0.0044 | - | - | - |
| Ethnicity: Asian / Asian British - Bangladeshi | - | - | - | 0.0032 | 0.0025 | 0.0052 | - | - | - |
| Asian / Asian British - Chinese | - | - | - | 0.0039 | 0.0050 | 0.0009 | - | - | - |
| Asian / Asian British - Indian | - | - | - | 0.0250 | 0.0268 | 0.0201 | - | - | - |
| Asian / Asian British - Pakistani | - | - | - | 0.0078 | 0.0075 | 0.0087 | - | - | - |
| Asian / Asian British - Other | - | - | - | 0.0050 | 0.0047 | 0.0061 | - | - | - |
| Black / African / Carib. / Black British - African | - | - | - | 0.0106 | 0.0106 | 0.0105 | - | - | - |
| Black / African / Carib. / Black British - Carib. | - | - | - | 0.0032 | 0.0034 | 0.0026 | - | - | - |
| Black / African / Carib. / Black British - Other | - | - | - | 0.0018 | 0.0019 | 0.0017 | - | - | - |

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Mixed / Multiple - White and Asian | - | - | - | 0.0044 | 0.0050 | 0.0026 | - | - | - |
| Mixed / Multiple - White and Black African | - | - | - | 0.0009 | 0.0009 | 0.0009 | - | - | - |
| Mixed / Multiple - White and Black Caribb. | - | - | - | 0.0011 | 0.0016 | 0.0000 | - | - | - |
| Mixed / Multiple - Other | - | - | - | 0.0034 | 0.0031 | 0.0044 | - | - | - |
| White - British / Engl. / Irish / Scottish / Welsh | - | - | - | 0.8336 | 0.8273 | 0.8514 | - | - | - |
| White - Gypsy or Irish Traveller | - | - | - | 0.0005 | 0.0003 | 0.0009 | - | - | - |
| White - Irish | - | - | - | 0.0149 | 0.0149 | 0.0149 | - | - | - |
| White - Other | - | - | - | 0.0734 | 0.0775 | 0.0621 | - | - | - |
| Other Ethnic Group - Arab | - | - | - | 0.0023 | 0.0025 | 0.0017 | - | - | - |
| Other Ethnic Group - Other | - | - | - | 0.0048 | 0.0047 | 0.0052 | - | - | - |
| Religion: None | 0.4772 | 0.4276 | 0.4824 | 0.3224 | 0.3108 | 0.3549 | 0.2262 | 0.1723 | 0.1842 |
| Buddhist | 0.0076 | 0.0073 | 0.0076 | 0.0106 | 0.0106 | 0.0105 | -0.0221 | -0.0249 | -0.0214 |
| Christian | 0.4522 | 0.4812 | 0.4491 | 0.5826 | 0.5930 | 0.5533 | -0.1861 | -0.1596 | -0.1481 |
| Hindu | 0.0018 | 0.0023 | 0.0017 | 0.0119 | 0.0143 | 0.0052 | -0.0873 | -0.0941 | -0.0424 |
| Jewish | 0.0124 | 0.0213 | 0.0115 | 0.0101 | 0.0128 | 0.0026 | 0.0155 | 0.0469 | 0.0749 |
| Muslim | 0.0043 | 0.0041 | 0.0043 | 0.0190 | 0.0162 | 0.0271 | -0.0976 | -0.0855 | -0.1303 |
| Sikh | 0.0009 | 0.0014 | 0.0009 | 0.0067 | 0.0072 | 0.0052 | -0.0656 | -0.0630 | -0.0554 |
| Other | 0.0310 | 0.0404 | 0.0301 | 0.0188 | 0.0184 | 0.0201 | 0.0555 | 0.0925 | 0.0450 |
| Prefer Not to Say | 0.0126 | 0.0145 | 0.0124 | 0.0179 | 0.0168 | 0.0210 | -0.0304 | -0.0130 | -0.0472 |
| Physical or Mental Health Condition: No | - | - | - | 0.7391 | 0.7321 | 0.7587 | - | - | - |
| Yes | - | - | - | 0.2402 | 0.2470 | 0.2212 | - | - | - |
| Don’t Know | - | - | - | 0.0094 | 0.0087 | 0.0114 | - | - | - |
| Prefer Not to Say | - | - | - | 0.0112 | 0.0121 | 0.0087 | - | - | - |
| Self-Isolating: No | - | - | - | 0.9137 | 0.9088 | 0.9274 | - | - | - |
| Yes | - | - | - | 0.0808 | 0.0856 | 0.0673 | - | - | - |
| Don’t Know | - | - | - | 0.0055 | 0.0056 | 0.0052 | - | - | - |
| Employment: Full-Time Employed | 0.3991 | 0.3572 | 0.4140 | 0.2636 | 0.2647 | 0.2605 | 0.2057 | 0.1420 | 0.2326 |

