

INCREASING PARTICIPATION IN A MOBILE APP STUDY: THE EFFECTS OF A SEQUENTIAL MIXED-MODE DESIGN AND IN-INTERVIEW INVITATION

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Mobile apps are an attractive and versatile method of collecting data in the social and behavioral sciences. In samples of the general population, however, participation in app-based data collection is still rather low. In this article, we examine two potential ways of increasing participation and potentially reducing participation bias in app-based data collection: (1) inviting sample members to a mobile app study within an interview rather than by post and (2) offering a browser-based follow-up to the mobile app. We use experimental data from Spending Study 2, collected on the *Understanding Society* Innovation Panel and on the Lightspeed UK online access panel. Sample members were invited to download a spending diary app on their smartphone or use a browser-based online diary to report all their purchases for one month. The results suggest that inviting sample members to an app study within a face-to-face interview

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increases participation rates but does not bring in different types of participants. In contrast, the browser-based alternative can both increase participation rates and reduce biases in who participates if offered immediately once the app had been declined. We find that the success of using mobile apps for data collection hinges on the protocols used to implement the app.

KEYWORDS: Spending diary; Smartphone; Mobile application; Participation rates; Participation bias; Protocol adherence.

Statement of Significance

We test different ways of increasing participation rates and reducing participation bias in data collection using mobile applications. Respondents were asked to install an app and use it daily for a month to report their spending. We find that inviting respondents to the app study within a face-to-face interview increases participation rates, compared to sending respondents an invitation by post. The mode of invitation does, however, not affect biases in which types of people participate. We further find that offering respondents a browser-based alternative to the app can increase participation rates and reduce biases in who participates. The findings show that success of using mobile apps for data collection hinges on the protocols used to invite respondents to the app.

1. INTRODUCTION

Mobile apps installed on smartphones are an attractive and versatile method of collecting data in the social and behavioral sciences. Since most people carry their smartphones with them throughout the day, mobile apps can be used to collect data passively, for example, about the person's geolocation or movements (Link, Murphy, Schober, Buskirk, Childs, et al. 2014; Harari, Lane, Wang, Crosier, Campbell, et al. 2016). Apps can also be used to collect data in real time, for example, by triggering surveys via geo-fences or at randomized times during the day, and to implement diaries that are ideally completed close in time to the events of interest. One of the challenges of app-based data collection, however, is that participation is still rather low in samples of the general population. For example, in an earlier app study that we implemented on the *Understanding Society* Innovation Panel (IP) in Great Britain (Spending Study 1 (SS1), where we invited sample members to download a receipt scanning app on their smartphone or tablet), we found that just 13 percent of the full sample, or 16 percent of mobile device owners, installed and used the app at

least once (Jäckle, Burton, Couper, and Lessof 2019). Similarly, an app study implemented on the Labour Market and Social Security (PASS) Panel in Germany (the IAB-SMART app study, capturing a range of smartphone sensor data), achieved a participation rate of 16 percent of the Android smartphone owners invited to the study (Kreuter, Haas, Keusch, Bähr, and Trappmann 2020). Two app studies implemented on the Longitudinal Internet Studies for the Social Sciences (LISS) Panel in the Netherlands (the Smartphone Time Use study, collecting time use and experience sampling data, and the Mobile Mobility study, collecting data from the user's geolocation and movements), achieved participation rates of 19 percent and 22 percent, respectively (Scherpenzeel 2017). For each of the app studies reported here, sample members were recruited from existing longitudinal panels who had been interviewed previously and might have developed some level of commitment to the study. App studies fielded on new cross-sectional samples of the general population are likely to yield even lower participation rates. McCool, Lugtig, Mussmann, and Schouten (2021) conducted a field test of a travel study in which participants were asked to download an app and provide seven days of time-location sensor data. Half the sample came from the Dutch population register, while the balance had previously participated in a travel diary survey. App registration rates were 26.5 percent for the fresh sample and 44.4 percent for previous respondents.

Previous research has shown that the low level of participation is in part due to coverage issues, not everyone has a mobile device that is compatible with the study app; in part it is due to the reluctance of sample members to download and use apps for data collection; and in part it is likely because we have not yet figured out how best to implement app-based data collection in ways that reduce the barriers to participation (Jäckle, Burton, Couper, et al. 2019; Keusch, Bähr, Haas, Kreuter, and Trappmann 2020; Wenz, Jäckle, Burton, and Couper 2020).

In our earlier app study, SS1, we found clear differences in the types of people who did and did not participate, which compounded differences between the types of people who did and did not have a mobile device (Jäckle, Burton, Couper, et al. 2019). Although there were differences in sociodemographic characteristics, these were mainly driven by mobile device access and mobile device usage patterns. Whether a sample member already did similar activities for their own purposes was highly predictive of whether they participated in SS1. For example, those who kept a budget were over-represented among participants by about 10 percentage points compared to the full sample, while those who used an app on a mobile device to check their bank balance were over-represented by 20 percentage points. There were, however, no differences between participants and nonparticipants in correlates of what the app was designed to measure: household spending, whether they were struggling or behind with paying their housing costs, and subjective assessment of how well they were getting by financially.

Various methods have been tested to increase coverage and participation rates and reduce participation biases by altering features of the data collection protocol. For example, the LISS Panel equipped sample members who did not have a compatible device with a smartphone, which reduced biases in the Smartphone Time Use study (Scherpenzeel 2017). The IAB-SMART app study experimented with different levels of monetary incentives for app installation and study participation: sample members were randomly allocated to an incentive of €10 versus €20 for installing the study app and to an incentive of €5 versus €10 for sharing the full range of sensor data (Haas et al. 2020). While the higher incentive for app installation led to a statistically significant but modest increase in participation rates, the higher incentive for data sharing did not significantly increase participation rates. Similarly, we experimented with different levels of incentives for app download (£2 versus £6) in SS1, but did not find a significant effect on participation rates (Jäckle, Burton, Couper, et al. 2019). We have also tested the effectiveness of providing personalized feedback as a nonmonetary approach to incentivizing sample members to participate in a mobile app study, in a part of the present study (Spending Study 2 (SS2)) implemented on the Lightspeed UK online access panel. Although participants reacted positively to the feedback, it did not have the intended effect of increasing initial participation in the study or ongoing adherence to the study protocol (Wenz et al. 2020).

