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Effectiveness of web-based tailored smoking cessation advice reports (iQuit): a randomized trial

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ABSTRACT

Aims To determine whether web-based tailored cessation advice, based on social cognitive theory and the perspectives on change model, was more effective in aiding a quit attempt than broadly similar web-based advice that was not tailored. **Design** Participants were allocated randomly to one of two groups, to receive either a cessation advice report and progress report that were tailored to individual-level characteristics or a cessation advice report that presented standardized (non-tailored) content. Tailoring was based on smoking-related beliefs, personal characteristics and smoking patterns, self-efficacy and outcome expectations. **Setting** Participant enrolment and baseline assessments were conducted remotely online via the study website, with the advice reports presented by the same website. **Participants** Participants ($n = 1758$) were visitors to the QUIT website who were based in the United Kingdom, aged 18 years or over and who smoked cigarettes or hand-rolled tobacco. **Measurements** Follow-up assessments were made at 6 months by telephone interview. The primary outcome measure was self-reported 3 months prolonged abstinence, and secondary outcomes were 1 month prolonged abstinence, 7-day and 24-hour point prevalence abstinence. **Findings** The intervention group did not differ from the control group on the primary outcome (9.1% versus 9.3%; odds ratio = 1.02 95% confidence interval 0.73–1.42) or on any of the secondary outcomes. Intervention participants gave more positive evaluations of the materials than control participants. **Conclusions** A web-based intervention that tailored content according to smoking-related beliefs, personal characteristics and smoking patterns, self-efficacy and outcome expectations, was not more effective than web-based materials presenting broadly similar non-tailored information.

Keywords E-health, internet intervention, online support, self-help, smoking cessation, tailored interventions, tailoring, web-based intervention.

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INTRODUCTION

Smokers attempting to quit can benefit from individual counselling [1], but counselling can only reach a minority, and most smokers prefer less intensive approaches [2]. Conversely, written self-help materials can be disseminated to large numbers of individuals, and there is evidence that they can help smokers to quit in an otherwise unaided manner [3]. One way of combining the individualized nature of counselling with the wide reach of written self-help materials is to use computer technology to tailor the written materials to the individual smoker's characteristics. When self-help materials are tailored in this way they not only increase the number of quitters, but do so more than standardized self-help materials [3].

The first generation of computer-tailored interventions, in which the participant was sent a tailored report generated by and printed from a central university or clinic computer (e.g. [4]), has evolved into a new generation of web-based smoking cessation programmes [5]. Web-based tailored interventions have been shown to be just as effective as other computer-driven tailored interventions in helping smokers to quit [6]. The web has huge potential in this domain, as it permits extensive tailoring of content in real time, can be accessed by a large proportion of the smoking population and carries low ongoing financial costs due to the limited personnel required [7]. A further advantage of the web-based setting is that, at little extra cost, the participant can be provided with contact on multiple occasions to receive ongoing

feedback and support. Conversely, web-based interventions are not easily accessible for some sections of the population. Despite trends towards widening access, internet users tend to be younger, better educated and of higher social grades [8].

There is evidence that web-based interventions that are tailored might be more effective than those that are static and standardized [9]. Given the socio-demographic gradient in internet access, the effectiveness of the tailoring component among people who seek support online is more specifically isolated if the medium of delivery is controlled. A recent meta-analysis found only two studies that compared web-based tailored and non-tailored versions while controlling delivery medium and website content [10]. These studies specifically examined effectiveness of the tailored component of a web-based intervention delivered as a supplement to nicotine replacement therapy [11,12]. In this paper we report the main findings from the iQuit trial, in which we hypothesized that a web-based tailored cessation advice report plus tailored progress report would yield a higher 3-month quit rate at 6 months than a web-based standardized cessation advice report among smokers who visited the UK charity-run QUIT website (http://www.quit.org.uk). The materials used are based closely on the tailored printed letters used by Sutton & Gilbert [4], who found the intervention was effective among baseline smokers but not among recent ex-smokers. They also found a marginally significant moderating effect of socio-economic deprivation whereby the intervention was more effective among more deprived smokers. We examine the moderating effect of baseline smoking status and deprivation on intervention effectiveness.