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Part-Time Employed | 0.1719 | 0.1745 | 0.1710 | 0.1486 | 0.1457 | 0.1565 | 0.0450 | 0.0554 | 0.0277 |
| Furloughed | - | - | - | 0.0900 | 0.0987 | 0.0656 | - | - | - |
| In Education | 0.0299 | 0.0272 | 0.0309 | 0.0099 | 0.0103 | 0.0087 | 0.1017 | 0.0883 | 0.1127 |
| Unemployed | 0.0224 | 0.0231 | 0.0222 | 0.0519 | 0.0517 | 0.0524 | -0.1106 | -0.1070 | -0.1134 |
| Disabled | 0.0503 | 0.0479 | 0.0512 | 0.0184 | 0.0206 | 0.0122 | 0.1245 | 0.1066 | 0.1581 |
| Retired | 0.2873 | 0.3297 | 0.2723 | 0.3031 | 0.2946 | 0.3269 | -0.0244 | 0.0536 | -0.0844 |
| Looking After Family | 0.0390 | 0.0405 | 0.0385 | 0.0556 | 0.0564 | 0.0533 | -0.0551 | -0.0523 | -0.0500 |
| Doing Something Else | - | - | - | 0.0590 | 0.0573 | 0.0638 | - | - | - |
| Motivation: Altruistic (i.e. Helping in Crisis) | - | - | - | 0.9837 | 0.9832 | 0.9851 | - | - | - |
| Thought It Was Expected | - | - | - | 0.0750 | 0.0719 | 0.0839 | - | - | - |
| Like Telling Family, Friends | - | - | - | 0.0679 | 0.0781 | 0.0393 | - | - | - |
| Was Asked | - | - | - | 0.0140 | 0.0134 | 0.0157 | - | - | - |
| Like Helping People | - | - | - | 0.7212 | 0.7576 | 0.6189 | - | - | - |
| Wanted to Meet People | - | - | - | 0.0546 | 0.0548 | 0.0542 | - | - | - |
| Gain Skills | - | - | - | 0.1622 | 0.1724 | 0.1337 | - | - | - |
| Pursue Career | - | - | - | 0.0523 | 0.0557 | 0.0428 | - | - | - |
| Had Some Time | - | - | - | 0.1147 | 0.1185 | 0.1040 | - | - | - |
| Other | - | - | - | 0.0404 | 0.0436 | 0.0315 | - | - | - |
| Volunteered Before: No | - | - | - | 0.2139 | 0.2122 | 0.2185 | - | - | - |
| Yes | - | - | - | 0.7861 | 0.7878 | 0.7815 | - | - | - |
| Volunteering Elsewhere: No | - | - | - | 0.6574 | 0.6658 | 0.6337 | - | - | - |
| Yes, One Other Group | - | - | - | 0.1996 | 0.1923 | 0.2203 | - | - | - |
| Yes, More Than One Other Group | - | - | - | 0.1430 | 0.1419 | 0.1460 | - | - | - |
| If Elsewhere: Informal Local Group | - | - | - | 0.0473 | 0.0448 | 0.0542 | - | - | - |
| Organised Local Group | - | - | - | 0.1042 | 0.0983 | 0.1206 | - | - | - |
| National Charity | - | - | - | 0.0404 | 0.0411 | 0.0385 | - | - | - |
| Public Sector | - | - | - | 0.0312 | 0.0314 | 0.0306 | - | - | - |
| Sports Group | - | - | - | 0.0177 | 0.0168 | 0.0201 | - | - | - |

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Faith Group | - | - | - | 0.0438 | 0.0448 | 0.0411 | - | - | - |
| Other | - | - | - | 0.0581 | 0.0569 | 0.0612 | - | - | - |
| N | 667612 | 176576 | 491036 | 4,358 | 3,214 | 1,144 | - | - | - |

*Notes:* Normalised differences are calculated as $\Delta x = (\bar{x}_t - \bar{x}_c) / \sqrt{(\sigma_t^2 + \sigma_c^2)}$, where $\bar{x}_t$ and $\bar{x}_c$ is the sample mean of the covariate for the treatment and control group, respectively. $\sigma^2$ denotes the respective variance. As a suggested threshold value, a normalised difference greater than 0.25 suggests a non-balanced covariate (Imbens and Wooldridge, 2009; Imbens and Rubin, 2015).
*Sources:* NHSVR Survey Data, Administrative Data, July 2020; UCL Covid-19 Social Study, July 2020; own calculations.

Table A6: Life Satisfaction of Volunteers and Non-Volunteers in External Datasets

| Dataset | | All | Volunteers | Non-Volunteers | Mean Difference Volunteers - Non-Volunteers |
|---|---|---|---|---|---|
| *USC19* | | | | | |
| | Mean | 7.8959 | 8.0995 | 7.8696 | 0.2299*** |
| | σ | 2.4311 | 2.4074 | 2.4329 | |
| | N | 10,892 | 1,264 | 9,628 | |
| *UCL19* | | | | | |
| | Mean | 6.0679 | 6.2272 | 6.0116 | 0.2156*** |
| | σ | 2.2937 | 2.2786 | 2.2964 | |
| | N | 664,597 | 173,580 | 491,017 | |

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

*Notes:* USC19: Understanding Society Covid-19 Wave. UCL19: UCL Covid-19 Social Study. The samples are raw samples. In the UCL19 sample, the same question on life satisfaction is asked as in our data, i.e. “Overall, how satisfied are you with your life nowadays?”, with answers from 0 (“not at all”) to 10 (“completely”). In the USC19 sample, a slightly different question is asked, i.e. “Here are some questions about how you feel about your life. Please choose the number which you feel best describes how dissatisfied or satisfied you are with the following aspects of your current situation. Your life overall.”, with answers from 1 (“Completely dissatisfied”) to 7 (“Completely satisfied”). For comparability, the latter question has been rescaled to a 0-to-10 scale.
*Sources:* Understanding Society Covid-19 Wave, July 2020; UCL Covid-19 Social Study, July 2020; own calculations.