In this paper, we examine the effectiveness of two potential ways of increasing participation rates and reducing participation biases in app-based data collection: (1) inviting sample members to the app study within an interview rather than by post; and (2) offering a browser-based follow-up to the mobile app. We use data from SS2 collected on two samples: the *Understanding Society* IP, a probability mixed mode (face-to-face, online) household panel in Great Britain, and the Lightspeed UK online access panel. In both samples, panelists were invited to use an app or browser-based spending diary to report all their purchases for one month.

We test the first design feature experimentally by randomly allocating sample members to invitations within a survey or by postal letter. Face-to-face interviewers might be able to motivate and assist sample members in downloading and installing the app and clarify questions that those who are less experienced or confident in using mobile devices may have. We know of no prior research on the mode of invitation to app-based studies. The literature on consent to data linkage, a within-survey request similar to the request of installing and sharing data via an app, suggests that consent rates are higher when the consent request is presented in an interviewer-administered (face-to-face) survey compared to a self-administered (post, web) survey (Sakshaug, Hülle, Schmucker, and Liebig 2017; Al Baghal, Sloan, Jessop, Williams, and Burnap 2020). However, there are more time pressures in a face-to-face interview and the respondent and interviewer may be reluctant to take the time to download the app during the interview. Those who complete the survey online may also

be more comfortable downloading an app, which may also negate the advantage of the interviewer. But even for sample members who complete the survey online rather than with an interviewer, the hurdle of downloading the app might seem lower if it is presented as part of a task that they are already engaged in. That is, we expect a positive effect of an in-interview request, but have no strong expectations regarding the effect of interview mode.

We test the second design feature quasi-experimentally by implementing a sequential mixed-mode design. Sample members who cannot or do not want to download the app are offered a browser-based follow-up as an alternative to the mobile app. The browser-based version mirrors the design and functionality of the app. Previous research suggests that presenting sample members with a sequential mixed-mode design can increase response rates (e.g., [Millar and Dillman 2011](#)) although the results are mixed and seem to depend on how the data collection modes are combined (see [Lynn 2013](#); [Patrick, Couper, Laetz, Schulenberg, O'Malley, et al. 2018](#)).

We use the data from SS2 to address the following research questions:

- (1) Does introducing the app within an interview increase participation compared to sending a separate invitation by post?
- (2) Does the effect of introducing the app within the interview vary with the mode of interview?
- (3) Does the in-interview invitation bring in different types of people than the postal invitation, reducing the selectiveness of participants?
- (4) Does a sequential mixed-mode design, where sample members who do not use the app are offered a browser-based follow-up, increase participation?
- (5) How well do the participants gained with the browser-based follow-up adhere to the study protocol compared to those using the app?
- (6) Does the browser-based follow-up bring in different types of people, reducing the selectiveness of participants?

2. DATA

In SS2, we asked participants to download an app on their smartphone and use the app to report all their purchases for thirty-one days, by entering amounts and categories of spending. The data for SS2 were collected in May to December 2018 using two samples: (1) the *Understanding Society* IP and (2) the Lightspeed UK online access panel. The study was implemented by Kantar Public UK and the data and documentation are available from the UK Data Service ([University of Essex, Institute for Social and Economic Research 2022](#)).

In the following, we describe the data collection protocols for SS2, including how these differed for the two samples in which the study was

implemented; we document the two surveys and how the analysis samples were selected; and we document the covariates used in our analyses.

2.1 Protocols for SS2 That Were the Same in Both Surveys

The design of the app was based on findings from in-depth qualitative interviews that explored how we could best help participants remember their daily spending (Suffield, Hasbrouck, Coulter, Jäckle, Burton, et al. 2018). The app was programmed by Kantar Public UK using the survey app platform QMob (<https://www.qmobme.com/>) and was compatible with iOS smartphones and Android smartphones and tablets. In one section of the app, participants were asked to enter their daily purchases by first selecting a category of spending and then entering the total value of the purchase or by reporting that they had not made any purchases that day. In another section, participants were asked to enter all direct debit payments and standing orders that would come out of their accounts within 31 days. Sample members who did not use the app were invited to use a browser-based version of the spending diary instead. The design and functionality of this online diary mirrored that of the app. For documentation of SS2, including screenshots of the app and online diary (see Jäckle, Burton, Wenz, and Read 2019; Jäckle, Burton, Wenz, Read, Hanson, et al. 2019).

All participants received a weekly email summarizing the incentives they had earned so far and encouraging them to continue participating (see the flowcharts in Appendices A.1 and A.2 in the [Supplementary Materials online](#) summarizing the study protocols). In addition, app users received a daily push notification sent out at 8 pm from within the app, reminding them to report their purchases for that day. Online diary users were not sent daily reminders, as customized emails to only those who had not logged in online by 8 pm were not possible and emailing everyone daily seemed excessive. The app and online diary implemented in the IP and access panel were the same, but there were some differences in fieldwork protocols between the two samples in respondent incentives, whether respondents were sent reminders to use the app, and how they were invited to the browser-based follow-up. At the end of the study participants and nonparticipants from both samples were sent debrief questionnaires about their experiences with SS2.