METHOD

Participants and design

Participants ($n = 1758$) were recruited from visitors to the QUIT website between November 2008 and May 2010. Participants had to be 18 years of age or over, based in the United Kingdom, and be cigarette or roll-up smokers. Smokers planning a quit attempt within the next 6 months and recent ex-smokers were included. Inclusion of recent ex-smokers allowed for a possible lag between initial registration and questionnaire completion for those registering on the day they quit or immediately following it. Figure 1 shows the progress of participants throughout the study.

Visitors to the QUIT website clicked a link to indicate interest in the study ($n = 2977$), upon which they registered an e-mail address and chose a username to identify themselves to the iQuit study website. A randomly generated password was sent to their email address to validate the address supplied. Upon logging back into the study website they read an information page, which explained that they would be asked to complete an online questionnaire and would then receive an advice report that was either tailored to their questionnaire answers or was general advice. If they consented to take part, they went on to complete the questionnaire and were then entered into the study ($n = 1758$; 59.1% of interested registrants).

Participants were allocated randomly to either (i) a control group who received a standardized advice report or (ii) an intervention group who received a tailored advice report plus an invitation 1 month later to complete a second online assessment and receive a tailored progress report. The advice and progress reports were delivered on screen with a 'print this page' button. Simultaneously, a copy of the report was sent to the participant's e-mail address. Presentation of the report was immediate upon completion of the questionnaire.

Allocation was randomized in blocks of 100 by one investigator (D.M.) using a computerized random number generator, so that half were allocated to the intervention group and half to the control. The sequence thus generated was stored in a database on the web server prior to the trial start. Participants enrolled themselves remotely online and the server performed allocation automatically. It implemented this by selecting successive codes from the sequence indicating control or intervention group membership. The code was only selected once the baseline questionnaire was submitted. Thus, although the allocation sequence was not concealed from the investigators, they could not influence the allocation of a particular participant. Allocation was concealed from the participant until the report was displayed.

Tailored advice report

The content of the messages that made up the tailored advice report was based on social cognitive theory [13] and the perspectives on change model [14], evidence of Quitline caller characteristics [15], input from the Quitline counsellors, and 'received wisdom' about smoking cessation. Detailed information about the theoretical basis and behaviour change techniques of the intervention are given in Appendix S1 (online Supporting information, see details given at the end). Tailoring variables included age, sex, previous quit attempts, reasons for quitting, dependence, motivation and determination to quit, self-image, advantages and disadvantages of quitting, tempting situations, living with children, living with other smokers, social support, pregnancy and health problems. Participants entering a proposed quit date that had already passed were classified as ex-smokers, and the