Table A7: Extended Estimation Sample vs. Main Estimation Sample

| | Extended<br>Mean (SD) | Main<br>Mean (SD) | Diff.<br>Extended - Main | P-Value | Normalised Diff.<br>Extended - Main |
|---|---|---|---|---|---|
| *Individual Controls* | | | | | |
| Age: 16 to 24 | 0.0094 | 0.0092 | 0.0002 | 0.9059 | 0.0015 |
| 25 to 34 | 0.0392 | 0.0415 | -0.0023 | 0.5153 | 0.0084 |
| 35 to 44 | 0.0886 | 0.1058 | -0.0171 | 0.0014 | 0.0409 |
| 45 to 54 | 0.2251 | 0.2327 | -0.0076 | 0.0014 | 0.0128 |
| 55 to 64 | 0.4083 | 0.3922 | 0.0161 | 0.0745 | 0.0232 |
| 65 to 74 | 0.2107 | 0.2035 | 0.0071 | 0.3393 | 0.0125 |
| 75 to 84 | 0.0140 | 0.0126 | 0.0014 | 0.5250 | 0.0083 |
| Prefer Not to Say | 0.0004 | 0.0000 | 0.0004 | 0.1677 | 0.0209 |
| | | | | | |
| Gender: Male | 0.3604 | 0.3394 | 0.0211 | 0.0166 | 0.0312 |
| Female | 0.6336 | 0.6574 | -0.0239 | 0.0069 | 0.0353 |
| Other | 0.0007 | 0.0007 | -0.0000 | 0.9438 | 0.0009 |
| Prefer Not to Say | 0.0053 | 0.0025 | 0.0028 | 0.0209 | 0.0319 |
| | | | | | |
| Ethnicity: Asian / Asian British - Bangladeshi | 0.0017 | 0.0032 | -0.0015 | 0.0905 | 0.0208 |
| Asian / Asian British - Chinese | 0.0031 | 0.0039 | -0.0008 | 0.4257 | 0.0101 |
| Asian / Asian British - Indian | 0.0167 | 0.0250 | -0.0083 | 0.0011 | 0.0411 |
| Asian / Asian British - Pakistani | 0.0046 | 0.0078 | -0.0032 | 0.0194 | 0.0290 |
| Asian / Asian British - Other | 0.0035 | 0.0050 | -0.0016 | 0.1803 | 0.0169 |
| Black / African / Carib. / Black British - African | 0.0067 | 0.0106 | -0.0039 | 0.0169 | 0.0298 |
| Black / African / Carib. / Black British - Carib. | 0.0031 | 0.0032 | -0.0002 | 0.8789 | 0.0020 |
| Black / African / Carib. / Black British - Other | 0.0011 | 0.0018 | -0.0007 | 0.2675 | 0.0138 |
| Mixed / Multiple - White and Asian | 0.0036 | 0.0044 | -0.0008 | 0.5060 | 0.0085 |
| Mixed / Multiple - White and Black African | 0.0007 | 0.0009 | -0.0003 | 0.5993 | 0.0066 |

| | | | | | |
|---|---|---|---|---|---|
| Mixed / Multiple - White and Black Caribb. | 0.0011 | 0.0011 | -0.0001 | 0.5993 | 0.0012 |
| Mixed / Multiple - Other | 0.0026 | 0.0034 | -0.0008 | 0.4049 | 0.0106 |
| White - British / Engl. / Irish / Scottish / Welsh | 0.8585 | 0.8336 | 0.0249 | 0.0001 | 0.0488 |
| White - Gypsy or Irish Traveller | 0.0011 | 0.0005 | 0.0006 | 0.2483 | 0.0161 |
| White - Irish | 0.0141 | 0.0149 | -0.0008 | 0.7033 | 0.0049 |
| White - Other | 0.0692 | 0.0734 | -0.0042 | 0.3704 | 0.0116 |
| Other Ethnic Group - Arab | 0.0016 | 0.0734 | -0.0007 | 0.4058 | 0.0105 |
| Other Ethnic Group - Other | 0.0071 | 0.0048 | 0.0023 | 0.1197 | 0.0209 |
| Religion: None | 0.3598 | 0.3224 | 0.0374 | 0.0000 | 0.0558 |
| Buddhist | 0.0085 | 0.0106 | -0.0020 | 0.2447 | 0.0149 |
| Christian | 0.5547 | 0.5826 | -0.0279 | 0.0023 | 0.0398 |
| Hindu | 0.0079 | 0.0119 | -0.0041 | 0.0203 | 0.0291 |
| Jewish | 0.0095 | 0.0101 | -0.0006 | 0.7394 | 0.0043 |
| Muslim | 0.0102 | 0.0190 | -0.0089 | 0.0000 | 0.0525 |
| Sikh | 0.0038 | 0.0067 | -0.0028 | 0.0249 | 0.0278 |
| Other | 0.0168 | 0.0188 | -0.0020 | 0.0249 | 0.0107 |
| Prefer Not to Say | 0.0288 | 0.0179 | 0.0109 | 0.0002 | 0.0512 |
| Physical or Mental Health Condition: No | 0.7468 | 0.7391 | 0.0077 | 0.3405 | 0.0124 |
| Yes | 0.7468 | 0.2402 | -0.0136 | 0.0788 | 0.0228 |
| Don’t Know | 0.0090 | 0.0094 | -0.0005 | 0.7937 | 0.0034 |
| Prefer Not to Say | 0.0177 | 0.0112 | 0.0064 | 0.0047 | 0.0382 |
| Self-Isolating: No | 0.9250 | 0.9137 | 0.0113 | 0.0227 | 0.0293 |
| Yes | 0.0704 | 0.0808 | -0.0104 | 0.0312 | 0.0277 |
| Don’t Know | 0.0046 | 0.0055 | -0.0009 | 0.4721 | 0.0092 |
| Employment: Full-Time Employed | 0.2564 | 0.2636 | -0.0072 | 0.3727 | 0.0116 |
| Part-Time Employed | 0.1545 | 0.1486 | 0.0059 | 0.3727 | 0.0117 |