2.2 Protocols for SS2 in the IP

In the IP, respondents were promised £1 if they completed the direct debit/standing order section, £0.50 per day on which they used the diary (whether to report purchases or a day without spending), a £10 bonus if they used the diary every day, and £3 if they completed a short debrief questionnaire at the end of the study. The incentives added up to a maximum of £29.50 and were sent to

participants by post at the end of the study, in the form of Love2Shop gift cards that can be used in many high street shops.

The IP11 interview included an experiment with the mode of interview: a random subset of households were issued to face-to-face interviewers (with nonrespondents followed up online), the rest were issued to web first (with nonrespondents followed up by face-to-face interviewers). For more details of the IP11 design and fieldwork see the user manual ([Institute for Social and Economic Research 2021](#)).

The implementation of SS2 in the IP included an experiment whereby a random half of the sample were invited to download the app within the annual interview. The other half were sent an invitation letter a couple of weeks after their interview. The treatments were allocated at the household level, so that all members of a household were treated in the same way, and stratified by allocation to mode of interview (see the flowchart documenting selection of the IP analysis sample in [Appendix A.3](#) in the [Supplementary Materials online](#)). The wording of the in-interview invitation to SS2 can be found in [Appendix A.4](#) in the [Supplementary Materials online](#). The letter for the postal invitation group can be found in [Appendix A.6](#) in the [Supplementary Materials online](#).

Respondents who did not use the app were sent up to two emails reminding them to download and use the app (see [Appendix A.1](#) in the [Supplementary Materials online](#)). One week after the second reminder, respondents were sent a letter invitation to the browser-based diary as an alternative way of participating in the study; an email invitation was also sent a day later.

Interviewers were informed about SS2 during in-person briefings, where they were given the opportunity to install and try out the app themselves. The interviewer project instruction manual included information on what respondent were being asked to do for SS2 and guidance on how to help respondents find, install and log in to the app. In addition, the showcards used by interviewers included several screenshots from the app that showed how to report daily purchases, how to report direct debits, and how to access the FAQs.¹

2.3 Protocols for SS2 in the Lightspeed Online Access Panel

In the Lightspeed online access panel, the incentive scheme was in line with standard Lightspeed rewards policy: panelist could earn a maximum of 500 points (equivalent to about £5) and could exchange their incentives for vouchers or charity donations.

The implementation on the Lightspeed sample included a randomized experiment whereby part of the sample were offered feedback about their personal spending ([Jäckle, Burton, Wenz, Read, Hanson, et al. 2019](#); [Wenz et al. 2020](#)). However, since the offer of feedback did not affect participation rates

1. See Section 7.3 in the Interviewer Materials document for wave 11, available at: <https://www.understandingsociety.ac.uk/documentation/innovation-panel/fieldwork-documents>.

or sample composition, for the purposes of this paper, we combine the feedback treatment groups in the analyses presented here.

Panelists were sent a baseline survey collecting background information on socio-demographic characteristics, mobile device access and usage and financial behaviors, mirroring content of the IP11 questionnaire. At the end of this baseline survey, respondents were invited to SS2 and asked to download the app (see [Appendix A.5](#) in the [Supplementary Materials online](#) for the wording of the invitation). Respondents who indicated that they had successfully downloaded and logged into the app reached the end of the baseline survey. Respondents who reported that they had tried but not succeeded, or not tried, were asked what prevented them or why they did not try to download the app. They were then immediately told about the option of using the online spending diary instead of the app (see [Appendix A.2](#) in the [Supplementary Materials online](#)) and routed directly into the browser-based diary. Note that this protocol was different from the IP, where those who did not use the app were sent two reminders to download it before being invited to the browser-based alternative.

2.4 Sample Selection: IP

The IP is part of *Understanding Society*: the UK Household Longitudinal Study and used as a platform for methodological testing and experimentation ([University of Essex, Institute for Social and Economic Research 2021](#)). It is a clustered and stratified probability sample of households in Great Britain that interviews all adult household members aged 16+ annually (see [Lynn 2009](#) for details of the sample design). The 2018 (wave 11) interview was used as a baseline survey and all respondents were invited to SS2. In the continuing sample 73.2 percent of households responded to the survey and 80.5 percent of individuals within those households. In the IP11 refreshment sample 24.4 percent of households and 73.6 percent of individuals in those households responded (AAPOR RR5; [The American Association for Public Opinion Research 2016](#)).²

[Appendix A.3](#) in the [Supplementary Materials online](#) documents the selection of the IP analysis sample, starting with the randomized allocation of households to the treatments for the mode of interview experiment and the SS2 invitation experiment. There were 90 respondents (from 89 sample households) who had a low predicted probability of completing the IP11 survey online. These households were not randomly allocated to data collection mode, but instead all allocated to CAPI-first (Computer Assisted Personal Interviews). Since part of the analyses for this paper relies on the randomized allocation to interview mode, we drop these cases from our analysis sample. In addition, we drop one respondent who completed the

2. See Tables 13.24 and 13.25 in the Innovation Panel User Manual ([Institute for Social and Economic Research 2021](#)).

survey by telephone, ninety-four proxy respondents, and 258 respondents from a reserve refreshment sample for whom the logins for SS2 were erroneously not activated. The resulting analysis sample includes 2,547 IP11 respondents invited to SS2. Six participants who used both the app and the online diary were classified as either app or online diary users according to which they used more frequently. Of the 2,547 respondents invited to SS2, 1,798 respondents (70.6 percent) were previously invited to SS1 but did not participate, 232 respondents (9.1 percent) participated in SS1, and 517 respondents (20.3 percent) were not invited to SS1 since they are part of the IP11 refreshment sample. The randomization to SS2 experimental conditions was independent of SS1 participation.

A randomization check using χ^2 tests shows that there is a slight imbalance between respondents allocated to the two invitation treatment groups at the 5 percent level in terms of education, but no differences in terms of age, gender, whether in work, region of residence, frequency of using the internet, and smartphone ownership.