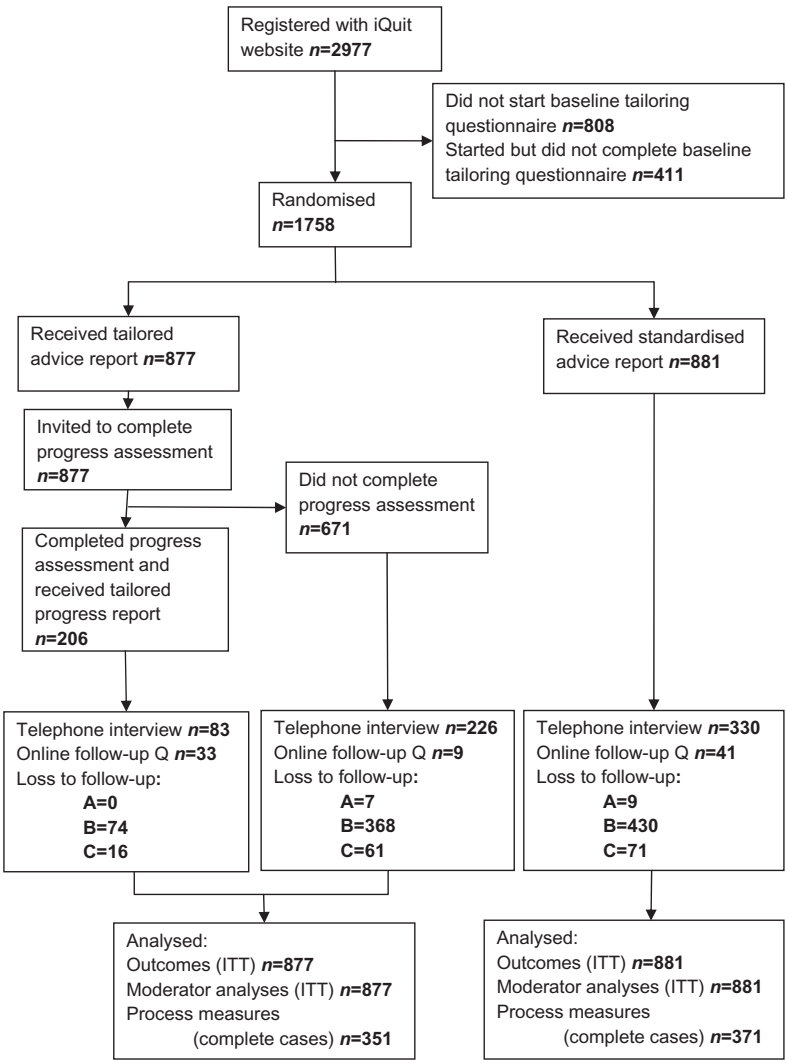

Figure 1 Flow of participants through the trial. Reasons for loss to follow-up: A = withdrew consent when contacted by telephone; B = did not provide a valid telephone number; C = could not contact within 10 attempts; ITT: intention to treat

messages they received were tailored accordingly. All participants entering a future quit date were classified as smokers. The report addressed the participant by name and stated that the content was based on their questionnaire responses. Its primary purpose was to encourage smokers who wished to quit on their own. However, participants were reminded that they could obtain support from the Quitline or from other local health professionals.

Tailored progress report

Intervention group participants were invited by e-mail to return to the website 4 weeks after receiving the tailored advice report to complete a progress assessment questionnaire. Tailoring variables for the progress report combined variables assessed previously at baseline (reasons for quitting, self-image, baseline dependence, living with other smokers) with variables assessed on the progress questionnaire (quit date, slip-ups in a quit attempt, change in self-image, change in dependence, situations provoking temptation, use of pharmacotherapy, perceived benefits of quitting). The purpose of the progress report was to encourage and prevent relapse in quitters, to reassess the expectations in non-quitters and analyse the relapse situation if applicable.

Standardized advice report

The standardized advice report was generated using the same tailoring algorithm and content as the tailored advice report, by entering missing values for those inputs where the tailoring algorithm could provide default content, and modal responses from a pilot study where inputs were required. The resultant text was edited slightly so as not to imply knowledge about the participant. The standardized advice report was identical for each control participant. It did not contain the participant's name and stated that it was not based on their answers to the questionnaire, but contained the best advice for 'most smokers'.

Outcome measures

Participants were contacted by telephone by a research interviewer 6 months after receiving the advice report. The interviewer was blind to the participant's group allocation when making initial contact, but the participant may have revealed information relevant to their allocation during the interview. Those who could not be contacted after 10 attempts were sent an e-mail inviting them to complete the follow-up questionnaire online.

The primary outcome measure was self-reported 3 months' prolonged abstinence at the 6-month follow-up. This allowed a 'grace period' between the intervention and the period of abstinence, which is appropriate for a 'cessation induction' trial [16]. Secondary outcomes were 1-month prolonged abstinence and 7-day and 24-hour point prevalence abstinence, all at 6 months. The primary outcome measure did not include biochemical verification and so did not meet the Russell Standard [17]. It has been recommended that biochemical verification is not necessary for trials that involve no face-to-face contact, present a low intensity intervention and where follow-up is conducted by telephone [18]. Furthermore, participants were distributed geographically throughout the United Kingdom, making it impracticable to visit them. Sending saliva kits by post was expected to yield a low response rate, and it would be possible for participants to modify their smoking behaviour between giving an interview and providing a sample by post.