| | | | | | |
|---|---|---|---|---|---|
| Furloughed | 0.0766 | 0.0900 | -0.0134 | 0.0075 | 0.0344 |
| In Education | 0.0087 | 0.0099 | -0.0011 | 0.5169 | 0.0083 |
| Unemployed | 0.0480 | 0.0519 | -0.0039 | 0.3235 | 0.0128 |
| Disabled | 0.0130 | 0.0184 | -0.0054 | 0.0155 | 0.0306 |
| Retired | 0.3333 | 0.3031 | 0.0302 | 0.0004 | 0.0459 |
| Looking After Family | 0.0484 | 0.0556 | -0.0072 | 0.0004 | 0.0228 |
| Doing Something Else | 0.0611 | 0.0590 | 0.0021 | 0.6386 | 0.0061 |
| Motivation: Altruistic (i.e. Helping in Crisis) | 0.9807 | 0.9837 | -0.0030 | 0.2200 | 0.0162 |
| Thought It Was Expected | 0.0651 | 0.0750 | -0.0100 | 0.0316 | 0.0276 |
| Like Telling Family, Friends | 0.0508 | 0.0679 | -0.0172 | 0.0001 | 0.0514 |
| Was Asked | 0.0102 | 0.0140 | -0.0038 | 0.0489 | 0.0249 |
| Like Helping People | 0.6253 | 0.7212 | -0.0959 | 0.0000 | 0.1454 |
| Wanted to Meet People | 0.0347 | 0.0546 | -0.0199 | 0.0000 | 0.0682 |
| Gain Skills | 0.1231 | 0.1622 | -0.0391 | 0.0000 | 0.0792 |
| Pursue Career | 0.0338 | 0.0523 | -0.0185 | 0.0000 | 0.0644 |
| Had Some Time | 0.1008 | 0.1147 | -0.0140 | 0.0133 | 0.0319 |
| Other | 0.0366 | 0.0404 | -0.0038 | 0.2767 | 0.0140 |
| Volunteered Before: No | 0.2115 | 0.2139 | -0.0023 | 0.7588 | 0.0040 |
| Yes | 0.7885 | 0.7861 | 0.0023 | 0.7588 | 0.0040 |
| Volunteering Elsewhere: No | 0.6619 | 0.6574 | 0.0045 | 0.6036 | 0.0068 |
| Yes, One Other Group | 0.2042 | 0.1996 | 0.0046 | 0.5339 | 0.0081 |
| Yes, More Than One Other Group | 0.1338 | 0.1430 | -0.0091 | 0.1489 | 0.0187 |
| If Elsewhere: Informal Local Group | 0.0468 | 0.0473 | -0.0004 | 0.9099 | 0.0015 |
| Organised Local Group | 0.1045 | 0.1042 | 0.0003 | 0.9591 | 0.0007 |
| National Charity | 0.0427 | 0.0404 | 0.0023 | 0.9591 | 0.0081 |
| Public Sector | 0.0311 | 0.0312 | -0.0001 | 0.9758 | 0.0004 |

| | | | | | |
|---|---|---|---|---|---|
| Sports Group | 0.0163 | 0.0177 | -0.0014 | 0.5518 | 0.0077 |
| Faith Group | 0.0407 | 0.0438 | -0.0031 | 0.3978 | 0.0109 |
| Other | 0.0560 | 0.0581 | -0.0021 | 0.6290 | 0.0063 |
| | | | | | |
| *Regional Covid-19 Controls* | | | | | |
| New Covid-19 Hospital Admissions | 12.5573 | 12.4711 | 0.0862 | 0.4939 | 0.0089 |
| Cumulative Covid-19 Hospital Admissions | 15,624.0968 | 15,888.2256 | -264.1287 | 0.0092 | 0.0339 |
| Current Covid-19 Hospital Cases | 213.5625 | 213.6037 | -0.0412 | 0.9835 | 0.0003 |
| Occupied Medical Ventilation Beds | 19.2030 | 19.4472 | -0.2442 | 0.2486 | 0.0150 |
| New Covid-19 Cases | 65.8391 | 67.1794 | -1.3403 | 0.0759 | 0.0231 |
| Cumulative Covid-19 Cases | 35,410.4116 | 35,794.2010 | -383.7894 | 0.0659 | 0.0240 |
| New Covid-19 Deaths | 3.2574 | 3.1820 | 0.0754 | 0.1242 | 0.0201 |
| Cumulative Covid-19 Deaths | 5,189.7423 | 5,252.5030 | -62.7607 | 0.0347 | 0.0276 |
| | | | | | |
| N | 9,163 | 4,358 | - | - | - |

*Notes:* Normalised differences are calculated as $\Delta x = (\bar{x}_t - \bar{x}_c) / \sqrt{(\sigma_t^2 + \sigma_c^2)}$, where $\bar{x}_t$ and $\bar{x}_c$ is the sample mean of the covariate in the treatment and control group, respectively (here: different samples). $\sigma^2$ denotes the respective variance. A normalised difference greater than 0.25 suggests a non-balanced covariate (Imbens and Wooldridge, 2009; Imbens and Rubin, 2015).
*Source:* NHSVR Survey Data, Administrative Data, July 2020; own calculations.

Table A8: Average Treatment Effects (Weighted by Inverse Probability of Being in Main Estimation Sample)

| | Life Satisfaction | Sense of Purpose in Life | Belongingness | Connectedness |
|---|---|---|---|---|
| *Inverse Probability Weights from Table 7 Column 1* | | | | |
| $Treatment_i$ | 0.2220*** | 0.2521*** | 0.0275* | 0.0613*** |
| *(Volunteered Vs. Not Given Task)* | (0.0740) | (0.0739) | (0.0164) | (0.0182) |
| Number of Observations | 4,358 | 4,358 | 4,358 | 4,358 |
| Number of Treated | 3,214 | 3,214 | 3,214 | 3,214 |
| Number of Controlled | 1,144 | 1,144 | 1,144 | 1,144 |
| R Squared | 0.1564 | 0.1422 | 0.1209 | 0.1090 |
| *Inverse Probability Weights from Table 7 Column 2* | | | | |
| $Treatment_i$ | 0.2388** | 0.2885*** | 0.0518** | 0.0584*** |
| *(Volunteered Vs. Not Given Task)* | (0.0965) | (0.0910) | (0.0210) | (0.0217) |
| Number of Observations | 4,358 | 4,358 | 4,358 | 4,358 |
| Number of Treated | 3,214 | 3,214 | 3,214 | 3,214 |
| Number of Controlled | 1,144 | 1,144 | 1,144 | 1,144 |
| R Squared | 0.2200 | 0.2426 | 0.2585 | 0.2548 |
| Individual Controls | Yes | Yes | Yes | Yes |
| Regional Covid-19 Controls | Yes | Yes | Yes | Yes |
| Postcode Fixed Effects | Yes | Yes | Yes | Yes |
| Date Fixed Effects | Yes | Yes | Yes | Yes |

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

*Notes:* $Treatment_i$ takes on one if a volunteer has been allocated, has accepted, and has completed at least one task in a specific service, and zero if a volunteer has not been allocated a task yet, at the time of the survey. The zero-category excludes individuals who have rejected a task. It is constructed using the archive of all tasks and their timestamps in administrative records. See Section 3.1 for a description of our data and Table 1 for summary statistics.