Table 1 documents the mode allocations and outcomes. Overall, 35.5 percent of the sample was allocated to CAPI-first and 64.5 percent to web-first. Nearly all respondents allocated to CAPI-first completed the survey with an interviewer (93.4 percent), the rest completed it online. Three-quarters of those allocated to web-first completed the survey online, the remaining quarter completed it with a CAPI interviewer. A randomization check using χ^2 tests shows that there is a slight difference between respondents allocated to the two mode treatment groups at the 5 percent level in terms of age and frequency of internet use, but no difference in terms of gender, education, whether in work, region of residence, and smartphone ownership.

2.5 Sample Selection: Lightspeed UK Online Access Panel

Lightspeed panelists are recruited in various ways, such as banners and pop-ups on websites where individuals can sign up to the panel. The baseline

Table 1. Mode of Interview Allocations and Outcomes (IP Wave 11)

Mode of interview	Mode allocation			
	CAPI-first		Web-first	
	<i>N</i>	%	<i>N</i>	%
CAPI	843	93.4	418	25.4
Web	60	6.6	1,226	74.6
Total	903	100.0	1,644	100.0

survey was sent to a sample of 57,096 panelists stratified by age, gender, and region. The survey was completed by 2,878 panelists who were all invited to SS2. This represents a participation rate ([The American Association for Public Opinion Research 2016](#)) of 5.0 percent. Two participants used both the app and the online diary and were classified as app users, since they used the app more frequently than the online diary.

2.6 Covariates: Respondent Characteristics

To examine participation bias (RQ3, RQ6) we use data from the IP11 interview and the access panel baseline survey, collected of everyone invited to SS2. The questions in the access panel baseline survey largely mirrored those from IP11 and the question wording is documented in [Appendix A.7](#) in the [Supplementary Materials online](#). The indicators we use include:

- Socio-demographic characteristics: gender, age, educational qualifications, and whether they did any paid work in the past week.
- Intensity of mobile device usage: measured by questions about which activities the respondent does on their smartphone.³ The indicator is coded as 0, 1–9, and 10–12 for the number of activities done, and the zero category includes respondents who did not have a smartphone.
- Financial behaviors: including how frequently the respondent checks their bank balance (coded as daily, once a week, or less frequently), and whether they keep a budget.
- Spending in the last seven days: derived from questions asking how much the respondent had spent in the last seven days on a comprehensive list of ten categories of spending and coded into spending quartiles.⁴

The spending questions were not answered by 9.3 percent of respondents in the IP and by 3.9 percent in the access panel. For these variables the analyses in [tables 5](#) and [7](#) are based on complete cases. For all other indicators the rate of missingness was <1 percent, with the exception of the IP variables on education, frequency of checking bank balance and budgeting which were missing for around 3 percent of respondents. For all variables, other than spending, missing observations are set to the modal categories in the corresponding sample.

3. The activities included browsing websites, email, taking photos, looking at content on social media websites/apps, posting content to social media websites/apps, making purchases, online banking, installing new apps, using GPS/location-aware apps, connecting to other electronic devices via Bluetooth, playing games, and streaming videos or music.

4. We replicated the analysis by coding spending in the last seven days into deciles but reached the same conclusion.

3. METHODS

The analyses presented below use different subsets of the data, depending on the research question. We use data from the IP sample to examine the effects of the invitation experiment (RQ1 to RQ3); data from both the IP and the access panel to examine the effectiveness of the sequential app and browser-based design (RQ4 and RQ5); and data from the access panel only (as there are too few users of the browser-based diary in the IP sample) to examine differences between app and browser-based diary (RQ6).

Depending on the research question we use different statistical analyses: χ^2 tests for each of the research questions, plus tests of means for RQ5, graphical representations for RQ1 and RQ5, and instrumental variable regressions for RQ2. We provide more information on the analysis methods in context when describing the results.

As the IP and the access panel differ in the composition of their samples and in the experiences the sample members have with their panel, we do not attempt to draw comparisons between the two in terms of completion rates. Instead, we focus on the effects of our protocols within the samples. All analyses are unweighted.

4. RESULTS

4.1 RQ1: Does Introducing the App Within an Interview Increase Participation Compared to Sending a Separate Invitation by Post?

Introducing the app as part of the annual IP interview significantly increased participation (table 2): 22.6 percent of respondents invited within the interview went on to use the app at least once to report a purchase compared to only 12.4 percent of respondents invited by post (a treatment effect of 10.2 percentage points). To check that these results are not affected by the prior SS1, we also examine the effects of the invitation experiment by participation in SS1. Among the 2,315 IP11 respondents who had not participated in SS1, the effect of the in-interview treatment is the same as in the full sample, increasing use of the app by 10.3 percent points compared to the postal invitation. However, among the 232 IP11 respondents who had participated in SS1, the invitation experiment had no effect: in both the in-interview and the postal invitation group, 59.1 percent of respondents used the app. Since this was only a small group, this did, however, not attenuate the treatment effect estimated on the full sample (table 2).

The further results in table 2, however, suggest that the additional participants brought in with the in-interview invitation were less committed and more likely to drop out: in the in-interview invitation group 65.0 percent of those who used the app at least once went on to use it in each of the four weeks,

Table 2. Participation Rates by Invitation Treatment and Participation in SS1 (IP)

	Invitation: In-interview		Invitation: Post		<i>p</i> -value
	<i>N</i>	%	<i>N</i>	%	
<i>Total</i>					
Completed IP11 interview	1,253	100.0	1,294	100.0	
Entered at least one purchase in app	283	22.6	160	12.4	<.001
Entered at least one purchase in each of the 4 weeks	184	14.7	118	9.1	<.001
<i>Participated in SS1</i>					
Completed IP11 interview	127	100.0	105	100.0	
Entered at least one purchase in app	75	59.1	62	59.1	.999
<i>Did not participate in SS1</i>					
Completed IP11 interview	1,126	100.0	1,189	100.0	
Entered at least one purchase in app	208	18.5	98	8.2	<.001

NOTE.—*p*-values from χ^2 tests.

compared to 73.8 percent in the postal invitation group. Nonetheless, the net effect of the in-interview invitation was positive: the percentage of respondents who used the app at least once in each of the four weeks remained significantly higher with the in-interview invitation (14.7 percent) than the postal invitation (9.1 percent).