Process measures

At the 6-month follow-up participants were asked whether they remembered receiving the report, assessed it on a variety of evaluative dimensions (e.g. readability, usefulness) and reported other sources of support they had used. Response to the progress assessment invitation provided a further process measure within the intervention group.

Sample size and statistical analysis

The original target sample size was 2224, giving 80% power to detect a difference in quit rate of 8% versus 5% (χ^2 test, two-tailed, alpha = 0.05). Recruitment was slower than expected, so the target sample size was revised down to 1800, giving 70% power to detect a difference of this size.

Outcomes were analysed with all randomized participants included in the analysis, and those not providing outcome data assumed to be smoking at follow-up. The binary outcome variables and categorical demographic and process measures were compared between groups using χ^2 tests. Continuous demographic and process measures were compared between groups using independent *t*-tests. Logistic regression was used to repeat the main analyses adjusting for baseline covariates thought to be predictive of successful cessation: age, sex, deprivation, dependence on cigarettes, intention to quit, motivation to quit, previous quit attempts and whether living with other smokers. Moderating effects of baseline smoking status and deprivation were examined by repeating the logistic regression analyses adding an interaction term for moderator × trial arm. The same method was used to examine predictors of attrition at 6 months. Sensitivity to missing outcomes assumptions was investigated by repeating the main effect analysis of the primary outcome under the assumption that outcomes were missing at random. All analyses used alpha = 0.05.

RESULTS

Baseline equivalence and attrition

The mean age of the sample was 37.8 [standard deviation (SD) 11.3] and 64.1% were female. Participants smoked on average 18.2 (SD = 8.7) cigarettes per day, with 68.3% saying they had their first cigarette of the day within 30 minutes of waking. They were strongly motivated to quit (Table 1). A minority (15.6%) were recent ex-smokers, indicated by reporting a quit date in the 2 weeks prior to baseline.

There were no significant differences in baseline characteristics between the intervention and control groups, with one exception. Dependence was higher in the control group than in the intervention group (mean 5.6 versus 5.4, respectively, $t(1756) = 2.1$, $P = 0.038$).

Follow-up response rate at 6 months was 40.0% (351 of 877) in the intervention group and 42.1% (371 of 881) in the control group, and these did not differ significantly (Fig. 1). Of the baseline characteristics, age and smoking status predicted attrition, with older participants [odds ratio (OR) = 1.02; 95% confidence interval (CI) 1.01–1.03] and baseline ex-smokers (OR = 1.58; 95% CI 1.09–2.30) more likely to respond to follow-up. Predictors of attrition were the same for the intervention and control group.

Outcome

The intervention group did not differ from the control group on self-reported abstinence at the 6-month follow-up, whether assessed on the primary outcome (prolonged 3-month abstinence) or any of the secondary outcomes (1 month, 7-day and 24-hour abstinence). The results did not change when adjusting for baseline characteristics (Table 2). There were no significant moderating effects of baseline smoking status or deprivation on the intervention effect for any of the four outcomes.

Table 1 Participant characteristics at baseline.

		Control	*Intervention*
n		881	877
% Female		63.5%	64.7%
Mean (SD) age		37.8 (11.2)	37.8 (11.4)
Mean (SD) deprivation (0–5)[a]		1.2 (1.1)	1.2 (1.1)
% Rented home		52.9%	51.4%
% Own no cars		19.4%	17.4%
% No educational qualifications		8.7%	9.5%
% Unemployed or full-time student		13.6%	12.9%
% Manual occupation		29.2%	26.7%
Ethnicity:	% White	92.7%	94.1%
	% Black	1.5%	0.9%
	% Asian	3.6%	3.4%
	% Other	2.2%	1.6%
% Current smokers		83.7%	85.1%
Mean (SD) cigarettes per day		18.5 (8.8)	17.8 (8.7)
% Smoke within 30 minutes of waking		70.5%	66.1%
Mean (SD) dependence (1–8)[b]		5.6 (0.8)	5.4 (0.8)
% Previously quit >3 months		48.7%	48.1%
% Lives with other smokers		41.2%	44.7%
Mean (SD) how much do you want to quit? (1–5)		4.4 (0.7)	4.4 (0.7)
Mean (SD) how determined are you to quit for good? (1–5)		4.3 (0.7)	4.3 (0.8)