*Source:* NHSVR Survey Data, Administrative Data, July 2020; own calculations.

**Online Appendix**

**W1. Replication Using Extended Estimation Sample**

Table W1.1: Average Treatment Effects in Extended Estimation Sample

| | Life Satisfaction (1) | Sense of Purpose in Life (2) | Belongingness (3) | Connectedness (4) |
|---|---|---|---|---|
| $Treatment_i$ | 0.1685*** | 0.1801*** | 0.0418*** | 0.0699*** |
| *(Volunteered Vs. Not Given Task)* | (0.0483) | (0.0473) | (0.0112) | (0.0120) |
| | | | | |
| Stepdown P-Value ($Treatment_i$) | 0.0099 | 0.0099 | 0.0099 | 0.0099 |
| | | | | |
| Individual Controls | Yes | Yes | Yes | Yes |
| Regional Covid-19 Controls | Yes | Yes | Yes | Yes |
| Region Fixed Effects | Yes | Yes | Yes | Yes |
| Date Fixed Effects | Yes | Yes | Yes | Yes |
| | | | | |
| Scaling | 0-10 | 0-10 | 0-1 | 0-1 |
| Mean | 7.2 | 7.5 | 0.7 | 0.5 |
| σ | 2.1 | 2.0 | 0.5 | 0.5 |
| | | | | |
| Number of Observations | 9,163 | 9,163 | 9,163 | 9,163 |
| Number of Treated | 6,375 | 6,375 | 6,375 | 6,375 |
| Number of Controlled | 2,788 | 2,788 | 2,788 | 2,788 |
| R Squared | 0.1195 | 0.1072 | 0.0582 | 0.0457 |

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
*Source:* NHSVR Survey Data, July 2020; own calculations.

Table W1.2: Heterogeneous Treatment Effects By Type of Task in Extended Estimation Sample

| | Life Satisfaction (1) | Sense of Purpose in Life (2) | Belongingness (3) | Connectedness (4) |
|---|---|---|---|---|
| *Transport* | | | | |
| Treatment$_i$ | 0.1410* | 0.1774** | 0.0435** | 0.0675*** |
| *(Volunteered Vs. Not Given Task)* | (0.0838) | (0.0818) | (0.0197) | (0.0210) |
| Stepdown P-Value (Treatment$_i$) | 0.0693 | 0.0594 | 0.0594 | 0.0099 |
| Number of Observations | 3,093 | 3,093 | 3,093 | 3,093 |
| Number of Treated | 1,845 | 1,845 | 1,845 | 1,845 |
| Number of Controlled | 1,248 | 1,248 | 1,248 | 1,248 |
| R Squared | 0.1548 | 0.1327 | 0.0855 | 0.0661 |
| *Community Response* | | | | |
| Treatment$_i$ | 0.1569** | 0.1812*** | 0.0399*** | 0.0813*** |
| *(Volunteered Vs. Not Given Task)* | (0.0615) | (0.0602) | (0.0142) | (0.0152) |
| Stepdown P-Value (Treatment$_i$) | 0.0198 | 0.0198 | 0.0198 | 0.0099 |
| Number of Observations | 5,839 | 5,839 | 5,839 | 5,839 |
| Number of Treated | 4,284 | 4,284 | 4,284 | 4,284 |
| Number of Controlled | 1,555 | 1,555 | 1,555 | 1,555 |
| R Squared | 0.1243 | 0.1129 | 0.0583 | 0.0560 |
| *Check In And Chat* | | | | |
| Treatment$_i$ | 0.2829*** | 0.2947*** | 0.0479*** | 0.0736*** |
| *(Volunteered Vs. Not Given Task)* | (0.0700) | (0.0705) | (0.0163) | (0.0174) |

| | | | | |
|---|---|---|---|---|
| Stepdown P-Value ($Treatment_i$) | 0.0099 | 0.0099 | 0.0099 | 0.0099 |
| | | | | |
| Number of Observations | 5,002 | 5,002 | 5,002 | 5,002 |
| Number of Treated | 3,894 | 3,894 | 3,894 | 3,894 |
| Number of Controlled | 1,108 | 1,108 | 1,108 | 1,108 |
| R Squared | 0.1358 | 0.1242 | 0.0660 | 0.0562 |
| | | | | |
| Individual Controls | Yes | Yes | Yes | Yes |
| Regional Covid-19 Controls | Yes | Yes | Yes | Yes |
| Region Fixed Effects | Yes | Yes | Yes | Yes |
| Date Fixed Effects | Yes | Yes | Yes | Yes |

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
*Source:* NHSVR Survey Data, July 2020; own calculations.

## W2. Further Robustness Checks

### *W2.1. Alternative Estimators*

Throughout the paper, we apply linear models to our ordinal outcomes *life satisfaction* and *sense of purpose in life,* which may yield measurement error. Although this error has been found to be minor in most applications (cf. Ferrer-i-Carbonell & Frijters, 2004), we nevertheless re-estimate our linear models using ordered logit models. Table W2.1 Panel A shows our results, presenting coefficients in both log-odds and odds ratios.

The log-odds and odds ratios of both life satisfaction and sense of purpose in life turn out significant and positive, in line with our main results. Moreover, the odds ratios suggest that volunteering increases the probability of being in a higher category (i.e. a one-point increase on the zero-to-ten scale) of life satisfaction and sense of purpose in life (as opposed to being in all lower categories) by about 20% and 22%, respectively.

In a recent paper, Bond and Lang (2019) have shown that results from ordered models, which typically focus on the mean levels of wellbeing in different groups, can be reversed. To avoid potential reversal, Chen et al. (2022) suggest reinterpreting the results of ordered models as the effects on the *median* rather than the mean. This is possible because the median and the mean of the latent variable in ordered models coincide due to the symmetry of logistic and normal distributions. Effects on the median can be estimated under very week conditions and the results cannot be reversed following the Bond and Lang (2019) argument, as long as heteroskedasticity is accounted for.