In both invitation modes, respondents who did not use the app were sent up to two weekly reminder emails to encourage app participation. To evaluate the effectiveness of these reminders, we examine how many days after the interview the first purchase was recorded in the app. Figure 1 shows the first day of app use by invitation treatment for those who entered at least one purchase in the app during the 31 days after their interview. The results suggest that the reminders did not have the intended effect: in the in-interview invitation treatment, the large majority of app users entered their first purchase on the day of the interview (72.7 percent); in the postal invitation treatment, the first day of app use was relatively spread out in the weeks following the interview, without any major peaks on particular days.

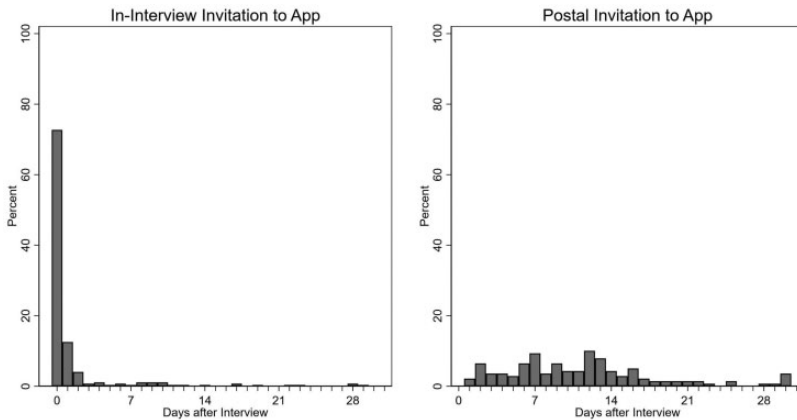


Figure 1. First Day of App Use by Invitation Treatment (in Days After Interview; IP).

4.2 RQ2: Does the Effect of Introducing the App Within the Interview Vary With the Mode of Interview?

Respondents who completed the IP11 interview online were more likely to own smartphones than CAPI respondents (82.2 percent compared to 78.4 percent, χ^2 test p -value = .017). Within IP11 interview modes, there were, however, no differences between the invitation treatment groups in smartphone ownership, as would be expected given the randomized assignment to treatments (table 3).

Among web respondents the invitation treatment had no effect: 24.9 percent of smartphone owners in the in-interview invitation group and 20.8 percent in the postal invitation group used the app at least once to report a purchase ($p = .116$). This result holds when all respondents, including those without smartphones, are included in the analysis (table 3, row 3).

Among CAPI respondents, however, the invitation treatment had a large effect. In the in-interview invitation group, 29.5 percent of smartphone users used the app at least once, compared to just 7.8 percent in the postal invitation group (an increase of 21.7 percentage points, $p < .001$). When all respondents, including those without smartphones, are included in the analysis, this result again holds, with the in-interview invitation increasing app use by 17.4 percentage points ($p < .001$).

These findings suggest that the in-interview invitation was very effective if the interview was carried out by an interviewer, but not if the respondent completed the survey online. However, these analyses do not account for self-selection of respondents into the mode of interview: not all respondents completed the survey in the mode to which they were randomly allocated. It is possible that respondents who completed the IP survey online were types of

Table 3. Smartphone Coverage and Participation Rates (IP)

Mode of interview	CAPI				Web			
	In-Int.	Post	Δ	<i>p</i> -value	In-Int.	Post	Δ	<i>p</i> -value
Invitation treatment								
Has smartphone	79.1	77.8	1.3	.587	82.5	81.9	0.6	.765
Used app (cond.) ¹	29.5	7.8	21.7	<.001	24.9	20.8	4.1	.116
Used app	23.8	6.4	17.4	<.001	21.4	18.0	3.4	.125
<i>N</i>	635	626			618	668		
Mode allocation								
Invitation treatment								
Has smartphone	84.1	80.0	4.2	.101	78.9	79.9	-1.0	.605
Used app (cond.) ^a	33.0	10.9	22.1	<.001	23.7	16.7	6.9	.002
Used app	28.4	9.1	19.3	<.001	19.3	14.1	5.2	.005
<i>N</i>	454	449			799	845		

NOTE.—In-Int. = In-interview invitation. Δ = Percentage point difference between prior two columns. Cond. = conditional on smartphone ownership.

^aThis row excludes 5 CAPI respondents and 11 web respondents who said they did not own a smartphone but did use the app. *p*-values from χ^2 tests.

Table 4. Predicted Probabilities of Using the App (IP)

Mode of interview	CAPI			Web		
	In-Int.	Post	Δ	In-Int.	Post	Δ
Invitation treatment						
Used app	29.4	8.7	20.7	15.6 ^A	15.8 ^A	-0.2

NOTE.—*N* = 2,547. Predicted probabilities estimated from instrumental variable regression. Predictions sharing a letter are not significantly different. All other pairwise comparisons are significant at the 5 percent level. Δ = Percentage point difference between prior two columns.

people who were more likely to participate in the app study without additional encouragement.

The lower half of [table 3](#) replicates the previous analyses by interview mode allocation (Intention-to-Treat analysis). For both invitation treatment groups, the participation rates are lower in the web-first group than among actual web respondents, and higher in the CAPI-first group than among actual CAPI respondents. This pattern suggests that there are selection effects: respondents who completed the survey online were people who were more likely to participate in SS2. The Intention-to-Treat analysis, however, does not allow us to draw conclusions about whether the in-interview invitation treatment is more or less effective when implemented in a CAPI or a web interview.