[a]Sum computed as one point for each of the following: rented home; no car; no educational qualifications; manual occupation; unemployed or full-time student. [b]The sum of time from waking to first cigarette (as 4 = <30 minutes, 3 = 31–60 minutes, 2 = 1–2 hours, 1 = >2 hours) and a categorical scale representing number of cigarettes smoked per day (as 0 = 1–5, 1 = 6–10, 2 = 11–20, 3 = 21–30, 4 = >30). SD: standard deviation.

Table 2 Outcome at 6-month follow-up.

	Control	*Intervention*	$\chi^2_{(1)}$	*Adjusted OR[a] (95% confidence interval)*
Including all participants randomized to control or intervention groups				
n	881	877		
3 months prolonged abstinence	9.3%	9.1%	0.018, $P = 0.893$	1.02 (0.73, 1.42)
1 month prolonged abstinence	12.3%	10.8%	0.876, $P = 0.349$	1.16 (0.86, 1.56)
7-day point prevalent abstinence	13.6%	12.5%	0.449, $P = 0.503$	1.11 (0.84, 1.47)
24-hour point prevalent abstinence	14.9%	14.1%	0.189, $P = 0.664$	1.07 (0.82,1.40)
Including only those smoking at baseline				
n	737	746		
3 months prolonged abstinence	7.9%	7.5%	0.069, $P = 0.793$	1.04 (0.71, 1.53)
1 month prolonged abstinence	10.7%	9.4%	0.732, $P = 0.392$	1.15 (0.81, 1.62)
7-day point prevalent abstinence	11.9%	11.1%	0.241, $P = 0.623$	1.07 (0.77, 1.48)
24-hour point prevalent abstinence	12.9%	12.3%	0.105, $P = 0.746$	1.04 (0.76, 1.43)

[a]Adjusted for baseline characteristics: age, sex, deprivation, dependence, intention to quit, motivation to quit, previous quit >3 months, lives with other smokers. OR: odds ratio.

A *post-hoc* per protocol analysis was conducted including only those participants who reported at 6 months that they remembered receiving the report (control $n = 278$, intervention $n = 310$). In no case was there a significant difference in outcome between the intervention and control groups. The analysis was repeated including only those participants who reported at 6 months that they had read all of the report (control $n = 244$, intervention $n = 281$), and again there were no significant differences between the intervention and control groups.

Sensitivity to missing outcome assumptions

Assuming that all participants who did not provide outcome data were smoking at 6-month follow-up is an

extreme assumption, equivalent to assuming that the informative missingness OR (IMOR: [19]) is equal to zero in the control and intervention groups. To investigate the sensitivity of the results to missing outcome assumptions, the effect size for the primary outcome was estimated under the assumption that participants who did not provide outcome data had the same quit rate as those who did provide outcome data in the same trial group—within-group IMOR = 1. This is equivalent to assuming that the outcome variable is missing at random. The quit rates increased from 9.1 to 22.8% in the intervention group, and from 9.3 to 22.1% in the control group. The large jump in quit rate is due to the high rate of attrition (58.9%). The intervention effect remained stable, the OR changing from 0.98 (95% CI 0.71–1.35) to 1.04 (95% CI 0.83–1.30). Thus, the main outcome results are not sensitive to missing outcome assumptions.