In our case, heteroskedasticity may cause concern if there is a difference between the variance in life satisfaction of our treatment group and that of our control group, for example. Appendix Figures W2.1a and W2.1b show that life satisfaction is similarly distributed in both groups, suggesting that heteroskedasticity may be less of a concern. We nevertheless re-estimate our models using heteroskedastic ordinal logit models. Table W2.1 Panel B shows our results, again presenting coefficients in both log-odds and odds ratios.

In line with our main results, we find that life satisfaction and sense of purpose in life turn out significant and positive. The size of the odds-logs and odds ratios is slightly higher, whereas the significance levels are slightly attenuated when accounting for heteroscedasticity in case of sense of purpose in life. Accounting for heteroskedasticity in our ordered logit models, therefore, leaves our results qualitatively unchanged, as has already been demonstrated for ordered probit models by Bond and Lang (2019) themselves. A caveat of our analysis is that we cannot control for fidelity (i.e. the time taken to complete the survey), regional Covid-19 controls, and interview date fixed effects, as the maximum likelihood estimator fails to converge when including these covariates.

Figure W2.1a: Distribution of Life Satisfaction in Estimation Sample for Treatment Group

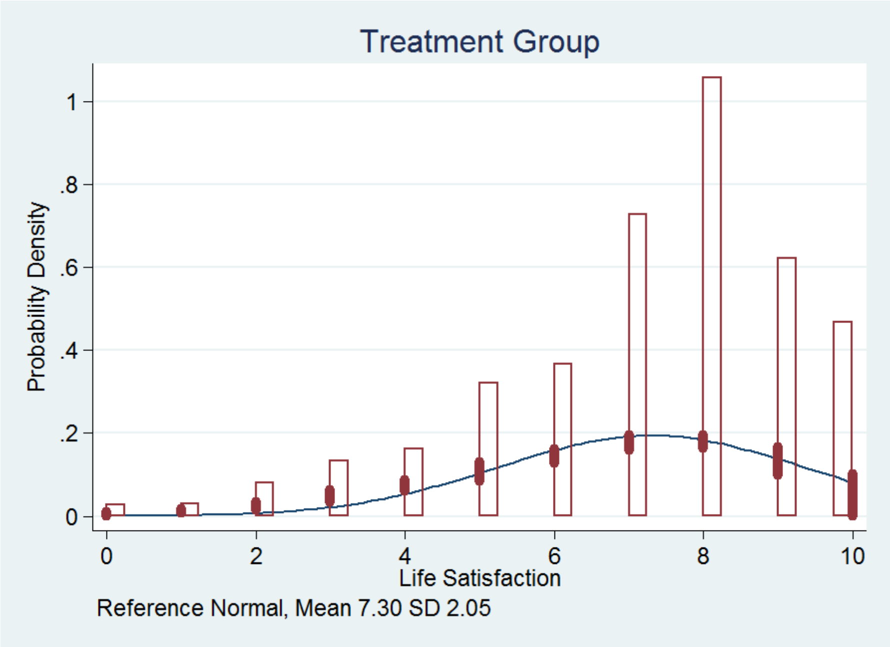



Figure W2.1b: Distribution of Life Satisfaction in Estimation Sample for Control Group