[Table 4](#) provides estimates of the Local Average Treatment Effect, that is, the effect for those who would complete the survey in a different mode, if their mode allocation was switched. The predicted probabilities are based on a two-stage least squares regression of the probability of using the app at least once regressed on the invitation treatment, the mode of interview, and the interaction between the two. The mode of interview and the interaction are treated as endogenous variables and instrumented with the randomized mode allocation and the interaction of the mode allocation and the invitation allocation ([Greenland 2000](#)).

The results suggest that if respondents complete the IP survey online, they are equally likely to participate in a follow-on self-completion task regardless of whether they were invited to that within the web survey or by post. In contrast, if respondents complete the IP interview with an interviewer, they are unlikely to respond to the postal invitation to use the app (8.7 percent) and more than three times as likely to use the app if they are invited to download it while the interviewer is present (29.4 percent).

Table 5. Participation Bias by Invitation Treatment Group (IP)

	Invitation: in-interview				Invitation: post			
	Full sample		App users—full sample		Full sample		App users—full sample	
	N	Col%	% points	p-value	N	Col %	% points	p-value
Female	690	55.1	2.9	.268	710	54.9	2.0	.586
Age 16–35	293	23.4	13.4		304	23.5	12.1	
Age 36–50	293	23.4	9.1		309	23.9	-2.6	
Age 51–65	371	29.6	-4.9		380	29.4	3.1	
Age 66+	296	23.6	-17.6	<.001	301	23.3	-12.6	<.001
Degree	465	37.1	7.8		561	43.4	3.5	
A/AS levels	181	14.4	3.2		169	13.1	2.6	
GCSE/CSE level	412	32.9	-2.1		355	27.4	0.1	
No educational qualification	195	15.6	-8.8	<.001	209	16.2	-6.2	.126
In work	709	56.6	18.0	<.001	732	56.6	12.8	.001
Activities on smartphone: 0	315	25.1	-19.8		358	27.7	-20.2	
Activities on smartphone: 1–9	486	38.8	-3.5		481	37.2	1.6	
Activities on smartphone: 10–12	452	36.1	23.3	<.001	455	35.2	18.6	<.001
Checks bank balance: most days	285	22.7	9.1		258	19.9	12.6	
At least once a week	477	38.1	4.0		496	38.3	2.3	
Less often	491	39.2	-13.0	<.001	540	41.7	-14.9	<.001
Does not keep a budget	773	61.7	-3.7	.141	779	60.2	-1.5	.689
Spending quartile 1	289	25.4	-2.4		288	24.6	-1.6	
Spending quartile 2	293	25.8	0.5		284	24.3	1.4	
Spending quartile 3	283	24.9	-0.8		295	25.2	1.1	
Spending quartile 4	272	23.9	2.7	.557	303	25.9	-0.9	.927
Average absolute bias			8.1				6.4	

NOTE.—*d*-values from χ^2 tests.

4.3 RQ3: Does the In-Interview Invitation Bring in Different Types of People Than the Postal Invitation, Reducing the Selectiveness of Participants?

Table 5 examines participation bias: the extent and nature of differences between those who did and those who did not use the SS2 app to report purchases—and how this varies between the invitation treatment groups. The first two columns show the characteristics of all IP11 respondents in the in-interview invitation group. The third column shows how app users differ from that full sample. The numbers give the percentage point difference in the proportion of app users with that characteristic compared to the full sample. For example, 23.6 percent of respondents in the full sample were aged 66 and above, but among app users the proportion in that age group was only 6.0 percent, so 17.6 percentage points lower. Column 4 reports p -values from χ^2 tests for differences between app users and those who did not use the app. Columns 4–8 repeat this analysis for the postal invitation group. The final row summarizes the absolute average bias for the two invitation treatment groups, calculated as the sum of the absolute percentage point differences in the column above, divided by the number of rows.

The results suggest that the invitation experiment did not affect the nature or extent of participation bias. In both treatment groups, there are differences in the socio-demographic characteristics of participants and nonparticipants: while gender is balanced, those who are younger, have higher educational qualifications, or are in work are significantly over-represented among participants. Intense smartphone users and those who frequently check their bank balance are also over-represented in both invitation treatment groups. That is, there are clear differences in socio-demographic characteristics, mobile device usage, and financial behaviors that are related to participation. There does, however, not appear to be any bias in terms of what the app was designed to measure: examining the spending respondents reported in the IP11 interview shows no differences between participants and nonparticipants, whether the spending is grouped into quartiles (see **table 5**) or percentiles (data not shown).

4.4 RQ4: Does a Sequential Mixed-Mode Design, Where Sample Members Who Do Not Use the App Are Offered a Browser-Based Follow-Up, Increase Participation?

Having examined the effects of how sample members are invited to the app study, we next examine the effects of the browser-based follow-up that was offered to those who did not use the app. For this purpose, we use both the IP and the Lightspeed UK access panel data.