Process measures

Of the follow-up respondents, 82.6% remembered receiving the report, more in the intervention group than in the control group (89.3% versus 76.2%; $\chi^2_{(1)} = 21.4$, $P < 0.01$). Of those who remembered receiving the report, 89.4% said they had read it all, but this did not differ by group. Of those abstinent at follow-up by the primary 3-month measure, 59.4% said they had quit as a result of receiving the report, more in the intervention than in the control group; (67.1% versus 49.1% $\chi^2_{(1)} = 4.37$, $P < 0.05$). Compared with control participants, intervention participants rated the report more highly on a range of evaluations (Table 3). There were no significant differences between the intervention and control groups on whether they sought advice from their general practitioner (GP), practice nurse or pharmacist, or made use of other smoking cessation products and services.

Among the intervention group, respondents to the progress assessment invitation ($n = 206$, 23.5%) were older than non-respondents (mean 40.1, SD 11.8 versus mean 37.1, SD 11.2, $t(875) = -3.41$, $P = 0.001$) and less deprived (mean 0.98, SD 0.99 versus mean 1.24, SD 1.07, $t(875) = 3.11$, $P = 0.002$). Response was unrelated to sex, dependence, intention to quit, motivation to quit, previous quit attempts and living with other smokers. On each outcome measure, those responding to the progress assessment invitation were more likely to be abstinent at 6 months than those not responding (Table 4).

DISCUSSION

A highly tailored advice report and follow-up progress report were no more effective in helping participants to quit smoking after 6 months than a standardized report that gave the same information to every participant. The absence of a difference was not sensitive to whether

Table 3 Participant evaluations of the report at 6-month follow-up.

Evaluation	*Control* *Mean (SD)*[a]	*Intervention* *Mean (SD)*[a]	
The report . . .			
Was easy to read	4.4 (0.8)	4.6 (0.7)	$P = 0.032$
Was easy to understand	4.5 (0.7)	4.6 (0.6)	$P = 0.005$
Was written for me	2.8 (1.8)	3.4 (1.1)	$P < 0.001$
Contained new information	2.6 (1.2)	2.8 (1.1)	$P = 0.033$
Was interesting	3.4 (1.2)	3.7 (1.0)	$P = 0.001$
Was useful	3.5 (1.1)	3.8 (1.1)	$P = 0.007$
Made me feel more confident about quitting	2.8 (1.2)	3.1 (1.2)	$P = 0.002$
Made me more determined to quit	3.0 (1.3)	3.2 (1.3)	$P = 0.033$
I liked the tone of the report	3.7 (0.9)	4.1 (1.0)	$P < 0.001$
I liked the appearance of the report	3.4 (1.0)	3.7 (1.0)	$P = 0.012$

[a]Numbers of participants for these comparisons range from 558 to 576. SD: standard deviation.

Table 4 Response to progress assessment invitation as predictor of abstinence among intervention group.

n	*Returned for progress report* *206*	*Did not return for progress report* *671*	$X^2_{(1)}$
3 months prolonged abstinence	22.3%	5.1%	56.7, $P < 0.001$
1 month prolonged abstinence	24.3%	6.7%	50.3, $P < 0.001$
7-day point prevalent abstinence	27.2%	8.0%	52.6, $P < 0.001$
24-hour point prevalent abstinence	29.1%	9.5%	49.8, $P < 0.001$

participants were smoking at baseline or had recently quit, or to whether participants were more or less deprived.

The content of the intervention and control reports was based on social cognitive theory [13] and the perspectives on change model [14], with the intervention reports tailored according to smoking-related beliefs, personal characteristics and smoking patterns, self-efficacy and outcome expectations, social support, pregnancy and health problems. Most trials of tailored smoking cessation interventions use standardized self-help materials or no intervention as the control comparison. The iQuit control comparison was unusual in using the same algorithm that produced the tailored reports to produce the standardized control report by entering modal values derived from a pilot study, and missing values where the algorithm permitted them. Thus, the behaviour change techniques [20] used in each condition were broadly similar (see Table S4 in Appendix S1). This provides a tough test of the tailored report. The modal report is the most appropriate control comparison for testing the efficacy of the tailoring component, as it isolates the tailoring component empirically, providing participants with the best support the system can provide if it does not have individual level information. Given that the control and tailored reports were constructed from the same database of messages, it is striking that participants evaluated the tailored report more positively, and were more likely to attribute quitting success to it.