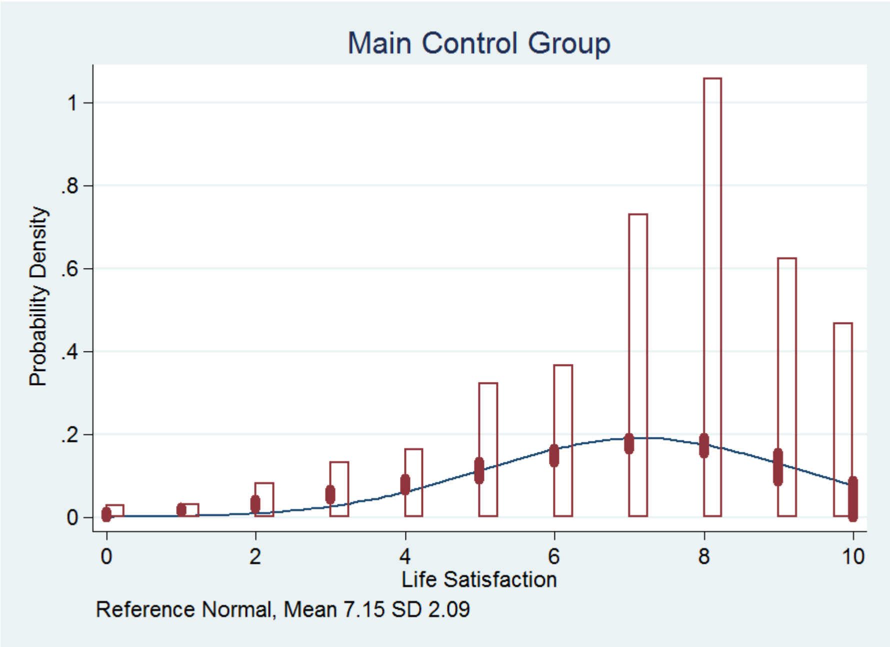


Table W2.1: Alternative Estimators

| | Life Satisfaction | | Sense of Purpose in Life | |
|---|---|---|---|---|
| | (1a) | (1b) | (2a) | (2b) |
| *Panel A: Ordered Logit Models* | *Odd-Logs* | *Odds Ratios* | *Odd-Logs* | *Odds Ratios* |
| Treatment$_i$ | 0.1871*** | 0.1968*** | 1.2057*** | 1.2175*** |
| *(Volunteered Vs. Not Given Task)* | (0.0646) | (0.0674) | (0.0779) | (0.0821) |
| | | | | |
| Number of Observations | 4,358 | 4,358 | 4,358 | 4,358 |
| Number of Treated | 3,214 | 3,214 | 3,214 | 3,214 |
| Number of Controlled | 1,144 | 1,144 | 1,144 | 1,144 |
| Pseudo R Squared | 0.0356 | 0.0314 | 0.0356 | 0.0314 |
| | | | | |
| Individual Controls | Yes | Yes | Yes | Yes |
| Regional Covid-19 Controls | Yes | Yes | Yes | Yes |
| Postcode Fixed Effects | Yes | Yes | Yes | Yes |
| Date Fixed Effects | Yes | Yes | Yes | Yes |
| | | | | |
| *Panel B: Heteroskedastic Ordered Logit Models* | *Odd-Logs* | *Odds Ratios* | *Odd-Logs* | *Odds Ratios* |
| Treatment$_i$ | 0.2347*** | 0.2525*** | 1.2645** | 1.2813** |
| *(Volunteered Vs. Not Given Task)* | (0.0990) | (0.0861) | (0.1251) | (0.1129) |
| | | | | |
| Number of Observations | 4,358 | 4,358 | 4,358 | 4,358 |
| Number of Treated | 3,214 | 3,214 | 3,214 | 3,214 |
| Number of Controlled | 1,144 | 1,144 | 1,144 | 1,144 |
| Pseudo R Squared | 0.0366 | 0.0319 | 0.0366 | 0.0319 |
| | | | | |
| Individual Controls | Yes | Yes | Yes | Yes |
| Regional Covid-19 Controls | Yes | Yes | Yes | Yes |

| | | | | |
|---|---|---|---|---|
| Postcode Fixed Effects | Yes | Yes | Yes | Yes |
| Date Fixed Effects | No | No | No | No |

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

*Notes:* $Treatment_i$ takes on one if a volunteer has been allocated, has accepted, and has completed at least one task, and zero if a volunteer has not been allocated a task yet, at the time of the survey. The zero-category excludes individuals who have rejected a task. It is constructed using the archive of all tasks and their timestamps in administrative records. The heteroskedastic ordered logit models do not control for fidelity (i.e. the time taken to complete the survey), regional Covid-19 controls, and interview date fixed effects, as the maximum likelihood estimator fails to converge when including these covariates. See Section 3.1 for a description of our data and Table 1 for summary statistics.
*Source:* NHSVR Survey Data, Administrative Data, July 2020; own calculations.

## W3. Additional Analyses

*Heterogeneous Treatment Effects by Motivation for Joining*

Our survey asked volunteers about their motivations to join the NHSVR programme. Notwithstanding issues of social desirability, attitude expression, and imperfect recall, we cautiously exploit volunteers' self-reports to shed light on heterogeneous treatment effects by motivation to join.

Volunteers could report multiple motivations at the same time. We group them into the following motivational categories:

- *Social: Pure Altruism* refers to whether a volunteer reports that they were responding to a national crisis, wanted to support the NHS, wanted to make a difference, or wanted to help their local community.
- *Social: Social Norm* refers to whether a volunteer reports to have thought that joining was expected of them.
- *Social: Social Reputation* refers to whether a volunteer reports to enjoy telling their friends or family about their volunteering.
- *Social: Social Network* refers to whether a volunteer reports that someone asked them to give help.
- *Self: Impure Altruism* refers to whether a volunteer reports to enjoy helping other people.
- *Self: Social Connection* refers to whether a volunteer reports to have wanted to meet new people or make new friends.
- *Self: Skills* refers to whether a volunteer reports to have wanted to gain or use skills and experience.

- *Self: Career* refers to whether a volunteer reported to have an interest in pursuing a career in healthcare or the NHS.
- *Self: Time* refers to whether a volunteer reported to have been furloughed and hence to have time to volunteer.

To look at heterogeneous treatment effects by motivation to join, we interact our treatment dummy with each motivational category. Table W3 below shows our findings, focusing, for ease of exposition, on the interactions and suppressing the levels.

We do not find strong evidence for heterogeneous treatment effects by motivation to join, possibly because motivations were already quite homogeneously distributed amongst individuals who selected into the NHSVR programme, with little differences between those who volunteered at any point in time (our treatment group) and those who did not get to volunteer because they had not been given a task yet (our control group).

If anything, we find some evidence that volunteers who report to be responding to social expectations or norms, or who report to have joined simply because they enjoy helping other people, to benefit more in terms of overall life satisfaction and sense of purpose in life. Interestingly, volunteers who report to have joined because of personal reputation generate the largest life satisfaction benefits, yet no benefits in terms of worthwhileness, possibly pointing towards the importance of relative social comparisons which are more likely to be picked up in hedonic (i.e. life satisfaction) rather than eudemonic measures (i.e. sense of purpose in life) of wellbeing.

Table W3: Heterogeneous Treatment Effects By Motivation

| Treatment$_i$ *(Volunteered Vs. Not Given Task)* | Life Satisfaction (1) | Sense of Purpose (2) | Belongingness (3) | Connectedness (4) |
|---|---|---|---|---|
| x Social: Pure Altruism | 0.2954 | 0.2375 | -0.0238 | 0.0032 |
| | (0.3455) | (0.3134) | (0.0742) | (0.0749) |
| x Social: Social Norm | 0.3459* | 0.3545* | -0.0118 | -0.0523 |
| | (0.1935) | (0.1895) | (0.0418) | (0.0467) |
| x Social: Reputation | 0.5735* | 0.3972 | 0.0503 | -0.0947 |
| | (0.3403) | (0.3363) | (0.0676) | (0.0716) |
| x Social: Network | -0.6664 | -0.2700 | -0.0184 | -0.0298 |
| | (0.4553) | (0.5562) | (0.0986) | (0.1127) |
| x Self: Impure Altruism | 0.1659* | 0.1617* | 0.0135 | 0.0160 |
| | (0.0945) | (0.0915) | (0.0221) | (0.0236) |
| x Self: Social Connection | 0.0922 | 0.0631 | -0.0405 | 0.0182 |
| | (0.2950) | (0.2912) | (0.0660) | (0.0680) |
| x Self: Skills | -0.1001 | 0.0485 | -0.0554 | 0.0083 |
| | (0.1575) | (0.1599) | (0.0357) | (0.0391) |
| x Self: Career | 0.4896 | 0.2658 | -0.0057 | 0.0100 |
| | (0.3472) | (0.3590) | (0.0721) | (0.0747) |
| x Self: Time | 0.0436 | -0.0108 | -0.0194 | 0.0072 |
| | (0.1572) | (0.1528) | (0.0370) | (0.0382) |
| | | | | |
| Individual Controls | Yes | Yes | Yes | Yes |
| Regional Covid-19 Controls | Yes | Yes | Yes | Yes |
| Region Fixed Effects | Yes | Yes | Yes | Yes |
| Date Fixed Effects | Yes | Yes | Yes | Yes |