Table 6 documents participation rates at different stages, in both the access panel and the IP. Respondents who were invited to SS2 within the baseline

Table 6. Participation Rates at Different Stages (Access Panel and IP)

	Access panel		IP: in-interview		IP: postal		IP: all	
	N	%	N	%	N	%	N	%
Completed baseline survey	2,878	100.0	1,253	100.0	1,294	100.0	2,547	100.0
Self-report: downloaded and logged in	696	24.2	328	26.2	—	—	—	—
Accessed app at least once	427	14.8	301	24.0	162	12.5	463	18.2
... entered 1+ purchase	408	14.2	283	22.6	160	12.4	443	17.4
Accessed OD at least once	1,213	42.2	34	2.7	53	4.1	87	3.4
... entered 1+ purchase	657	22.8	29	2.3	50	3.9	79	3.1
1+ purchase in app or OD	1,065	37.0	312	24.9	210	16.2	522	20.5

NOTE.—OD, online diary; IP, innovation panel.

interview⁵ were immediately asked to report whether they had tried to download the app. In both samples about a quarter confirmed that they had successfully downloaded and logged into the app. It was not possible to identify who had actually downloaded the app, therefore, we next examine who actually used the app. In the IP, most people who self-reported that they had logged into the app during the interview did actually use it at least once. This is true both for web and CAPI respondents: within the in-interview invitation group, there is no difference between modes in the percentage who used the app at least once (χ^2 test $p = .263$). In contrast, in the access panel there was a large drop from 24.2 percent who downloaded and logged into the app during the baseline survey, to 14.8 percent who used the app at least once. In all samples and groups, nearly everyone who opened the app entered at least one purchase.

In the access panel, 42.2 percent of respondents opened the online diary at least once but only 22.8 percent entered at least one purchase. Nonetheless, this increased the participation rate from 14.2 percent with only the app, to 37.0 percent overall. In the IP, most respondents who opened the online diary did enter at least one purchase, but the rates were extremely low, only 3.1 percent overall. Across both samples, the majority of participants who entered at least one purchase in the online diary used a desktop PC or laptop (access panel: 66.5 percent; IP: 63.3 percent) or a tablet (access panel: 26.5 percent; IP: 29.1 percent), whereas only a small fraction completed the online diary on a smartphone (access panel: 7.0 percent; IP: 7.6 percent).

These results suggest that the sequential design can more than double participation rates, but seemingly only if the alternative is also offered within the interview, as soon as respondents decline the app. We interpret these results with caution, since they come from two very different samples and so the differences in outcomes could be due to factors other than the protocols we controlled.

4.5 RQ5: How Well Do the Participants Gained with the Browser-Based Follow-Up Adhere to the Study Protocol Compared to Those Using the App?

Figure 2 illustrates the patterns of participation over the thirty-one days, using the access panel and IP data. The graphs are based on participants who reported at least one purchase. For each participant, day 1 is the day on which they first used the app or online diary. The solid lines show the proportion of participants who used the diary on a given study day to enter a purchase, direct debit, standing order, or to report a no spend day. The dashed lines show the proportion who remained in the study and used the diary at least once on a later day.

Those who used the app maintained their participation during the study period: about 80 percent remained in the study past day 25 in both samples,

5. This includes all access panel respondents and the IP respondents in the in-interview invitation group, and excludes the IP respondents in the postal invitation group.

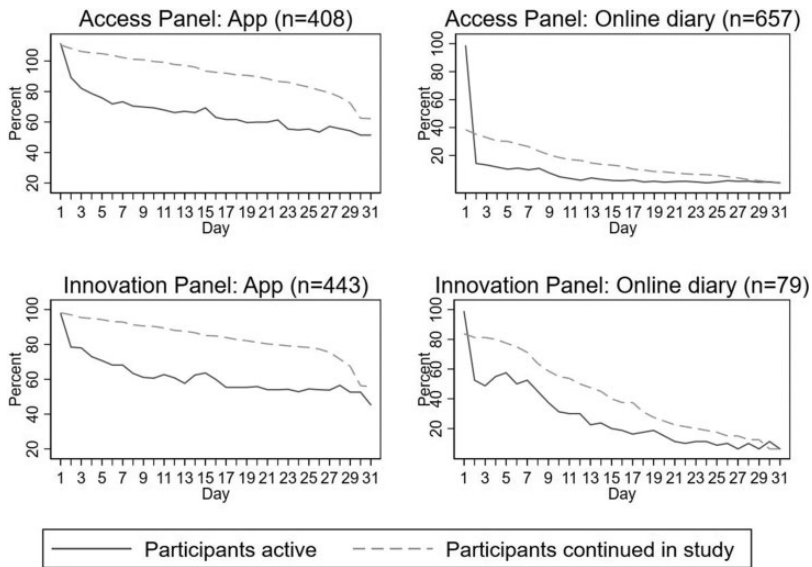


Figure 2. Participation Over the Month.

although they did not use the app every day. In contrast, those who used the online diary did not maintain their participation over the month. In the access panel, only 40 percent of online diary users used it on a second day and by day 25 only about 10 percent remained in the study. In the IP, the initial dropout was less steep, with about 80 percent still in the study by day 5. However, by day 25 fewer than 20 percent remained in the study.

Examining the first day on which each participant used the diary suggests that, on day 1 at least, the reporting behaviors of those who used the online diary were no worse than those of app users. In the IP, 46.8 percent of online diary users and 40.6 percent of app users entered information about direct debits and standing orders, around 77 percent reported at least one purchase, and those reporting purchases made on average about 1.5 entries ($p > .3$ for all comparisons between IP app and online diary users). In the access panel, a higher proportion of online diary users reported direct debits or standing orders (58.8 percent compared to 46.3 percent of app users, $p < .001$), a higher proportion reported at least one purchase (87.7 percent compared to 77.4 percent of app users, $p < .001$), and the mean number of purchases reported was similar (1.5 compared to 1.9 among app users, $p = .063$).

These results suggest that the online diary and app might produce similar reporting behaviors and complement earlier research that we conducted as part of SS1, showing that the distribution of spending recorded in a receipt scanning app is comparable to benchmark data from the Living Costs and Food Survey (Wenz, Jäckle, Burton, Couper, and Read 2021). The main challenge is

how to maintain daily participation in the online diary. It seems that the weekly email that diary participants received, in contrast to the daily push message for the app participants, was not enough to sustain daily participation.