The design does not rule out the possibility that both interventions were each, to some extent, effective in helping participants to quit, as it does not provide a comparison with a no-intervention condition. Six-month quit rates were somewhat higher than in a recent trial, in which participants were e-mailed a single tailored versus non-tailored report, where 24-hour point prevalence abstinence rates at 6 months were 4.3 and 8.9% in control and intervention groups, respectively [21]. However, participants in that trial had less immediate plans to quit on enrolment (34% within 1 month) than iQuit participants (92% within 30 days). Six-month quit rates were lower than a previous trial in which participants were mailed paper copies of a similar tailored report [4], leading to 3-month prolonged abstinence rates at 6 months of 14.5 and 13.7% in intervention and control groups, respectively. Participants in that trial were recruited from callers to the Quitline, so they may have been more ready to engage in a quit attempt, and they all received telephone counselling. The 6-month quit rates appear to compare favourably with the pooled mid-term quit rates in a recent meta-analysis of computer and web-based smoking cessation interventions [6].

The current trial used the same intervention content as Sutton & Gilbert [4], but took advantage of the extra flexibility afforded by web-based presentation; for example, the relative ease with which participants can be invited back and given feedback. However, it is possible that the web-based presentation incurs a cost in terms of lower engagement due to the lower commitment of time and effort on the part of the individual smoker to become involved. This is perhaps reflected in the somewhat high attrition. The most common reason for lack of response was that no valid telephone number was given ($n = 872$, 49.6%; Fig. 1), where the participant left the non-mandatory telephone number field blank or entered a false number. Presumably, all or nearly all of these individuals did not wish to be contacted by telephone, yet they were willing to provide personal data and receive advice online. This indicates a sizeable group of smokers seeking advice who wish to remain somewhat anonymous, or at least personally distant from their advisers. Boosting online follow-up facilities and targeting these at individuals who indicate a desire to retain personal distance could improve response rates. Small financial incentives for completion may also be effective (e.g. [22]).

We also saw low uptake of the intervention group progress assessment, consistent with previous findings. For example, in two separate trials, 27 and 20% of participants returned to a smoking cessation website after the first visit [23,24]. Uptake of the iQuit progress assessment was predicted by participants being older and less deprived, but uptake was itself a strong predictor of later success in quitting (Table 4), perhaps indicating a higher level of engagement with the quitting endeavour. It is possible that the rate of return to receive further advice would have been higher if iQuit had provided a more immersive web environment, or if it had made more extensive use of cross-modal prompts [e.g. e-mail reminders, short message service (SMS) texting]. It is worth noting that iQuit presented static materials, derived from a previous printed materials intervention [4], via a website, so that participants could print or keep a copy in their e-mail inbox. This is structurally similar to a tailored printed self-help leaflet, and is unlike the more immersive web experience to which many participants may have become accustomed. It is possible for a computer system that uses individual data to provide a more interactive interface to tailored content, and thus to provide some of the features of face-to-face contact (see [25] for a review of interactive web-based interventions).

In conclusion, a web-based intervention that tailored content according to smoking-related beliefs, personal characteristics and smoking patterns, self-efficacy and outcome expectations was not more effective than web-based materials presenting broadly similar non-tailored information. Participants evaluated the tailored report more positively than the standardized report, and felt more strongly that it had helped them to quit. Tailoring was popular but not effective. Attrition was high, and a

particular challenge for those designing web-based interventions is how to keep individuals engaged after their first visit.

Clinical trial registration

Not applicable.

Declarations of interest

None.

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Supporting information

Additional Supporting Information may be found in the online version of this article:

Appendix S1 Effectiveness of web-based tailored smoking cessation advice reports (iQuit): a randomized trial.