| | | | | |
|---|---|---|---|---|
| Number of Observations | 9,163 | 9,163 | 9,163 | 9,163 |
| Number of Treated | 6,375 | 6,375 | 6,375 | 6,375 |
| Number of Controlled | 2,788 | 2,788 | 2,788 | 2,788 |
| R Squared | 0.1196 | 0.1075 | 0.0549 | 0.0426 |

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
*Sources:* NHSVR Survey Data, July 2020; own calculations.

## W4. Description of Data

The survey data include the wellbeing outcomes as well as self-reports on volunteering in the programme (and elsewhere) and self-reports of the broad geographical regions of the places of residence of volunteers, alongside a range of self-reported covariates. The administrative data include only administrative variables on tasks and task behaviour (such as the services of volunteering and the number of tasks in each service, as well as the geographical coordinates and timestamps of tasks) as well as the exact geographical coordinates of the places of residence of volunteers, all of which were collected by the app. Table W4.1 provides an overview of our datasets and which types of variables are available in which dataset.

Table W4.1: Overview of Datasets and Types of Variables

| | A: Survey Data | B: Combined Dataset of Survey and Administrative Data (= A ∩ C) | C: Administrative Records |
|---|---|---|---|
| Person Identifiers | ▪ Survey ID | ▪ Survey ID<br>▪ Administrative ID | ▪ Administrative ID |
| Geographical Identifiers | ▪ 12 NHS Regions | ▪ 12 NHS Regions<br>▪ Precise Geographical Coordinates *(for Postcodes and Other Geographical Units)* | ▪ Precise Geographical Coordinates *(for Postcodes and Other Geographical Units)* |
| Outcomes | ▪ Wellbeing | ▪ Wellbeing | |
| Variables of Interest | ▪ Self-Reported Volunteering<br>▪ Self-Reported Number of Tasks *(Truncated at 10+)*<br>▪ Self-Reported Services | ▪ Self-Reported Volunteering<br>▪ Self-Reported Number of Tasks *(Truncated at 10+)*<br>▪ Self-Reported Services<br>▪ Administrative Volunteering *(Allocated, Accepted, Completed, Rejected, or Timed-Out Task; Precise Geographical Coordinates* | ▪ Administrative Volunteering *(Allocated, Accepted, Completed, Rejected, or Timed-Out Task; Precise Geographical Coordinates* |

| | | | |
|---|---|---|---|
| | | *and Timestamps of Tasks)*<br>▪ Administrative Number of Tasks<br>(in Total, by Service)<br>▪ Administrative Services | *and Timestamps of Tasks)*<br>▪ Administrative Number of Tasks<br>(in Total, by Service)<br>▪ Administrative Services |
| Controls | ▪ Self-Reported Age<br>▪ Other Self-Reported Demographic and Socio-Economic Characteristics | ▪ Self-Reported Age<br>▪ Other Self-Reported Demographic and Socio-Economic Characteristics<br><br>▪ Administrative Age | ▪ Administrative Age |

By linking our survey data (A in Table W4.1) to the administrative records (C), we retain only about half of the observations in our estimation sample (4,358 out of 9,163 observations, or 48%) of the combined dataset of survey and administrative data (B = A ∩ C). This is because the unique person identifier linking our survey data to the administrative records is a volunteer's e-mail address, and we have e-mail addresses for only about half of our volunteers.

The combined survey and administrative data form the basis for our *main estimation sample*, whereas the survey data (including respondents who can and who cannot be matched to their administrative records) form the basis for our *extended estimation sample*. Despite a lower sample size, we are using the combined survey and administrative data as our main estimation sample to avoid relying on self-reports of volunteering as well as to avoid relying on self-reports of broader geographical regions from our survey data. Importantly, though, including postcode fixed effects helps with our identification strategy. Table W4.2 provides an overview of our datasets and samples.

Table W4.2: Overview of Datasets and Samples

| | A: Survey Data | B: Combined Dataset of Survey and Administrative Data<br>(= A ∩ C) | C: Administrative Records |
|---|---|---|---|

| | | | |
|---|---|---|---|
| Raw Sample | N = 12,056 | N = 4,898 | 590,633 |
| Intermediary Sample<br>*No Missings for Group Allocation* | N = 10,682<br>**$N_T$ = 7,356 (68.9%)**<br>**$N_C$ = 3,326 (31.1%)** | N = 4,718<br>**$N_T$ = 3,415 (72.4%)**<br>**$N_C$ = 1,303 (27.6%)** | N = 366,482 [a)]<br>**$N_T$ = 225,069 (61.4%)**<br>**$N_C$ = 141,413 (38.6%)** |
| Estimation Sample<br>*No Missings for Outcomes and Controls* | N = 9,163<br>**$N_T$ = 6,375 (69.6%)**<br>**$N_C$ = 2,788 (30.4%)** | N = 4,358<br>**$N_T$ = 3,214 (73.8%)**<br>**$N_C$ = 1,144 (26.3%)** | N/A |

$N_T$ = treatment group size, $N_C$ = control group size; figures are rounded.
a) Volunteers who downloaded the app and switched it ‘on duty’ at least once during our observation period (hence chose to be available for task allocation and, thereby, treatment or control group allocation).

**W5. Materials**

Link