In the debrief questionnaire at the end of the study, we asked app users about their experience with the daily reminders sent through the app. Of the 259 IP respondents and 106 access panel respondents who completed the debrief survey, a quarter (24.3 percent and 22.6 percent, respectively) indicated that they turned the push notifications off during the study period or did not receive the notifications. Almost all respondents who kept the push notifications on confirmed that the reminders were very or somewhat useful (IP: 95.4 percent; access panel: 98.8 percent), corroborating the effectiveness of daily app reminders in sustaining participation.

4.6 RQ6: Does the Browser-Based Follow-Up Bring in Different Types of People, Reducing the Selectiveness of Participants?

Table 7 examines differences between participants and nonparticipants, and how the extent and nature of bias changes when those who used the online diary are added to the participant pool. The analyses are based on the access panel data only, since the number of online diary users in the IP is too small to examine changes in sample composition. Columns 1 and 2 show the characteristics of respondents who completed the baseline questionnaire. For each characteristic, column 3 documents the percentage point difference in the proportion of app users with that characteristic. Column 4 reports p -values from χ^2 tests of differences between app users and those who did not use the app. Columns 5 and 6 repeat this analysis, comparing those who used either the app or the online diary with those who did not participate in either way.

The selectiveness of app users compared to all baseline respondents mirrors the pattern in the IP (**table 5**). Once online diary users are added to the participant pool, the extent of bias decreases: the average absolute bias for the variables examined more than halves, from 6.4 to 2.8. However, the biases remain significant. That is, offering the browser-based alternative reduces but does not eliminate participation bias.

5. DISCUSSION AND CONCLUSIONS

While mobile apps are an attractive way of collecting data, participation in app-based data collection in the general population is still rather low. This paper provides novel evidence on how protocols used to implement app-based data collection can affect participation rates and biases. Using experimental data, we show that inviting respondents to the app-based task within a CAPI interview more than triples participation rates compared to sending an invitation letter by post, but does not bring in new types of participants. In contrast,

Table 7. Participation Bias of App Users and Combined App and Online Diary Users (Access Panel)

	Full sample		App users—full sample		App/OD users—full sample	
	<i>N</i>	Col %	% points	<i>p</i> -Value	% points	<i>p</i> -Value
Female	1,998	69.4	3.1	.139	1.5	.190
Age 16–35	986	34.3	1.3		1.1	
Age 36–50	777	27.0	10.3		4.4	
Age 51+	1,115	38.7	-11.5	<.001	-5.5	<.001
Degree	1,205	41.9	3.7		2.0	
A/AS levels	740	25.7	2.7		1.0	
GCSE/CSE level	795	27.6	-4.8		-1.6	
No educational qualification	138	4.8	-1.6	.023	-1.3	.019
In work	1,901	66.1	7.7	<.001	3.2	.005
Activities on smartphone: 0	458	15.9	-14.2		-4.6	
Activities on smartphone: 1–9	1,268	44.1	-5.3		-1.6	
Activities on smartphone: 10–12	1,152	40.0	19.5	<.001	6.3	<.001
Checks bank balance: most days	945	32.8	12.0		4.0	
At least once a week	1,165	40.5	-0.3		1.8	
Less often	768	26.7	-11.7	<.001	-5.7	<.001
Does not keep a budget	588	20.4	3.8	.038	0.2	.817
Spending quartile 1	692	25.0	-3.3		-3.2	
Spending quartile 2	712	25.7	-3.8		-1.5	
Spending quartile 3	675	24.4	3.6		1.3	
Spending quartile 4	688	24.9	3.6	.023	3.4	.001
Average absolute bias			6.4		2.8	

NOTE.—*p*-Values from χ^2 tests.
OD, online diary.

inviting respondents to download the app within a web survey offers no advantage in terms of rates of participation over sending the invitation by post after the web survey; however, doing so may save some money.

On the one hand, the presence of an interviewer in persuading survey respondents to participate in the app-based study and assisting them with the process of downloading the app and logging in has clear advantages. On the other hand, interviewer administration is costly and time-consuming. *Understanding Society*, like many other panel studies, is increasingly pushing panel members to complete surveys online. Understanding what role the interviewers play in this process and trying to replicate that role in self-administered modes remains a key challenge for broader adoption of app-based tools in large-scale surveys of the general population. Similarly, understanding the barriers to participation among panel members asked to take on this extra task is an important step in increasing participation rates.

Offering a browser-based alternative to those who do not use the app can more than double participation rates. However, this appears to only be effective if the invitation to the app study is embedded in a survey and if the alternative is offered as soon as the app is declined. The browser-based alternative brings different types of people into the participant pool, reducing nonparticipation bias. We speculate that the immediate offer of a potentially less demanding alternative can persuade participants to give it a try.

A key challenge with the browser-based alternative, however, is how to maintain participant engagement throughout the study period: while the app can be set to push out daily reminders, it is less clear how to implement comparable reminders for a browser-based instrument. Bringing in the extra participants initially does not lead to sustained participation. Apps and browser-based approaches have different strengths and weaknesses. Finding the best ways to exploit these and offering a solution that best suits the respondents' preferences may increase both initial participation and ongoing adherence in the study.

Our paper has several limitations. Most notably, we were unable to replicate all experimental manipulations in both the IP and the access panel. Online access panels by definition do not use interviewer-administered modes. Testing the effect of the in-interview request in other studies is important. The differential effect of the online diary follow-up in the two samples also needs further exploration. A key strength of our paper is testing experimental protocols in two very different samples.

The take home message is that the success of using mobile apps for data collection hinges on the protocols used to implement the app, such as protocols for invitations and protocols for following up nonparticipants. The successful implementation of app-based protocols in large population-based surveys needs to overcome the challenges of recruitment and adherence for these methods to be effective. While our study provides some useful evidence, further research should focus on the optimal protocols for recruitment and follow-up to

maximize initial participation and ongoing adherence and reduce potential participation biases in app-based studies.

Supplementary Materials

Supplementary materials are available online at academic.oup.com/jssam.

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